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BEING A

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SATURDAY, APRIL 7, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Part I.—Lecture XXVI.

Treatment of Intermittent Fever.

The means which we employ, gentlemen, for the purpose of curing intermittent fever, may be divided into those which are had recourse to in the intermission of the disease, and those which are employed during the paroxysm. With regard to the latter, the greater number of them are adopted rather with a view to the temporary alleviation, than to the cure of the affection; and I need not say that they must be very different according to the stage in which they are employed.

Treatment during the Paroxysm.

Cold Stage, Hot Air.—In the cold stage common sense would point out that there should be plenty of covering, and warm drinks, and some have recourse to the warm bath: it would, however, I conceive, be better to employ dry heat; for warm air may be easily and instantly conveyed to a patient when in bed, by having something under the clothes to raise them, and a tube continued from above a a spirit lamp. Thus you surround the patient with hot air, and you may convey it in any quantity you please, and of any temperature. Something like an inverted funnel is placed near the bed, with a spirit lamp under it; a tube goes from the extremity of this funnel-shaped body, and is conveyed under the bed clothes, so that the air is dispersed throughout the bed. The degree of heat may be regulated by altering the proximity of the lamp to the funnel. I think if the cholera make its appearance amongst us, this will be found by far the best mode of applying heat *externally*; and on this account particularly, that when you employ vapour, the patient for the most part must be in a sitting posture, or at any rate taken out of bed, and more or less exposed after the bath; but it is a material thing to avoid all this in extreme exhaustion, and to keep the person horizontal; and by this contrivance you need not take him out of bed, nor remove him from the horizontal posture. Heat may thus be employed to a sufficient extent thoroughly to warm the surface, and friction especially to the extremities, as grooms rub shivering horses, chiefly on the legs, and even the ears, may be had recourse to at the same time. If it be right to apply heat during the intensity of the cold stage of intermittent fever, I imagine that this contrivance will be found much better than either the hot water or the vapour bath.

Warm Diluents.—Warm liquids, of course, appear indicated, but you should be on your guard against administering real stimulants. It is very well to apply caloric by means of hot liquids, but by giving stimuli, such as brandy and wine, you incur the risk of doing more than you intended—of increasing the subsequent hot stage, of inducing delirium, and causing congestion and inflammation of the head and internal organs. I should rather therefore give hot diluents, or at the utmost hot spiced diluents, than alcohol, or things which are in themselves real and permanent stimulants.

Venesection.—A remedy has lately been recommended which one could not *à priori* imagine to be proper, but in favour of which there is very strong testimony. I allude to the abstraction of blood. The best information you can have upon this subject is to be found in Dr. Mackintosh's Practice of Physic. I believe he is the gentleman who first advised and put in practice this mode of treatment. You are aware that in the cold stage of fever the circulation is greatly deranged, the internal parts are in a state of

congestion, the head, the lungs, the heart, and all the organs whose veins go to form the vena portæ, as well as that vein itself, and its divisions themselves, are in a state of extreme turgescence, and by removing a certain quantity of blood, you unquestionably diminish this load of blood, you give ease to nature, and it is found that the loss even of a small quantity of blood, relieves more or less the general uneasiness of the patient. In hot countries, where they have what are called *pernicious fevers*, I mentioned that it is very common for those fevers to be attended by such congestion as we rarely see in this country—congestion which speedily proves fatal, and it must therefore be a very great point to bleed as early as possible. Dr. Mackintosh mentions that the cold stage is shortened by this practice; that the hot stage is either prevented or diminished in violence and duration; and that many cases are entirely cured. You will find a report on this subject in the Edinb. Med. Journal for 1829, by Dr. Stokes, which is not altogether favourable. He says venesection is more or less useless, and more or less injurious, in different respects. He says it is beneficial in removing the local symptoms—that is just what one would imagine—in removing the symptoms of congestion about the chest and abdomen; but, he says, he was always compelled to exhibit quinine sooner or later: and he adds as a reason for limiting the performance of venesection to those cases only where there is coma or inflammation, that a friend has informed him of some patients so treated who never recovered. There can be no doubt, I imagine, that the practice is not so injurious as one would imagine before hand, perhaps, but that on the contrary, it frequently does a great deal of good; it removes the local symptoms, it frequently shortens the complaint, and some times cures it. As far as I can understand it, it is rather a means of alleviation for the most part than of cure, but I have no doubt that it is imperiously necessary in many cases abroad where the congestion is extreme. I never myself have had recourse to it, and I therefore cannot speak except as a matter of opinion. I put the testimony of others together and infer for myself, but I have no facts to give you. I have never seen a case of ague which I thought required such treatment. I have never seen a case which I could not cure by the sulphate of quinine, and knowing this, and not seeing any violent local symptoms during the cold stage, to make such treatment necessary, of course I have never employed it.

I may mention, speaking of venesection in general, and not of the cold stage in particular, that Dr. Lind says that he and two others had each three patients with ague: each bled his three patients, and each lost one. This, however, is saying very little, unless we knew the particulars of the cases. Dr. Mackintosh says that it is always safe, often shortens, and sometimes cures; but as I find that I can always remedy the affection by the sulphate of quinine in some quantity or other, I have not thought it at all right to take away blood, and thus impair the powers of the patient. There can be no doubt, however, that it is a remedy to which it is our duty to have recourse, if we find great symptoms of congestion, and if we fear that the next attack will prove fatal.

Opium.—Opium is found both to shorten and to alleviate this stage. I myself have given it with very great success. The best authors will tell you that a full dose of opium certainly shortens this stage, and always alleviates it; but still, if I found great congestion of the head, or of other parts, I certainly would order bleeding in preference to opium.

Mechanical Means.—A tourniquet has been applied to an extremity, for the purpose of producing early excitement. One would conceive that internal congestion must be increased by this means. If you compress the femoral or brachial artery, there must be a larger quantity of blood thrown upon other parts, and if such a remedy does good, it must be by producing such a load as will stir up nature to attempt its removal, so that the body will be hurried out of the cold stage more quickly than it otherwise would have been.

Hot Stage.—However, when the hot stage comes, we have to give cool drinks, to take off some of the clothes, and to employ, if it be necessary and advisable, the tepid bath or warm affusion, or warm ablution. There would, I should think, be no harm in cold affusion or cold ablution; neither should I hesitate to bleed in the hot stage, if there were any congestion, or any inflammation. A cure has now and then been effected in the hot stage by a person jumping into a pond, which is a cold bath; but I believe this is rather a dangerous practice. Opium in this stage has likewise been found of use. Dr. Lind says that he found opium useful in the hot stage, both in shortening its duration and mitigating its violence. Hence it would appear that venesection is admissible in the cold stage as well as in the hot, and that opium does good in both stages.

Sweating Stage.—When the sweating stage has arrived, all is going on well, and the paroxysm is nearly at an end, and you then (if the patient chooses), give him warm diluents, and put on more clothes than in the hot stage, because he is cooling gradually. You must not allow a draught to blow upon him, but certainly you may attempt to lessen the duration of the sweating stage, which will sometimes run on for many hours, by diminishing the quantity of clothes and cooling him gradually;

but you must be very cautious. In the hot stage there is no danger, I presume, from tepid ablution, but in the sweating stage we must be more on our guard. In this stage narcotics, and every thing that debilitates, would be wrong. Nature is now exhausted, and she wants repose, and to lower her still more must be not only injurious but unnecessary, because health is about to be restored, and things are now nearly at a conclusion. If the patient be exceedingly faint, stimulants may be given, but we must take care to observe whether local congestion or inflammation is present before we exhibit them.

Treatment during the Intermission.

The great means, however, for curing the disease, are certainly to be employed, (unless venesection be considered a very important means) in the intermission. By the term *intermission* we mean the period between the paroxysms; but *by interval* we mean the period of the paroxysm and the period of intermission together.

Bark.—The two chief remedies certainly in the cure of this disease are bark and arsenic. Nothing is more instructive in pointing out the impropriety of hastily deciding against a medicine than the history of bark. Peruvian bark was first brought from South America in the year 1632, but the mode in which its efficacy was discovered has not been well ascertained; so highly, however, was it thought of at first, that in the year 1658, sixty florins were given for twenty doses. It was then made a nostrum of, as all good medicines are, till at length many regular practitioners wrote against it, and such was the prejudice excited, that about 30 years afterwards, several hundred pounds weight of it lay at Piura, unable to find a purchaser. An alderman died while he was using it to cure his ague, and therefore it was immediately asserted that he was killed by it. Oliver Cromwell, who, as I before stated, had an ague, was not allowed to take bark, and consequently died of the affection. The prejudice against it continued to extend, and great violence was manifested. Sydenham however, though at first opposed to it, gave it boldly, and warmly advocated its employment. Violent medical altercations took place, far greater than any which now occur, and those who employed it were called "murderers," unfit to practise. Sydenham gave it in larger doses than his cotemporaries, and exhibited it not during the paroxysm, but in the intermission. Now there can be no doubt that it may do harm during the paroxysm; it may overload the stomach and increase congestion, if it be given in large quantities; Sydenham therefore introduced a great improvement, by giving it in the intermission, and he improved its efficacy by giving it in large doses. The efficacy of many medicines is not properly esteemed, in consequence of their not being given in sufficiently large doses. Many persons will exhibit a medicine, and if it does not appear to answer, immediately they relinquish it, without finding whether it will really do good or not. Sydenham, by giving larger doses than his cotemporaries, by exhibiting it between the paroxysms, and by gradually increasing the dose, found that it deserved all the praise which had been bestowed upon it.

When it does not cure the disease as rapidly as you wish, it should be given in the largest dose that the patient will bear. A drachm or two drachms every two hours, is frequently necessary to cure the affection. Dr. Gregory used to tell us that he was informed by a practitioner, that a friend of his, in the delirium of ague, took from three to four ounces of bark, and not only no ill effect ensued, but he was cured from that moment. It ought to be given finely powdered, because if it be not, a very small quantity only will come in contact with the stomach and intestines, and its virtue is lost.

Of course you know that there are three kinds of Peruvian bark—the pale, the yellow, and the red. The latter is preferred by many as being the most powerful; but although it is the most powerful, yet it is not so well borne by the stomach. Dr. Chapman, of New York, Dr. Sanders, who practised at Guy's Hospital, Dr. Rigby, of Norwich, and Dr. Skeete, all wrote on the red bark, and extolled it as being more efficacious than the others. Milk is one of the best vehicles in which bark can be given. Some give the powder in the decoction, and some give it as an electuary, and it may be given with any syrup or sweet substance mixed thoroughly, and then diffused in some liquid, so as to be drank. Sometimes it disagrees with the stomach, and therefore it is necessary to add something to it. Many persons from pure bark experience nausea, and perhaps vomiting—in some it occasions purging; and in all such cases a few drops of tincture of opium will frequently enable the stomach and intestines to bear it. If it be only the stomach which is disturbed, an effervescing draught will answer the purpose, and so will prussic acid. A small quantity of prussic acid given at intervals in the course of the day, will enable the stomach to bear bark, and many other things, very well. But if the intestines be deranged at the same time, opium is probably one of the best things, because neither an effervescing draught, nor prussic acid, will lessen the purging. You may sometimes bring a patient to bear bark by degrees, giving him small doses and increasing them. In the case of children, it may be given in the form of clysters, and some persons have been cured, it is said, by its external application, by having it tied in fine

muslin, or fine linen, on different parts of the body. I recollect hearing Sir Henry Halford say, that when he was a child he had ague, of which he was cured by wearing a jacket of bark. A double jacket was filled with powdered bark, and put next his skin.

Quinine.—However, it is now ascertained that the power of Peruvian bark resides in two substances, called *quinine* and *cinchonine*, and of these two the former is chiefly used, and that in the form of sulphate. It is found by chemists that both these substances abound most in the red bark, and we therefore see how it is that the red bark was supposed by many to be the most efficacious of the three kinds. Quinine is said to abound most in the yellow bark, the cinchona cardifolia, and the cinchonine in the pale bark, the cinchona lancifolia. I have myself given the quinine, simple, and in the form of sulphate, and cured ague with it very well; but I have never employed the cinchonine.

The mode of giving the quinine is usually, as I have just said, in the form of sulphate: some grains every eight hours will here generally cure the complaint. Some give a grain or two every two hours, and say it answers very well, but others give a larger quantity at more distant intervals. Either way is very good; but if the stomach reject it, certainly it would be better to give only a grain or two every two hours throughout the twenty-four, rather than a large quantity at once. I have found it the most efficacious mode to give a large dose immediately after the paroxysm. It is very good to give it before the paroxysm, but I think of the two, more effect is produced by giving it as soon as the paroxysm is over. You, for the most part, will put a stop to it directly, if you give ten or twelve grains as soon as the paroxysm is over, but you do not usually cure the disease, you only suspend it. To effect a cure, you must give a larger quantity generally in the course of the 24 hours, and for some time. When we first had sulphate of quinine, I was generally able to cure the disease with two or three grains, two or three times a-day, but that does not generally happen to me now, and therefore I cannot but believe that it is a medicine which is exceedingly adulterated. I continually have to give people twenty or thirty grains in the twenty-four hours, before I can cure the complaint.

No general rule can be laid down respecting the quantity required. Some persons may be cured by a few grains, and some will require a very large quantity. I had a patient labouring under quartan ague, in February 1829, which did not yield to less than forty-five grains in the twenty-four hours. I thought that a very considerable quantity, but in the ensuing October, on my return from the Continent, I found a patient in my wards who was taking, by direction of Dr. Roots, a scruple every eight hours, together with ten minims of liq. arsenicalis. This man also had a quartan ague, and he was no better, notwithstanding he took this large quantity; however, as it did not disagree with him, and as I never failed in curing the complaint, I determined that if it could be cured it should, and I gave, as Dr. Roots would no doubt have done, had he continued to treat the case, the remedy every six hours instead of eight, and he was then cured directly. That was the largest quantity that I have ever been obliged to give, but it was indispensible in this case. I should certainly advise you to give a large dose, such as twelve or fifteen grains, in some cases, but eight or ten, in general, immediately after the fit, to *suspend the disease* at once, and then to give a small quantity at intervals during the twenty-four hours, for the purpose of *curing* the disease. In the first case you knock it down at once, which is a material point, because every paroxysm shatters the patient, and is a period of great suffering, and therefore I would stop it immediately; but you will not always stop it *permanently*, unless you give a larger quantity in the twenty-four hours, and continue it for some time. I am in the habit of giving ten grains as a medium dose, immediately after the paroxysm, and then five grains every six or eight hours, whether the disease comes back or not. It is necessary to continue the medicine for some time after the disease appears to be cured; for if it be left off directly, the disease, in all probability, will return. It is just the same in the cure of syphilis; if you leave off mercury as soon as the sore is healed, or the pain has ceased, the symptoms will possibly return. Although ague, therefore, appears to be quite arrested, it is necessary to continue the remedy for two or three weeks afterwards, in order to make sure of your cure.

The remedy, however, I know, may be given with perfect safety during the paroxysm, when the pyrexia is all present, and it may be exhibited even if there be local inflammation; but I should conceive that it is a waste of a good thing to give it during the paroxysm. At that period the system is in such an unnatural state, that all agents are resisted, and a dose of quinine, that produces a good effect during the intermission, will produce no effect during the paroxysm. Medicines are all resisted more or less when we are in an unnatural state. It is found to be more efficacious during the intermission, and sufficient may then be given to cure the disease. I do not know that it is injurious to give it during the paroxysm, but certainly it is rather wasteful.

I have frequently had occasion to give it, notwithstanding there was local inflammation. My object was to cure both diseases; and while I was curing the local affection

by bleeding and other means, I stopped the ague with the quinine; and I never saw any harm done by it. When I first employed this remedy many years ago, I had a patient with ague, who shivered every other day, and then he had intense heat and sweating, but during the intermission he was always hot, always had a very quick pulse, and was always thirsty. He had continued fever, with the addition of a tertian ague, or he would be said by some to have remittent fever. I did not then know a great deal about the remedy, but I gave it him at regular hours, day and night, and he became perfectly cured. Notwithstanding the great feverishness, no inconvenience arose. The case is published, along with several others, of which I made a report to the profession, in the 12th volume of the Med.-Chirurg. Transactions, before the remedy was much known in this country. I have continually seen quinine cure the disease when bark was given in as large a quantity as could be borne, and likewise arsenic, and both had failed. I have also known it stop vomiting. When a person has been exceedingly troubled with vomiting, with great irritability of stomach in ague, I have known it put a stop to the irritability as well as to the ague. Still it is to be remembered, that now and then it disagrees with the stomach, now and then it produces vomiting and gastrodynia, just as bark will do, and more frequently still, it will also purge. When you find the stomach is disordered, whether it be thrown into spasmodic pain by it, or vomiting is occasioned, you may give aromatics, or opium, or, what is far the best, prussic acid. If there be mere pain, an effervescing draught will not be of any use, though it might be serviceable if there were nausea and vomiting.

In one respect it is best given in a fluid state, and there should be a slight addition of sulphuric acid, by means of which you make a super-sulphate. Sulphate of quinine is not very soluble, but by adding one drop of sulphuric acid to every grain of sulphate of quinine, you have a super-sulphate which is sufficiently soluble. It goes farther, I should imagine, when given in this form, but on account of its taste, and for the comfort of the patient, we often give it in a solid form. If it be exhibited in a pill, its bitterness is not tasted.

It is said to have failed sometimes, and bark has been said then to have cured the affection, just as in many, many cases, I have seen it successful, when bark had failed. I imagine that when this has happened, it has been from its disagreeing with the stomach, so that a sufficient quantity could not be taken. Cases have occurred to me in which the stomach became so irritated, from the intensity of the medicine, that the patient rejected it, and could not take it in sufficient quantity; whereas bark, being far less intense, could be borne, and was taken in such a quantity as cured the disease. I had a case exactly of this description, in the lady who caught an ague from walking on the ramparts of the Tower. A very small dose of sulphate of quinine produced most excruciating pain at the stomach, and she could not persevere with the medicine; bark was substituted for it, was borne very well, and cured the disease. Whether if she had taken tincture of opium, or prussic acid, in proper quantity, it could then have been borne, I do not know, but I think it probable that it might, and that it would then have cured her. I think Andral mentions the circumstance of quinine having failed, and bark subsequently curing the disease. I do not know the particulars of his cases, but I imagine that such a circumstance as this could only arise from the medicine irritating the stomach, so that it was not taken in a sufficiently small quantity, because it has all the virtue of the bark, and in far greater intensity.

The power of quinine is one of the most extraordinary facts in medicine, and always reminds me of the lines in Virgil—

> Hæc certamina tanta,
> Pulveris exigui jactu, compressa quiescunt.

Salacina, &c.—Many other barks, however, besides this, cure the disease. The bark of willow, and the alcaloid principle of the willow-bark—salacina, and swietenia, and all astringents of the vegetable kingdom, and more especially a combination of both, such as a combination of columbo and galls, will cure the affection. A combination of these is found to be more powerful than either of them taken separately, and those barks which contain both a natural bitter and astringent, are those which answer best. It is said with respect to these, that they have sometimes cured the disease after Peruvian bark has failed, and various other combinations of bitters and astringents, have done the same. Piperine will cure ague.

Arsenic.—However, next to sulphate of quinine or bark, certainly comes arsenic. The solution of arsenite of potassa, is what is commonly used, in doses of from two to ten, or twelve drops or more, two or three times a day. It is best to give this remedy in a very gradual mode, and I never begin with more than two or three drops, three times a day, gradually increasing it. It is always best not to give this medicine on an empty stomach. You find it by far the best practice, when you are giving an acrid matter, such as iodine, the oxymuriate of mercury, or antimony, with a view not to nauseate but merely to sweat, to exhibit it after food has been taken. It is evident that any thing acrid coming in contact with the mucous membrane of the stomach, is more likely to irritate it, than if food had been first taken,

so that it is applied partially to the stomach and gradually. Whenever Peruvian bark or sulphate of quinine irritates the stomach, it is best to try whether it will not be borne by giving it after meals. But in the case of arsenic, you should always make it a rule never to give it before breakfast. Persons are continually nauseated, and they will even vomit, by taking it before breakfast; whereas, after breakfast they may bear it very well. When it produces nausea or vomiting, a little tincture of opium is indispensable; indeed it is well in general to conbine a little tincture of opium with every dose of liquor arsenicalis.

The ill effects of arsenic are symptoms of gastritis. The slightest symptom is nausea, the next vomiting, then pain of the stomach, and heat there, tenderness on pressure, and heat ascending up the throat. Arsenic has a tendency to excite gastritis, even when not taken into the stomach. I knew an instance of a person being seized with gastritis, and thrown into the greatest danger, simply from having arsenic applied to a sore of the leg. One of the first effects of arsenic is to irritate the stomach, and the irritation soon arises to the pitch of inflammation; but it will also produce other effects. Singularly enough, it occasions soreness and redness of the tarsi, redness and heat of the fingers, soreness of the throat, and œdema of some part of the body. Very frequently when patients have been taking arsenic, I have seen the face swell, sometimes the lower extremities; sometimes one part and sometimes another, but more frequently the face. Frequently too there is heat of the system. When this local inflammation of the stomach, throat, or face, takes place, the whole body will become more or less excited, so that you have general pyrexia. In this case it is immediately necessary to omit the medicine, not to lower the dose, but omit the medicine altogether, and then you generally see the symptoms decline; but frequently it is necessary to apply leeches freely at the pit of the stomach, or to bleed at the time. I never but once saw any harm ensue, and in that case the remedy had been carried on very injudiciously. If while you are giving arsenic you look carefully at the patient's face, and ask about his throat, and also respecting the state of the stomach, as to whether there is pain, tenderness, nausea, heat, or vomiting, I believe no harm can arise.

Arsenic will sometimes cure the disease immediately. I have seen ague cease from the first day it was given, although the disease had continued for a length of time; but frequently a longer period is required for its successful exhibition. I need not say, that on account of its virtues in this disease, it has been made a nostrum of, exactly as was the case with bark. What is called "Tasteless Ague Drop," is a preparation of arsenic. The largest dose required, I believe, is from twelve to fifteen drops. (I never had occasion to go beyond that, nor should I like to do so), three times a-day, or as frequently as is necessary.

It is asserted, but I cannot say, from experience whether it is correct, that when liq. arsenicalis has failed, the pure arsenic has succeeded; that is to say, what is called arsenic in commerce, has succeeded, when its combination with potassa, according to the directions in the Pharmacopœia, has failed. It is said that about the sixteenth of a grain may be given, three or four times a day, that the eighth of a grain can seldom be borne, but that the sixteenth of a grain succeeds very well. You will find this stated by Dr. Macculloch, in his second volume on Marsh Fever, but I never exhibited it.

Zinc, Copper, &c.—Many other minerals, which are astringents, will cure cases of this disease; sulphate of zinc, oxyde of zinc, sulphate of copper, and alum. Muriate of ammonia is likewise said to have a power over the disease, and it is thought by many, at least abroad, that it heightens the power of bark. Carbonate of potass is supposed to possess some virtue. A combination of some of these things is thought to be best, but no doubt sulphate of zinc, and sulphate of copper, will frequently cure the disease.

Spiders, Cobwebs, &c.—As a full dose of sulphate of quinine, or a full dose of bark, is found more useful immediately before the paroxysm, when it is just about to begin, than at other periods in the intermission, excepting immediately after it is over, so various other remedies have been employed at this period. These have been of various kinds, sometimes medicinal, and sometimes articles, or procedures, calculated only to make an impression on the mind. Immediately before the beginning of a paroxysm, a large dose of opium has sometimes prevented it from coming on violently. Sudorifics and emetics have been employed for the same purpose. Mental emotion has been excited at the moment with a similar view, so that a child has been flogged out of his ague, or frightened out of it by threats. He has been threatened with a good thrashing if his ague came on, and there can be no doubt that this has cured his complaint. Sometimes by exciting disgust, by insisting that a person should swallow spiders and cobwebs, the paroxysm has been prevented. Some persons, however, imagine that the latter really possess a power in preventing the disease. I have no personal knowledge of the virtue of spiders and cobwebs, but you will find in Dr. Chapman's work on Materia Medica, a very strong testimony in favour of the soothing effects of cobweb. He mentions that it will procure sleep in fever, and tranquillize the system,

when all other things have failed. I know that many persons declare that it has a sort of sedative quality, and there is no reason why it should not; but in the cure of ague, it has been employed merely with a view of exciting disgust, and it has succeeded. These effects might be produced at any time during the intermission, but it is immediately before the paroxysm that disgust, or fright, or violent emotion of mind, is most likely to be of use.

The great remedies, however, for the disease, are quinine and arsenic, all other means being more or less uncertain, except perhaps salacine. I never now think of employing any thing but quinine: under particular circumstances, however, I might be induced to order venesection.

These two medicines, quinine and arsenic, are also equally good in other complaints of an intermittent, though not aguish, character. In intermittent rheumatism and neuralgia, they are among the best remedies. It is a singular circumstance, but even in intermittent vomiting, arsenic has been known to effect a cure. In the last volume but one of the Transactions of the London Medical Society, you will find a paper by Dr. Adams, on periodical vomiting, coming on at certain intervals, and which was cured by what would in many cases have produced vomiting, a small quantity of arsenic. Whenever complaints assume an intermittent form, whether they be aguish or not, you will find that the sulphate of quinine and arsenic, are among the best remedies. If either of these failed, I should not hesitate to give them both together. I have failed with arsenic, but not with quinine: whenever a case was obstinate, I increased the dose to the largest quantity the stomach would bear.

Propriety of stopping Ague.

It was formerly imagined that ague was too good a thing to be cured—that it was very wrong to stop it; at any rate many contended that it ought not to be stopped till the patient had gone through a certain preparation. I know that I have astonished many persons, (who, when abroad, were accustomed to see some preparation employed before the remedies for ague were given), by stopping the disease immediately. I never saw any harm done by it; though if there be any local affection of the head, chest, or abdomen, you must take care to attend to that at the same time. If there be any congestion of the head, lungs, or abdomen, it may be necessary to bleed, to purge, and to use all the remedies for such a state as this; for if you do not, it is possible that the circumstance of stopping the ague at once, may be useless. If you do every thing indicated by the local affection, I am quite satisfied there will be no danger in stopping the ague. I never did harm by it in a single case; I never lost a patient from ague, notwithstanding that I gave sulphate of quinine, when local inflammation was present. Arsenic, I need not say, may be very improper when the inflammation which is present affects the stomach, and in the case of gastritis, possibly neither arsenic nor quinine can be borne, and you do not remedy the morbid condition till you adopt either local or general bleeding.

So far from the stoppage of the ague doing any harm, I have found any local disease that might be present, removed the more easily; for every attack of ague of course disturbs the circulation, renders it more irregular, and is likely to throw a greater load of blood upon those organs which are in a state of congestion; at any rate, ague always makes bad worse. I have always considered that I have treated local complaints more successfully, by having stopped the ague. Sydenham says, cure the ague first, and then you will easily cure the dropsy, which otherwise might be impossible. I never saw any chronic complaints ensue on stopping ague. I believe they do not arise from the ague itself, but are, like ague itself, the result of malaria; and I know that for many of them, one of the best remedies is sulphate of quinine, bark, or arsenic. I have seen many cases of dropsy vanish on the exhibition of sulphate of quinine, without any thing being conjoined with it. You will find all those effects which are ascribed to ague, the more safely cured, if you stop the ague, whether they be acute or chronic. Still it is possible that sometimes ague will not give way till you have remedied the local mischief. It is said that now and then ague will not yield to any thing till you make use of local bleeding. You will find many say that they have met with cases where bark would not cure the disease, till they made the patient's mouth sore. I never met with a case of that description, but still it is so asserted, and it is right that you should know it. If what is said be correct, such a practice must be adopted, but I should nevertheless, in such a case, go on with the quinine while I was giving the mercury, or making use of local means.

Prophylactica.

In regard to the prevention of ague, the great point is to drain the country, to prevent all accumulations of dead vegetable substance, in order to prevent, as much as possible, the exhalations from putrefied vegetable matter. When a part cannot be drained, occasional inundations may be necessary, in order to throw more water upon it than is compatible with vegetable decomposition.

With regard to individuals who must be exposed to ague, the best mode of prevent-

ing it is to live as well as possible, to have good food, a certain portion of wine or beer, and never to expose themselves to malaria on empty stomachs. Persons so situated, ought always to take something before they go out of doors, and where malaria is very intense, spirits may be found necessary. Smoking is an excellent preventive in damp places. I mentioned that when Napoleon was in Italy, he found the use of keeping up good fires in preventing disease. When persons are obliged to live in a house where there is malaria around, it is best to sleep as high as possible, rather to condescend to sleep in a garret, than to aspire at sleeping in the best apartments; and it is of great importance to avoid going out at night, or very early in the morning. It is said that the application of a gauze veil around the head, when persons must be exposed, is likewise of use. Besides, if a person must be exposed to malaria, it may be highly beneficial to take sulphate of quinine.

At the next lecture I shall proceed to the consideration of Continued Fever.

CLINICAL LECTURE

ON

DEFICIENCY OF THE PARIETES OF THE ABDOMEN AND ANTERIOR WALL OF THE BLADDER,

Delivered at St. Bartholomew's Hospital,

By Mr. Earle.

Mr. Earle drew the attention of the class to the case of a young female, æt. 15, in Sitwell's ward, who presented a curious specimen of deficient formation of the bladder. She had been under his care in this hospital five years ago, when he had constructed for her an instrument which materially contributed to her comfort, and it was for the purpose of procuring another, of the same description, (the former having been stolen from her) that she again applied to him.

Upon examination, the following state of things is observed. A deficiency of a large portion of the lower part of the parietes of the abdomen, as also of the anterior wall of the bladder, so that the mucous surface of its posterior wall protrudes forth at the opening in the abdomen. It is a red, vascular, sensitive, pulpy surface, defended by a large secretion of mucus, and which has very much increased in size since her former sojourn in the hospital. At its lower part the enlarged ureters are seen to open, from which a constant distillation of urine is taking place, although she has sometimes the power of retaining for a short time a few drops in the lower ends of these canals, and afterwards ejecting them in a jet. At the upper portion may be observed some cicatrices, the result of some attempts to destroy the sensible mucous surface which had been formerly made in that situation. Half an inch below the protrusion, there are two minute spots, (exuding a milky secretion) into which a small probe can be passed, and which no doubt lead to the vagina. These openings did not exist when she was before in the hospital, or if they did, they escaped observation, which latter circumstance is not at all probable, as she underwent repeated careful examination, and, indeed, during her stay this time, the central band separating these orifices has been observed to become very much more narrow. This is a very curious fact, for the development of this part would seem to be cotemporary with the approach to puberty, which is now taking place, the uterus also being fully developed, as ascertained by examination by the rectum. There is a portion of skin analogous to a perineum, and above a bifid clitoris, and the rudiments of the nymphæ may be observed. The labia are much separated, flattened, and broader, converging as they descend. A great interval prevails between the ossa pubis, and the recti muscles are widely separated above the protrusion, this space being filled by a firm tendinous expansion and the common integuments. So great a separation of the pubes of course removes a great portion of the bony support; the pelvis is widened, and her gait becomes what is called *waddling*, in a great degree. At present the ligamentous connexion between the bones is much firmer, but as large a space as ever exists between them. There is no umbilicus; probably the vessels entered just above the protrusion.

From the consideration of this and other similar cases, we observe that the bladder is not an organ essential to life, although eminently conducive to the comfort and convenience of the individual.

These cases afford us an excellent opportunity of ascertaining the structure and vital properties of mucous membranes, and of exhibiting the fallacy of those arguments which would in many respects identify them with common integument. In the present instance it has remained from birth quite unchanged in its appearance, although constantly exposed to the influence of the atmosphere. It also resisted, in a wonderful manner, when she was formerly in the hospital, the attempts which were made to destroy it, for the purpose of effecting cicatrization, even by the strongest escharotics, being so very speedily reproduced. Haller, and many physiologists since him, have remarked the great analogy prevailing between the common integument and mucous membrane, and indeed some proceed so far as to

consider them as mere modifications*, and that in the mucous membrane we have a minute epidermis existing, and also that mucous membrane, upon long exposure, will take on the appearance of common integument. The present case may be said to present a refutation of such opinions. Although the extremity of the rectum, the vagina, the mouth, or even the œsophagus, are lined by a *prolongation* of *skin*, which may very readily become altered by exposure, yet a *mucous* surface cannot be converted into skin. The effects of inflammation have given rise to the formation of pseudo-membranes, and these have been the cause of much illusive reasoning. In the present case the characters of the mucous membrane have been retained, and on attempting, we could not even compel it to take on those of skin. Thus also we see in a case of vesico-vaginal fistula a marked difference between the two exposed surfaces of the vagina and the bladder, although they may be contiguous, the one being very slightly, the other highly vascular. We may have an artificial anus for years, and yet no such alteration take place in the mucous membrane. So when the mucous surface of the male urethra is exposed, its sensibility will continue until by some means we destroy such surface.

This case, moreover, affords a good opportunity of putting into practice experiments on the nature of the urine—experiments which, as far as the individual herself is concerned, are perfectly innocent. When she was in the hospital before, Mr. Earle performed a series of such, in order to ascertain the rapidity with which certain substances might be detected in the secretion of the urine after they had been taken by the mouth. Various substances, such as rhubarb, turpentine, asparagus, were administered, but, unfortunately, Mr. E. had mislaid the memorandum containing a detail of the results. He, however, perfectly recollected that the shortest time in which the peculiar violet odour could be detected in the urine, after turpentine was swallowed, was four minutes and a half; the odour peculiar to asparagus in eight minutes: a most extraordinary result, when we take into consideration that the substance must have undergone the process of digestion and absorption, and traversed the round of the circulation, previously to being eliminated by the kidney. It was Mr. E.'s intention to repeat some of these experiments, and try others, relative to the power of medicines in producing an alkaline or acid state of the secretion; an important investigation, as determining how far we can call in chemistry to the aid of medicine in the treatment of certain classes of diseases. This case he considered favourable for trying such, as, in the ordinary mode of doing so, the secretions of the kidney often undergo some changes in the bladder, especially if this viscus be diseased in any way.

This may certainly be termed a very rare case, if we regard the *sex* of the individual; for while there are but seven or eight recorded cases of such malformation in the female, there are at least sixty related of its occurrence in the male.

Among the instances recorded of its occurrence in females, there is one published in the Philosophical Transactions, in which the woman became impregnated and bore a child. In another, reported by Dr. Neville, the umbilicus was natural. Another very interesting case is also recorded by Mr. Coates, in the Edinburgh Medical and Physical Journal.

There are certain characters common to all these cases occurring in either sex, while there are others peculiar to the sex, but which have not reference to the urinary, but to the genital system.

In both cases we have a want of bony union between the ossa pubis, which are, in different cases, more or less separated. This might have been considered as the primary defect upon which all the others hinge; but that in the case related by Mr. Coates, union did exist between the two bones—the urethra passed over the pubes, and was pervious to the extent of half an inch, and then terminated in a cul de sac. Moreover, we have in all cases an enlargement and a greater contractile power of the ureters—an absence of the urethra, as also of the umbilicus. In all these circumstances, then, both sexes accord. There are, however, some points of difference. In the *female*, the uterus, ovaries, and vagina, all exist, although this last may be closed. In the present instance, no opening existed formerly, but one is now taking place; and the same would very probably have occurred with respect to the other cases, had they lived to the period of puberty: and, indeed, such a closure as the present need not operate as an obstacle to impregnation, since we see it take place in cases of imperforate hymen which have required liberation by a surgical operation; and in these other cases, if the organs were in a fit state, there could be no objection to making an opening into the vagina. Thus, then, the female has the power of continuing her species, which the male does not possess. In one recorded case, the female bore a child; and there can be little doubt that it would be possible for

* There is a remarkable analogy between the skin and mucous membranes. The latter may be viewed as prolongations of the skin over internal surfaces, modified only to suit the difference of place; or the skin may be said to contain the elements of the mucous tubes, but more firmly and closely wrought, and protected by the cuticle, as the latter are protected by the mucus they secrete."—*Mayo's Physiology.*

this young person to do the same. In the *male*, the penis is invariably short; it may extend an inch in length, being generally broad at the root, and then becoming bifid, there is no prepuce or urethra. In some instances there is an open groove for some distance, with a vascular mucous surface—sometimes a mere slit; at its base is usually the fossa navicularis and the caput gallinaginis. The testes are natural and usually much separated. Indeed, the scrotum being completely divided and separated, has very much the appearance of labia, which (conjoined to the diminutive penis, being mistaken for an enlarged clitoris) has given rise to the supposition that these individuals are hermaphrodites. The vas deferens terminates variously—in the ureter, perineum, or even rectum. The venereal passion exists, but the individual has not the power of gratifying it. The anus, in the male, is usually perfect, but placed more forward than natural; though Bertolinus relates a case in which the patient vomited all his fæces for forty years. Littré also relates a case in which there was neither cæcum, colon, or rectum, the ilium terminating in a cul de sac.

Dr. Duncan has framed an hypothesis respecting these cases, founded on the supposition of the primary cause being an impervious state of the urethra.

"As soon as the urine begins to be secreted, it will accumulate in the bladder, and distend it, as well as the ureters and kidneys. In the adult, ischuria proves fatal in a short time, both from the rigidity of the containing parts preventing them from yielding to the distending pressure, and from the reabsorption of an excrementitious matter highly deleterious to animal life. In the early fœtus, however, circumstances are extremely different; in it the urine can contain very little, if any, excrementitious matter, and the whole containing parts are soft and plastic, the bones scarcely cartilaginous, and no where knit together. The bones of the pubes, therefore, yield to the distending pressure, and are separated gradually from each other, until they become so firm that it has no longer any effect upon them. But by this separation of the bones of the pubes the recti muscles are also separated, and the bladder is deprived of its natural support at the anterior portion, while below, behind, and above, it is supported by the bones of the pelvis, spine, and various firm viscera. The whole distending force will therefore act on the anterior portion, which, with the skin, will be protruded forwards, and becoming thinner, will at last give way and burst outwardly. The bladder, no longer able to contain any urine, will contract, and, by the pressure of the abdominal viscera, will be protruded through the ruptured aperture. Thus, in addition to an impervious urethra, we shall find the bones of the pubes separated, the ureters opening externally through a ruptured and inverted bladder, and the ureters and kidneys very much enlarged; which are all essential circumstances of the malformation."

This hypothesis is ingenious, but wholly untenable, as it presupposes the secretion of urine to take place at a very early period of the fœtal existence; and even should this explanation be correct, it does not explain why, when the bladder had given way, the ossa pubis did not approximate and unite in a symphisis. The enlarged state of the ureters is rather referrible to the efforts of nature to supply the deficiency of the bladder. Moreover, the case related by Mr. Coates affords a direct contradiction to this theory. It is much more probable that these effects are all of simultaneous occurrence from the same defective formation.

At the conclusion of his lecture, Mr. Earle referred to the case of a child on whom he operated for nævus on the forehead, and who had also a very peculiar malformation of the genital organs. The penis was situated in perineo, and directed backwards, so as to have caused the child to be retromingent, if the urethra had been perfect; this, however, terminated by an open surface in perineo. In this case, at the time of birth there was a firm membrane closing this opening, which was divided before any urine could pass. This case affords additional proof, if any were requisite, against the hypothesis of Dr. Duncan, relative to the probable cause of defective organization of the bladder.

ON THE USE OF OPIUM IN FEVERS.

By P. M. Latham, M.D.

Physician to St. Bartholomew's Hospital.

Read before the College of Physicians.

There are forms of fever which are very simple and very easy to treat; and the simplest form is that in which all the symptoms observe a certain proportion to the state of the vascular system, so that, however various they may be, and how many soever the organs they involve, they become greater or less, and rise or fall, according to the degree of excitement manifest in the heart and arteries. Here the treatment is easy, because it hangs upon a single indication. Reduce vascular action, and the fever begins to decline; and with it also begin to decline whatever symptoms appertain to particular organs—to the

sensorium, to the organs of the chest, or the organs of the abdomen, or to any other part.

But there are forms of fever which are very complex and very difficult to manage; where the symptoms, taken altogether, do not conform themselves to the existing state of the vascular system, and where a single indication thence derived does not guide us in the treatment of the entire disease. Here, whatever the condition of the blood-vessels may require to be done, some special treatment is moreover demanded for the relief of particular organs; so that how far depletion should be carried, is often in practice a very subordinate question; while the life of the patient may depend upon a just determination in giving or withholding purgative medicine, or in making use of calomel largely or sparingly, according to present conditions of disorder in the abdominal viscera.

But my present purpose is with certain disorders of the brain which arise in the course of fevers, and with opium as their appropriate remedy.

It hardly ever happens that a fever passes through its entire course without some symptoms declaring themselves which are especially referable to the sensorium; and in the majority of cases where such symptoms appear, the morbid affection of the sensorium is unquestionably derived from the blood-vessels. Still the cases are not few in which it is derived from some other source.

Now, how important the brain is as an object of the physician's attention in fevers, must appear, not only from its extreme proneness to participate in the disease, but chiefly from the consideration that it is through the medium of this organ that death very frequently takes place.

The morbid affections of the brain in fevers are never without an evil tendency, come from what source they may. If from the blood-vessels, the danger is upon the whole less, because the method of cure is more evident; if from other sources, the danger is augmented by the difficulty of obtaining clear indications of treatment.

My observation has taught me, that, connected with fever, there are affections of the brain which are essentially inherent in the organ itself; and capable of relief only by remedies which exercise an immediate impression upon that organ. But of such remedies, it is opium only which I have learnt to employ to any salutary purpose.

With respect to the diagnosis of these affections, they are rather denoted by concomitant circumstances than by any stamp of peculiarity they bear on themselves. So much so, that I could not mention *any series* of symptoms flowing from the brain, and cured by opium, which I have not also seen cured by bleeding. Indeed I only know a *single symptom* referable to the brain which would suggest the use of opium as the suitable remedy; and that symptom is a state of wakefulness.

There may be much, or little, or no delirium—much, or little, or no tremor, or subsultus; in short, cases successfully treated by opium, have, in respect of other symptoms belonging to the sensorium, differed as much as possible from each other, while they have agreed in this one of perpetual wakefulness.

Yet the state of wakefulness does not alone justify the remedy; if it did, nothing more would remain to be said in defining its use. But the state of wakefulness may be present, and still opium not be the appropriate remedy. That *it is* or *is not*, must be determined by circumstances to be sought for, not in the sensorium itself, but elsewhere. To these concomitant circumstances, therefore, both as best discriminating the disease and best indicating the remedy, our attention must be mainly directed.

These circumstances are often such as to exhibit a sort of contrast with the existing affection of the brain. The proper febrile symptoms are just enough to characterise the disease and to constitute it *a fever*: some chilliness and some heat, alternating with each other; the countenance now a little pale, and now a little flushed; and the pulse more frequent, but not more forcible than natural; and the tongue merely clammy, and the stomach and bowels not notably deranged. Yet, with these moderate symptoms, there will arise at an early period, or at the very commencement of the fever, a disorder of the sensorium which is strangely disproportionate. Day after day the disproportion will be more striking, until at no distant period (for such cases pass on with great rapidity to their fatal termination), with a

pulse almost too feeble to be felt and too frequent to be counted, will be combined the wildest delirium and the most violent exertion of muscular force. If, under these circumstances, a state of continual wakefulness form (as it generally does) a part of the sensorial affection, opium is the appropriate remedy; and there is no other remedy (as far as I know) that offers the slightest chance of saving life.

In the cases described, the symptoms referable to the sensorium outrun, from first to last, every other symptom, whether belonging to particular organs or to the constitution at large. The symptoms, in their aggregate, constitute a fever; but the sensorium has its own special disorder, requiring a special remedy, and which is independent of the blood-vessels. When the patient dies, it is this disorder which kills him; for he dies of exhausted nervous energy, and, upon dissection, you find no visible vestige of disease either in the brain or in any other part.

But there are other cases of fever in which there is no evident contrast between other symptoms and those belonging to the brain, *at first*. The disorder of the sensorium keeps pace with that of other parts, and with the state of the circulation; and thus the disease proceeds until it is somewhat advanced, when the harmony between its symptoms is disturbed. Those of the brain outrun the rest. New indications of treatment arise, and if the patient can be saved at all, it must be by opium.

Thus the heart and arteries may be full of activity, and every symptom in proportion to it. Remedies are accordingly addressed to the vascular system, and succeed for every purpose they are intended to fulfil *except one*. The general febrile symptoms are abated by venesection. By venesection, or by topical bleeding, each organ loses its peculiar distress. The respiration is easier; the abdomen bears pressure, and even the head ceases to ache and throb. But, withal, *the delirium continues*. As other symptoms are relieved, the delirium is even aggravated. The patient mutters, or sings, or talks nonsense; he is absolutely sleepless day and night, and is jumping up and endeavouring to get out of bed. Such is the condition of the sensorium with an improved state of the circulation and of other organs—and even of the tongue.

To this delirium, low, muttering, or wild, as it may happen (for it may be all of these in the same individual in the course of a few hours), subsultus is liable to be added, and the unrestrained passage of the different evacuations.

In such a case as this, I have seen a single dose of opium, dextrously administered, change the whole complexion of the disorder in a single night, and place the patient at once in a state of safety.

Again, I have seen the sensorial affections incident to fever, which require opium for their cure, manifest themselves in another form. There has been high vascular action from the first; and large depletion has been required to subdue it and to guard particular organs, and especially the brain, from injury. Under such treatment, all has gone on successfully, and the patient has reached the point of convalescence; with a soft pulse, a cleaning tongue, no pain, and refreshing sleep for two or three days; when suddenly (the tongue, the pulse, and all other circumstances continuing the same) some strangeness of manner has arisen, and then the wildest delirium, and then the unrestrained passage of the evacuations. I have known the transition from such a state of convalescence to such a state of peril, take place in a few hours; and I have known the patient again brought back to a state of convalescence in twenty-four hours by a moderate dose of opium. This is a rare form of disease, but one in which, when it does occur, opium is eminently indicated.

Now, when the affections of the brain put on a character of great energy and violence, they are apt to suggest the notion of inflammatory action. Indeed the mere sensorial symptoms alone cannot be always distinguished from those of phrenitis; but there may be no phrenitis nevertheless. Those who are experienced in the medical treatment of the insane, have ably discriminated a class of cases characterized by great excitement, which they know to be curable by no remedies which deplete, but specially and exclusively by such as have a direct influence upon the nervous system: and although, in fevers, the very nature of the disease would always lead us to watch narrowly the state of the circulation, and suspect its possible agency in the produc-

tion of any sensorial affections that occur, yet it need not surprise us to find that even in fevers there should be some such affections which are independent of the blood-vessels, both in their production and their cure.

Still there are cases of fever in which the symptoms belonging to the brain have no such energy or violence, and the symptoms belonging to the vascular system present no such contrast with them as we have described, and yet opium is essential to the cure.

There may be simple wakefulness, and literally no other symptom whatever referable to the brain; and, as to symptoms belonging to other parts, they may be just enough to constitute a fever, and no more.

This is a form of fever which often ends ill, to the utter amazement of those who witness it, and who cannot tell how it has happened thus. The patient has hardly any fever; therefore there is no thought of danger. There is no change in his symptoms during several days; until suddenly all his strength is gone, and he cannot raise himself in bed. His tongue is become dry, and it trembles; some muttering delirium is added to his wakefulness; he passes his evacuations involuntarily, and soon dies.

In this manner does death sometimes take place, because one symptom, small and unobtrusive indeed, but of inestimable importance as a guide to practice, has been unfortunately overlooked from the beginning. The patient, perhaps, never mentioned it, and the physician did not inquire after it. Yet such a case as this, if this single symptom of wakefulness be duly estimated, and opium administered in season, will generally terminate well, and at an early period.

These affections of the brain, incident to fever so peculiar and so perilous, and requiring opium for their cure, deserve to be illustrated by any circumstances that can be brought to bear upon them beyond the mere symptoms of the particular case; and, indeed, they are capable of much illustration by the habits and state of health of individuals, before they became the subjects of fever.

And here I would make one general remark—that, by knowing *what* a man is, and *how* he lives habitually, the physician often arrives at a much better judgment and a better treatment of his diseases. It is trying a man's diseases by his health; and a most valuable test it is, and of great practical utility.

In healthy and vigorous bodies there is a certain balance and regularity of function which, even when disease befalls them, is seldom lost, but their morbid action is still harmonious and proportional. In them diseases are often severe, but they are generally simple: they often require the most active remedies, but they are generally easy of cure. On the other hand, the weak and valetudinary, who at the best are full of jars and incongruities, are obnoxious to the strangest forms of disease, hard to understand and hard to treat.

Now fever, when it happens to a perfectly vigorous and healthy man, is never characterized by any such peculiar affections of the sensorium as have been mentioned. These are incident, according to my observation, to those only whose habits and mode of living have been calculated to do an abiding injury to the nervous system, and who have been long actually suffering from such injury.

Every class of society has furnished me with instances of this form of fever, and every instance has confirmed the truth of the remark.

Among the higher and educated classes there is, in this age and country, a wonderful striving for all the objects of wealth, and honour, and power. We need only think upon the strife of politics, the hazards of mercantile gambling, and the wear and tear of hard professional toil, to see how many there must be who, from the common business of life, have derived both to their minds and bodies new feelings and impulses, and new susceptibilities of disease. These susceptibilities belong chiefly to the brain and nervous system, and they are apt to come forth into frightful activity when such men become the subjects of fever. The trouble of the brain gets the mastery (as it were) of the disorder of every other part.

The poor and mean among mankind have the mind overwrought, and the nervous system exhausted, by real calamity, just as the high and the educated by their more refined cares; an thus they often claim an unenviable approximation to them in the character of their diseases.

Into the hospitals of London a miserable class of patients is often admitted—the wretched outcasts of the streets.

They have crawled about asking alms all day, and at night have lain down in the open air and slept. They are sometimes picked up in a state of half-consciousness, and brought to a hospital. Of these some only require to be washed and fed, kept warm, and allowed to sleep, and then they recover without manifesting any real disease. Others, (and the greater number) after they have lain awhile, and their blood-vessels begin to react, put forth the symptoms of some serious malady; it may be an inflammation of some particular organ, or it may be a fever; and if a fever, it is almost always of that kind in which the derangement of the brain outruns that of the vascular system, and of every other part.

It appears probable, then, that the sensorial affections alluded to are ultimately determined by something peculiar to the constitutions of individuals before they become the subjects of fever. One may call it an unhealthy susceptibility at least, if not an actually morbid condition, belonging at all times to the sensorium and its functions. It is one of those things which cannot be spoken of with precision, and of which more may be learnt by attending to the *moral* causes (for such they are) out of which it seems to arise than to the thing itself. To these moral causes, in further illustration of it, I will add one *physical* cause, which is of most extensive influence—the habitual indulgence in spirituous liquors. Individuals, who have done a permanent harm to the functions of the nervous system by the abuse of spirits, do never, when they become the subjects of fever, suffer a delirium of the ordinary kind, in which the brain is excited nearly in the same proportion with the blood-vessels, and which, by remedies addressed to the blood-vessels, is uniformly controuled; but they suffer a delirium in which the brain is actuated disproportionably to, and (perhaps) independently of, the blood-vessels, and, if curable, to be cured by opium. This I venture to state almost absolutely, and without exception.

Now, when we contemplate these sensorial affections, said to be incident to fever, in their kind, in their causes, and in their cure, we cannot help seeing how much they possess in common with what is called "delirium tremens." In fact, they are the same thing. Yet I will not call them the "delirium tremens of fever," for fear of misleading by a name. Besides, the name itself is ill-chosen, inasmuch as there are affections of the brain, which, from their causes, their method of cure, and their own essential nature, must be supposed to belong to the same category, and yet have no tremor whatever among their symptoms.

There is a wide range of sensorial affections all *pathologically* the same. But the extreme instances, being the most striking, they have been picked out, and called by a particular name—"delirium tremens," as if they constituted a class by themselves, whereas they are only individuals of a much larger class. To this class also belong the sensorial affections in question, which are incident to fever.

Having then settled the nature of the disease, and its remedy, we must now determine the mode in which that remedy is to be applied, and its quantity.

When we desire to abate pain by opium, the degree of pain is the measure of the dose required. So, too, if we would subdue nervous irritation by opium, the degree of excitement informs us how much we ought to give. Simple wakefulness may be gently lulled to sleep by a few drops of laudanum, but wild delirium requires to be mastered, and (as it were) forced into repose by a much larger dose.

Thus the sensorial affections incident to fever, which are curable by opium, require various quantities of the remedy according to their degree. But in no case, even if the excitement reaches to convulsive action of the muscles, and the wildest delirium, is the quantity required *absolutely very large*. And here my experience has led me to a conclusion which no reasoning *à priori* could have reached, viz. that a much larger quantity of opium is necessary to remedy certain sensorial disorders when they exist alone, than when they are combined with fever.

In extreme cases of delirium tremens you fling away the resources of your art unless you venture to administer opium in doses which would run the hazard of poisoning a healthy man. The very same symptoms, carried to the greatest extremity, *and combined with fever*, are still to be subdued by opium; but twenty minims of the tincture are now quite sufficient for the purpose. I never gave a larger dose at once, and

I have seldom found it necessary to repeat it. If the dose be too little it is easily increased, but there is great peril if it be too much—*the peril of coma.*

The success of the remedy turns entirely upon the condition of its procuring sleep; and it is more or less complete in proportion as the sleep procured is, within certain limits, of longer duration.

When, therefore, in a case of fever, after long wakefulness, accompanied by wild delirium and a violent exertion of muscular force, with such a state of pulse as absolutely forbids the use of further depletion; when, in this extreme case, we administer the extreme dose (for such it is) of twenty minims of tincture of opium, we must be content to wait patiently the result; for the use of every other remedy is now sacrificed to this single one; indeed, while it is in the course of operation, the effective employment of any other is necessarily precluded.

We should wait patiently four hours at least; and in the meantime let every thing be made to favour the success of the remedy: let the room be kept silent and dark, and one individual only remain with the patient, charged not to utter a single word.

The four hours having elapsed, we are to determine from the state of the patient whether still more opium be needed or no, for the accomplishment of our purpose. We may find, that soon after taking the laudanum, the patient fell fast asleep, and has continued sleeping ever since. Thus the remedy has done all that we desire. Or we may find that he fell asleep, and soon woke again, and so has slept and woke, and slept and woke again ten times in the course of an hour, and in the intervals of waking his delirium has returned. Under these circumstances, should more laudanum be given? I think not. After a considerable dose has been administered, if any real sleep, however short, has succeeded, I would not give more until after a long interval, because I would not hazard the peril of an over-dose. Besides, though the remedy has not entirely fulfilled my wishes, it has not entirely disappointed them. These little snatches of sleep, after long-protracted wakefulness, are often productive of great benefit. The sleep, thus fitfully obtained in the course of one night, has entirely changed the complexion of a doubtful case, and placed the patient in security.

But we may find that the patient has not slept at all, or that it is doubtful whether he has or has not; yet, though the wakefulness remain, there may be an evident abatement of other symptoms which flow directly from the sensorium, of the delirium, of the muscular twitching; that the pulse is a little diminished in frequency and increased in power; in short, that the patient is altogether more composed, although he has not slept. Under these circumstances, it is safe and expedient to give more opium to the amount of half the original dose.

In cases where the delirium and excitement, accompanied by wakefulness, are less in degree, a smaller dose of opium may be relied upon for effects equally beneficial. In such cases I have been accustomed to give five minims of the tincture every hour, or every other hour, until the patient begin to dose.

There are cases where the indications for the employment of opium are doubtful. Wild delirium, and long wakefulness, and a circulation weak and fluttering, seem to call for a considerable dose of opium. Yet, withal, there is *a certain jerk in* the pulse, so that we cannot help suspecting that the blood-vessels have something to do with the sensorial excitement. Under such circumstances, I have certainly seen twenty minims of laudanum produce tranquil sleep, from which the patient has awoke quite a new man. But I have also seen the same quantity produce a fatal coma, from which he has never been roused.

Now, since it is a fearful thing to strike a heavy blow in the dark, where the alternative is of such magnitude, it is the safest and the best method to administer a small dose at intervals of an hour or two, so as to stop short of actual mischief at the first glimpse of its approach, or be led by a plain earnest of benefit to push the remedy to its full and consummate effect. Many doses may be required for this purpose, but we shall see, after the first or second, whether to go on or to desist.

There are cases where all the curative effects of opium are obtained by very small doses given at very distant intervals.

I have mentioned a form of fever in which simple wakefulness exists from the beginning, while the proper febrile symptoms are very moderate, and delirium is only at length added from the very exhaustion of the nervous system. Here *the whole treatment* consists in the dextrous use of opium, and it may be given at any, even the earliest period of the disorder. Sleep, sleep is the want of nature. Five minims of laudanum given every night will be enough to procure it while there is mere wakefulness, but when a little delirium is added, five minims more should be given twice in the twenty-four hours.

Do not let this be thought an insignificant practice. It is enough to save life, which will inevitably be lost without it.

March 17, 1832.

REPORT ON SMALL-POX.

To the Editor of the London Medical Gazette.

Sir,

The following is the substance of the report which I made to the General Court of Governors of the Small-Pox Hospital on the 2d of February. Should you be of opinion that the details therein given possess sufficient interest, you will oblige me by giving them publicity in the columns of your useful and widely circulated journal.

I have the honour to be, &c.

George Gregory, M.D.

31, Weymouth-Street,
Feb. 10, 1832.

Small-pox has not prevailed in London to any great extent during the past year. The bills of mortality give 563 as the total number who have perished by the disease in London in 1831, less by 64 than the mortality of 1830. The admissions into the hospital (as might reasonably be expected) have fallen off in a corresponding degree. 180 cases of variola, under some of its modifications, were received into the hospital in the past year, besides 13 cases of lichen and scarlatina, mistaken at the onset for small-pox. In the preceding year the admission of small-pox patients had been 255.

The mortality in the hospital during the past year has adhered closely to the common average, viz. thirty in a hundred. Out of the 180 cases 53 have died.

The proportion of patients admitted with small-pox subsequent to well-ascertained vaccination has increased, though in a slight degree only. In 1830 the proportion was 33 per cent. In 1831 it was 37 per cent; that is, out of the 180 cases received, 66 had been previously *vaccinated.* Several had been cut for the cow-pock ineffectually, but they are, of course, excluded.

Of these 66, 26 took the small-pox in that very mild form which practitioners generally call the chicken-pox, but which would more aptly be designated as the five day small-pox. In the remaining 40, the disease shewed itself in various degrees of severity. 6 of them died; three of the common and acknowledged effects of small-pox, and other three of a superadded disease which prevailed in the hospital during the spring and summer months, and on which I would beg to offer a few remarks.

Early in the year 1831, I noticed the interruption of convalescence in several cases by attacks of fever, sometimes simple, but oftener complicated with sore throat, or with erysipelas. The concurrence of sore throat and erysipelas has been noticed by several authors, especially by Mr. Arnott. In the month of April these cases increased considerably both in number and severity: one or two boarders were attacked, and one of the hospital servants. In them the disease took the form of simple fever. Erysipelas frequently shewed itself after the fever and sore throat had nearly subsided. It attacked equally the head, genitals, and extremities; and in several cases terminated by gangrene.

This superadded malady prevailed fully as much among those who had the small-pox mildly, as in those who had been weakened by a severe attack. Those who had undergone vaccination, therefore, were among the sufferers; and as I have stated, three deaths in that class of patients may be attributed to this unusual cause. In the month of July this disease gradually declined, and the hospital during the remainder of the year happily continued free from it. The opinions entertained regarding the causes of that erysipelatous fever which

occasionally spreads in hospitals, have been various, and even contradictory. Some have attributed it to *deficient*, and some to *superabundant* ventilation. Others have tried to connect it with the too frequent washing of the floors. In the present instance nothing satisfactory was made out with regard to its origin. It may, perhaps, be viewed as one of the evils necessarily attendant upon the collecting of patients together; that is to say, the air becomes at times tainted by vitiated secretions; and in spite of every precaution, fever will then spread.

To return to the subject of small-pox. The admissions consisted, as usual, chiefly of adults. The proportion of fatal cases among those unprotected by previous vaccination, though smaller than had been observed in some years, was still painful to witness. Of the 114 persons thus circumstanced, 47 died. It was curious, however, to observe the different grades of intensity in the action of the variolous poison upon the animal economy. Several of the number had the mild five day (or chicken) pox, undistinguishable from that form of the disease which occurred in 26 of those who had undergone previous vaccination. How much of this mildness is to be attributed to idiosyncracy, and how much to the quality of the infective germ, is a question not very easy to answer. Since the beginning of 1832, several cases have occurred tending to shew, that a mild case *produces* mild cases, and that malignant cases, in like manner, originate from each other. Ten or twelve sailors have come into the hospital from the neighbourhood of Wapping, all of them with mild small-pox; while two policemen, belonging to the same division, have taken the malignant disease.

Vaccination has been diligently pursued at the hospital during the past year. 3062 persons have been vaccinated, and 1060 charges of lymph have been distributed to medical practitioners and other applicants. Abundant supplies have been sent, on demand, to all parts of the Continent, to the West Indies, New South Wales, &c. From very ample experience, I can strongly recommend the mode of vaccination pursued at this hospital. From ten to fifteen vesicles are raised upon every arm. The local and constitutional irritation which follows is not greater than where two or three incisions only are made, whilst the supply of lymph is proportionably increased. The quantity of good lymph which runs to waste after suplying all applicants, especially during the summer months, is immense. It is, I believe, generally known that practitioners can be supplied gratuitously with charged glasses every morning from eleven to one o'clock; and that those who apply by letter from the country will have the same forwarded to them by that day's post. I should be much obliged to any of your correspondents if they would state, through the medium of your journal, the comparative efficiency of ivory points and glasses. The former would be adopted at the hospital, if their superiority could be made to appear.

REPORT

OF A

RECENT IRRUPTION OF SMALL POX IN CEYLON.

By J. Forbes, M.D.
Superintend. Gen. Vaccine Establishment.

Small-pox, after prevailing to a greater or less extent, in different parts of the island, for more than twelve months, appears at last to have ceased; still we have no other security against its future introduction and propagation than the vaccination of every individual who might by chance be exposed to it. That it affords the most ample protection against the dangers and mortality of small pox, can be in no way better or more easily proved, than by comparing together two different periods in which that disease raged here, namely, 1819 and 1830. On both occasions it was introduced from the coast of the Indian peninsula, appeared first in the maritime districts, and spread afterwards into the interior. As we possess no accurate information respecting the ravages it committed after the first six months from its commencement in 1819, our comparison will be limited to this space of time in both years. The number attacked then, during the first six months of its prevalence,

In 1819 was	7,874, and mortality	2,945
1830 ...	806	167
	7,068	2,778

The great difference between the two years, in the prevalence and mortality of small pox, cannot fail to strike every one, and can only be accounted for by the protection which has been afforded by the great increase in the number of persons vaccinated since 1819, amounting to nearly 300,000; the number vaccinated in 1830 only, being 63,284. We have no means of ascertaining, with any degree of accuracy, to what extent the disease spread after the first six months in 1819. But from its first detection in Colombo on the 21st January 1830, to its disappearance in March 1831, the numbers were—

	Admitted.			Died.	Proportion of Deaths to Admissions.
	Small Pox.	Chicken Pox and modified Small Pox.	Total.		
Maritime Districts	619	411	1,030	147	1 to 7
Kandyan Provinces	198	...	198	110	... $1\frac{3}{11}$
Whole island.....	817	411	1,228	257	... $4\frac{4}{5}$

There are several circumstances that clearly shew the advantages of vaccination, particularly the small proportion of deaths in the maritime provinces, where vaccination is pretty generally practised, as compared with the great mortality in the Kandyan provinces, where the prejudices of the inhabitants have until very lately prevented vaccination from being carried to any extent. The proportion of deaths to the number attacked in the maritime provinces being as one to seven—147 having died out of 1030; while in the Kandyan provinces the mortality amounts to one in one and three-fourths nearly—110 having died out of 198 attacked.

In the maritime provinces again, there is no district where vaccination has been carried to so great an extent as in that of Jaffna, and here the good effects of vaccination appear most conspicuous; for only six cases of severe small pox occurred in 117, and only two deaths took place in the whole—proving in the most satisfactory manner that, even when vaccination does not absolutely prevent small pox, it always ensures the occurrence of a mild and harmless, instead of a highly contagious, virulent, and fatal, disease.

During the first seven months, when the disease was most prevalent and fatal, great pains were taken in Colombo to find out the causes of the remarkable differences, with regard to severity and mortality, which it exhibited in different individuals; and the following table, drawn up from observations on 414 cases, admitted into the Pauper and St. Sebastian Hospitals, will go far to prove that these differences arise chiefly, if not altogether, from the greater or less success with which the operation of vaccination has been performed.

Cases occurring in persons who were attacked under the following circumstances.	Small Pox.		Chicken Pox.	Total.	Fatal.	Proportion of Deaths to Admissions.
	Severe.	Mild.				
1. Never vaccinated	90	33	8	131	58	1 to $2\frac{1}{4}$
2. Vaccinated, but without marks.....	37	19	8	64	18	... $3\frac{5}{9}$
3. Vaccinated, with unsatisfactory marks	34	23	12	69	15	... $4\frac{3}{5}$
4. Vaccinated, with satisfactory marks...	3	73	51	127	1	... 127
5. Who had previously had small pox ...	2	2	19	23	2	... $11\frac{1}{2}$
Total.....	166	150	98	414	94	... $4\frac{2}{5}$

Of the 414 cases here analyzed, it appears from the table that 131 acknowledged that they had never been vaccinated, and 58 of them died; 64 said they had been vaccinated, but could shew no marks, and 18 of them died; 69 had marks or scars, of one kind or another, on the arms, but not such as are left by vaccination when undisturbed in its course, and 15 of them died;

while, in those who had good marks of vaccination, the disease was rendered so mild that only one died. Nor can we allow this last case to throw any discredit on vaccination, when we find, immediately under it, that two patients died who had previously passed through small pox—as if on purpose to prove that the vaccine disease has at least as much, and perhaps more, power in preventing small pox, than a first attack of this disease has in preventing a second.

The protecting power which the increase of vaccination has afforded is also well established, by comparing the numbers attacked by small pox in 1819 and 1830; for every 10 persons attacked by the disease in 1830, nearly 100 were attacked in 1819; and for every 10 persons who died in 1830, 176 died in 1819. Had the protecting influence of vaccination been universally diffused throughout the island at both the periods referred to, 1819 and 1830, we have every reason to believe that 9,000 persons might have escaped the attack of a dangerous and painful disease; and that 3,000 of those who fell victims to it, might have been at this moment alive and well in the bosoms of their families.

CASE OF UNUSUAL DISLOCATION OF HIP JOINT.

To the Editor of the London Medical Gazette.

Albemarle-Street, March 24, 1832.

Sir,

If the following brief notice of an unusual luxation of the hip-joint should be considered worthy of a place in the Medical Gazette, it is much at your service.

I am, sir,
Your very obedient servant,
Robert Keate.

I was called into the country, on the 13th February, to see a gentleman who had met with a severe accident, by his horse having fallen backwards with him, and upon him, into a deep and narrow ditch; where he had remained, as he supposes, for near a quarter of an hour before he was discovered, when he was nearly exhausted by pain and by fruitless exertions in calling aloud for aid. The horse was lying on its back upon him, with his heels struggling in the air; but the gentleman, who is strong and muscular, appears to have retained a firm hold of the bridle, and thus to have kept down the horse's head, and restrained, in some degree, the violent efforts of the animal.

He had been brought home, and was on his bed when I arrived. On examining the limb, I found it unusually elongated, at least from three to three inches and a half. The thigh was much flexed upon the pelvis; the leg as much bent on the thigh. The whole limb was carried outward, or apart from the other, more than I had ever observed in any case of luxation. The knee and the foot were much everted; the trochanter extremely sunk, the soft parts being elevated in a circle around it. I found that the head of the femur was displaced in a very unusual manner, to a situation inferior to the ischiatic notch; and I felt it lying close to, and on a level with, the tuberosity of the ischium, where it was capable of being freely moved under my fingers.

Without noticing the usual preparations for reducing a luxation, it will be sufficient to say, that, in the first attempt, the head of the bone was thrown into the foramen ovale. A second extension enabled me to place it *nearly* in its proper position in the acetabulum, but it could not be perfectly replaced; and on gently moving it, and placing my ear on the trochanter, I felt and heard a distinct grating, as if of ruptured cartilage. By drawing the upper part of the femur outwards (by means of a round towel thrown over my neck), and pressing the knee sharply inwards, the head of the bone was replaced, with a snap, in the acetabulum; but even after this I was able to elongate or pull down the limb, and it was evident to me that this was owing to a portion of the cartilaginous labrum having been broken off during the violence of the accident.

The gentleman was quite aware, and mentioned, after the first step, as I may call it, of the reduction into the foramen ovale, that the head of the bone was not properly replaced; and he stated that the luxation had taken place by the same route, first into the thyroid foramen, and afterwards, while struggling in the ditch, from thence downwards to the situation in which I

found it. The case has proved very favourable, but there was a severe injury at the same time to the knee, which threatens still to be troublesome.

STRICTURE OF THE INTESTINES.

To the Editor of the London Medical Gazette.

SIR,

CONVINCED of the importance of the subject alluded to by your correspondent, Mr. Howship, in your journal of Saturday last, I request your insertion of a few comments on the opinions he has advanced. In primis, I object to the term "*permanent spasmodic*" stricture, as being altogether inappropriate; for though it may be presumed that all strictures, unless they are of a specific character, are in their commencement spasmodic affections of the intestine, producing at first no organic alteration, yet I contend that before the obstruction can become "permanent," an obvious deviation from the healthy condition of the part must ensue.

Certainly no one can for a moment doubt that spasmodic contraction frequently gives rise to "all the inconveniences induced by habitual confinement of the bowels," and "that the bowels are occasionally subject to spasm," or that spasm and pain, though not "pain and spasm," are inseparable companions. Equally obvious is it, that nothing would be so likely to remove such a disordered condition of the intestine as the use of injections, or tepid bathing, locally applied.

But if I understand Mr. Howship correctly, (as from the two cases he has adduced in exemplification of his theory, it may fairly be presumed I do) he states that permanent stricture of the sigmoid flexure of the colon, if not of the rectum, may be removed by injecting the intestine—an opinion from which I must dissent in toto. Furthermore, and I speak from extensive experience, we shall find it impossible in many cases of confirmed contraction, whether in the colon or rectum, to inject the bowel so as to produce distention of its canal, owing to the irritability of the stricture; and when we are so fortunate as to succeed thus far, the fluid does not operate by dilating the constricted part, but by preternaturally distending the intestine above the diseased portion, from which we incur no small risk of rupturing the bowel, especially if it be, as is frequently the case, debilitated by disease, and from ulceration of its internal tunic.

The treatment recommended by Mr. Howship was practised in cases of constriction of the sigmoid flexure of the colon and rectum, by that excellent surgeon and worthy man, the late Mr. White, of Bath, *as an auxiliary to the bougie;* but I have reason to know that many years prior to his death he abandoned the plan as next to useless. A similar method has of late been adopted on the continent, upon the principle of hydrostatic pressure.

There has not perhaps been a more simple, yet valuable improvement in modern practice, than the habitual use of enemas: never, however, ought it to be forgotten that the use of remedies is one thing, the abuse of them another. Experience has long ago convinced me that while the plan advised by Mr. Howship is of no service in *confirmed* stricture, whether in the colon or rectum, it is almost certain, if used for a continuance, to produce serious injury; for from the immoderate distention thus induced, the bowel becomes insusceptible of the natural stimulus which arises from any ordinary accumulation of fæces, and hence results habitual costiveness and a long train of ills.

I offer these observations on Mr. Howship's paper with the most perfect good feeling towards that gentleman, and am, sir,

Your obedient servant,

FRED. SALMON,

Surgeon to the General Dispensary.

12, Old Broad-Street,
March 26th, 1832.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Report on the Chemical Pathology of the Malignant Cholera; containing Analyses of the Blood, Dejections, &c. of patients labouring under that disease in Newcastle and London, &c. &c. By W. B. O'SHAUGHNESSY, M.D. &c. &c.

SOME weeks ago, Dr. O'Shaughnessy

published in this journal the result of his examination into the chemical history of the blood and dejections in cholera, promising to lay the details before the public at an early period. These are now before us.

The report is divided into three sections, whereof the first relates to the blood in its healthy state, the second to the chemical pathology of malignant cholera, and the third to the conclusions which the premises appear to warrant.

I. *Chemical Composition of Healthy Blood.*—In this portion of his pamphlet the author alludes particularly, and in terms of strong commendation, to the recent analysis of the blood by M. Lecanu, of Paris. It is as follows, and will be found useful to our readers as a standard of reference:—

ANALYSIS OF THE BLOOD, BY M. L. R. LECANU.

Journal de Pharmacie, No. IX. September 1831, p. 502.

Water	780.145	785.590
Fibrine	2.100	3.565
Albumen	65.090	69.415
Fatty matter:		
a. crystalline	2.430	4.300
b. oily	1.310	2.270
Colouring matter	133.000	119.626
Extractive *soluble in alcohol water*	1.790	1.920
Albuminate of soda	1.265	2.010
Muriate of soda; Muriate of potassa; Carbonate; Phosphate; Sulphate — Alkaline	8.370	7.304
Carbonate of lime; Carbonate of magnesia; Phosphate of lime; Phosphate of magnesia; Phosphate of iron	2.100	1.414
Loss	2.400	2.586
Total	1000.000	1000.000

In commenting on the various matters which are either invariably or occasionally found in the circulating mass, Dr. O'Shaughnessy makes some observations on the colouring matter, particularly in reference to the changes which it undergoes in the transition from venous to arterial blood, and *vice versâ*; but on this point we shall reserve ourselves till the appearance of Dr. Stevens' work on the blood, now in the press. He then proceeds to speak of certain substances asserted by some to exist in healthy blood, but the presence of which is disputed by others; and of those, the first we shall notice is the statement of Hermann, of Moscow, because he has connected it with the pathology of cholera. It may, perhaps, be remembered that we formerly referred to the views of this gentleman, and endeavoured to shew their inaccuracy. He asserts that the healthy blood contains acetic acid in a *free* state; but we have only to repeat what we said before, that until he disproves the existence of soda either pure or in the form of carbonate, it is superfluous to enter into any argument upon a position so absurd, as the existence in the same solution of acetic acid and an alkali refusing to combine.

The analysis of the blood, recently published by Dr. Clanny, in which he not only maintains the presence of free carbon, but actually states the quantity to have been 32° in a healthy specimen, and 60° in the blood of a patient labouring under cholera, is adverted to by Dr.

O'Shaughnessy with much scepticism; and for ourselves, we must confess, that considering the statement to be at variance with the results obtained by all the best chemists, and keeping in mind the loose manner in which, as we have demonstrated in the pages of this journal, Dr. Clanny has been in the habit of advancing untenable positions, we have no hesitation whatever in rejecting his analysis as wholly undeserving attention.

Among the other ingredients which have been spoken of as existing in the blood, there remain cholesterine, ozmazome, and cuorine, silica, manganese, and copper. The three first rest on the authority of experiments performed by M. Denis, and which have since been proved to be fallacious: the three latter have been met with, but in quantities so infinitessimally small as to afford merely traces of their existence.

Lastly, we ought to mention, as a constituent of venous blood, carbonic acid, the source of which is one of the points we shall discuss in reviewing the forthcoming work of Dr. Stevens, who has taken so prominent a part in its investigation.

II. *Chemical Pathology of Malignant Cholera.*—Until very recently no satisfactory attempts have been made to ascertain the changes which the fluids undergo during the presence of cholera. The first notice on the subject, we believe, was contained in Dr. Christie's work, but this merely went the length of indicating the existence of albumen in the liquid parts, and of fibrine in the soft flaky matters evacuated. Professor Hermann, of Moscow, came next, and the author before us shews that the only part of his analysis of the blood of cholera patients which is correct, is the very obvious one, that there is a deficiency of water. It is unnecessary to go into M. Hermann's statements, because the observations we have made above shew how little he is entitled to confidence. He represents the serum as alkaline, and the crassamentum as acid!

The next analysis is that of Dr. O'Shaughnessy, the results of which we published at the time, and we think the best graphic view we can offer is the following table, which our readers will find one of useful reference: it contains, 0, the composition of healthy serum according to M. Lecanu. 1 and 3, that of persons who died of malignant cholera; and 2, that of man labouring under violent *bilious diarrhœa.*

COMPARATIVE ANALYSIS OF SERUM IN HEALTH, MALIGNANT CHOLERA, AND BILIOUS DIARRHŒA *.

INGREDIENTS.	Healthy Standard of Lecanu.	Malignant Cholera. Mrs. Barras.	Bilious Diarrhœa. Mr. Hawthorn's.	Malignant Cholera. Dewar.	REMARKS.
	0.	1.	2.	3.	
Water	906.00	854.00	921.75	866.80	
Albumen	78.00	133.00	61.85	124.0	
Urea	0.00	0.40	0.00	0.00	
Organic matter, soluble in alcohol and water	1 69	*4.80	*5.20	*4.00	* Embrace the organic matter and albumen with soda.
Albumen combined with soda	2.10				
Fatty matter:					
Crystalline	1.20	1·40	1.90	1.23	
Oily	1.0				
Muriat. Soda	6.00	4.00	5.00	2.17	
Muriat. Potassa					
Carbonate of Soda	...	0.00			
Phosphate of Soda	2.10		2.30	0.5	
Sulphate of Soda					
Carb. Lime					
Carb. Magnesia					
Phosp. Lime	0.91	1.60	1.10	0.70	
Phosp. Magnesia					
Phosp. Iron					
Loss	1.00	0.60	0.90	1.5	
Total	1000.00	1000.00	1000.00	1000.00	

* In the original there are one or two misprints in this table, which we have corrected.—E. G.

The crassamentum, it ought to have been observed, was found normal in the proportion of its ingredients in cholera, and therefore only wanting the watery portion to restore it to its proper state. It will be observed that in one case (that of Mrs. Barras) a small portion of urea was found, which was not so in the case of Dewar. The blood of the latter, we are told, when examined by means of a powerful microscope, "did not differ in any apparent degree as to its physical organization from that drawn from an individual in robust health."

The *dejections* were composed of water, mucus, carbonate, acetate, muriate, phosphate, and sulphate of soda. There was no albumen, neither traces of caseum, nor of the principles of bile. The flaky matters were composed of albumen and fibrine. It is of importance to add that the specimen examined was obtained before any medicines had been exhibited.

It only remains that we should give the processes in detail by which these results were obtained.

Analysis of a Specimen of Serum taken from a Patient labouring under severe Blue Cholera.

"Mrs. Barras, ætat. thirty-nine, a widow of excellent habits, general good health, and comfortable circumstances, was seized with cramps, epigastric pain, and giddiness, about 10 P.M., on the 17th December. The attendants state that she became deadly cold, her features altered, and her strength overpowered, and that she spent some hours in this condition (having vomited and been purged about six times) during the night. At 9 A.M. a vein was opened at the bend of the elbow; the blood issued with great difficulty, and was at first extremely viscid and dark; but subsequently flowed more freely, and brightened in colour.

"The blood was placed in a basin, and at half-past 11 A.M. had separated into a loose, bulky, crassamentum, and transparent, but unusually viscid, serum. The crassamentum having been broken and much exposed to the air, the serum alone was removed for analysis.

"From the commencement of the attack to several hours after the bleeding, the patient passed no urine. The hot-air bath having been assiduously employed during the day, she rallied a little, but sunk and died during the night.

"*Examination of the Serum.*—The serum having been carefully preserved in a stoppered phial, was examined 24 hours after removal. In colour and degree of transparency it differed in no respect from ordinary serum.

"1. Its *spec. gravity*, compared to water (1.008), was 1,0.41.

"2. It did not turn turmeric paper brown, or restore the colour of reddened litmus.

"3. Five hundred grs. by weight were evaporated in a water bath, until a small mirror was not dimmed by the discharge of aqueous vapour. A transparent brittle mass was obtained, weighing 73 grs., indicating a loss of water of 427 grs.

4. The powdered residuum was introduced into a flask and boiled for ten minutes with distilled water. No diminution in bulk was sustained, and the mixture was filtered.

Examination of the Filtered Fluid in No. 4.

"5. Evaporated to dryness in the water-bath, left a brownish deliquescent residuum, weighing 5 grs.

"6. Residuum treated with strong boiling alcohol, lost 2.50 grs.

"7. Alcoholic solution did not affect litmus or turmeric paper.

"8. Alcoholic solution evaporated to one-eighth its original bulk, and allowed to cool, deposited groups of minute, white, cubic-shaped crystals.

"9. The superjacent fluid was cautiously transferred to a watch crystal.

"10. The crystals (No. 8) were dissolved in a drop of distilled water, and the drop divided into three parts by a capillary tube, and transferred to three separate watch crystals, marked *a*, *b*, *c*. A minute drop of the *nitrate of silver* caused a white, clotted precipitate in *a*, which redissolved in *ammonia*, and was again precipitated by *nitric acid*. *Chloride of platinum* produced no effect in *b*. *Oxalate of ammonia* no effect in *c*.

"11. A very minute crystal (No. 8) was heated on platinum foil, over the spirit-lamp flame. It blackened at first, and then left a white stain, which did not turn turmeric paper brown, and which gave a yellow tinge to the blow-pipe flame.

"12. The fluid (No. 9) was diluted with a few drops of distilled water, and a drop of nitric acid added. A white, flaky precipitate was formed.

"13. The fluid (12) being separated from the flakes by a capillary tube, was tranferred to a watch crystal, and evaporated to dryness at a temperature of 100°. A numerous group of radiated crystals was formed, of a strong urinous smell, of yellow colour, soluble in water, and solution reddening litmus, soluble in alcohol, from which it was again crystallized, retaining its urinous odour. Heated on platinum foil it blackened, and was totally consumed.

"14. The flaky precipitate (12) was washed with water, and divided into three minute portions. It was found to be solu-

ble in ammonia, acetic and muriatic acids, and to be totally destructible by heat.

" *Conclusions.*—1 shews the serum to be deficient in alkali; 10, 11, 12, 13, and 14, denote the presence of muriate of soda, urea, and albumen.

Examination of the Solid Residuum, No. 5.

" 15. The residuum (5), insoluble in alcohol, and weighing 2.20 grs., was calcined on platinum foil, and left 2 grs.

" 16. The residue was dissolved in distilled water, and the solution slightly tinged tumeric paper brown.

" 17. Solution (16) divided into several drops, previously acidulated with nitric acid; was precipitated by nitrate of baryta, nitrate of silver, and chloride of platinum; by oxalate of ammonia and ammoniaco-phosphate of soda. It gave no blue colour with chloro-ferro-cyanate of potash, and a drop evaporated to dryness, and the residue held on platinum wire in the blow-pipe flame, gave it a fine yellow colour.

" *Conclusions.*—16 indicates traces of alkali; 17 shews the presence of sulphates and muriates of soda, potassa, traces of combinations of lime, and magnesia—no iron.

Examination of the Solid Residuum, No. 4.

" 18. The solid substance remaining on the filter (4) was again dried in a water-bath, and then boiled for ten minutes in an excess of strong alcohol, and filtered while warm.

" 19. The alcoholic solution was clear while hot, but became opaque and turbid as it cooled.

" 20. Evaporated to dryness in a water-bath, left a yellow oleaginous residuum, weighing seven-tenths of a grain, insoluble in water, and totally soluble in sulphuric ether, forming a yellow tincture.

" 21. The ethereal solution (20), allowed to evaporate spontaneously to one-eighth its bulk, deposited numerous crystalline scales, surrounded by a yellow fluid of the consistence of olive oil.

" 22. The residue (21) washed in cold alcohol, the oily matter was dissolved, and the crystalline scales remained behind.

" 23. Boiling alcohol dissolved the scales, and again deposited them unchanged. The hot solution did not tinge turmeric paper.

" 24. When crystallised and drained, the crystals were quite white, fused at a gentle heat, and were totally insoluble in, or unchanged by, caustic potassa. They were decomposed by heat, and left traces of earthy matter, which did not tinge turmeric paper.

" 25. The oily matter (21) floated on water in globules, was totally soluble in cold alcohol and ether, and, on incineration, left a trace of earthy matter, which did not tinge tumeric paper.

" *Conclusions.*—All these experiments indicate the presence of the crystalline and oily fatty principles of the blood described by Lecanu.

Examination of the Residuum, No. 18, insoluble in Alcohol and Water.

" 26. The residuum (18) was again carefully dried in a water-bath.

" 27. Incinerated in a platinum crucible, weighed 0.80.

" 28. Residuum (27) insoluble in alcohol and water, and did not redden turmeric paper.

" 29. Dissolved completely, without effervescence, in diluted muriatic acid.

" 30. Muriatic acid solution, divided into several parts, was tested by the following re-agents.

" 31. The red prussiate, or chloro-ferro-prussiate of potash, gave a faint blueish green precipitate.

" 32. Ammonia a white precipitate, which was re-dissolved in muriatic acid.

" 33. The solution (32) formed a copious precipitate with oxalate of ammonia; the precipitate was thrown on a watch crystal, and the fluid separated by a capillary tube, and transferred to another crystal.

" 34. The precipitate by oxalate of ammonia (33) was calcined on a slip of platina foil, and the residue stained turmeric paper a deep brown.

" 35. The fluid (33) was precipitated white by caustic potassa.

" *Conclusions.*—26, 28, and 29, shew the absence of carbonate of soda; 31 denotes a trace of iron; 32, 33, 34, and 35, concur to shew the presence of minute traces of lime and magnesia."

ANALYSIS OF ALVINE DEJECTIONS.

" Case I.—James Dewar. See Report.

" The appearance of these evacuations was most characteristic; they were perfectly colourless, very fluid, and containing numerous white flakes, which subsided when the fluid was allowed to stand. The fluid having been filtered *through fine gauze*, the solid flakes and the liquid part were separately examined.

" *Examination of Liquid part.*—Sp. gr. 1.008.

" 1. Turmeric paper immersed in the fluid was strongly reddened, and reddened litmus paper restored to its blue tint.

" 2. Did not coagulate by boiling, or by the addition of the mineral acids.

" 3. Evaporated to one-eighth at a boiling temperature, still reddened turmeric paper, and turned reddened litmus blue.

" 4. Before evaporation, was precipitated by corrosive sublimate and ferro-cyanate of potassa.

" 5. Alcohol added to the reduced fluid, occasioned a precipitate of white flakes, soluble in water.

"6. Two hundred and fifty grains of fluid evaporated to dryness left 1.1—45 grs.

"7. Boiling alcohol took up from the residuum No. 6, 0.30 grs.

"8. Alcoholic solution (7) evaporated to dryness, blackened when exposed to heat, and left a crystalline residue of the muriate and acetate of soda.

"9. Permuriate of iron occasioned no red colour in the fluid (1).

"10. 1000 grs. of the fluid were evaporated to dryness, and calcined on a platinum tray: $5\frac{1}{10}$ grs. of saline matter were obtained. The saline residue reddened turmeric paper powerfully; effervesced with acetic acid. Sulphuric acid caused the evolution of fumes of acetic acid.

"11. No precipitate in its aqueous solution by tartaric acid, chloride of platinum, or perchloric acid.

"12. The aqueous solution precipitated nitrate of silver white: precipitate insoluble in nitric acid, and soluble in ammonia.

"*Conclusions.*

"These experiments indicate that the liquid portion of this evacuation consisted of water, mucus, albumen in small quantity, and muriate, acetate, and carbonate of soda. It contained no caseum, sulpho-cyanate of potassa, or bile.

"*Examination of Solid Portion.*

"Insoluble in alcohol and water; totally destructible by red heat. Readily soluble in acetic acid and alkalies, and not precipitated from its alkaline solution by acetic acid. *Not reddened when treated by* STRONG SULPHURIC ACID, *and cautiously warmed.* Precipitated copiously, of a yellow colour, by ferro-cyanate of potash, from its solution in acetic acid.

"From these experiments, I am inclined to conclude that the flaky matter in *this case* was principally composed of *fibrine.* All practical animal chemists are aware of the extreme difficulty of distinguishing this substance, in the solid state, from coagulated albumen. The only mode of distinction I have been satisfied with, is the experiment with *sulphuric acid,* which, when properly conducted, affords strong evidence of the nature of the substance under examination.

"We thus find that the dejections in the case of Dewar contained the most remarkable of the principles deficient in his blood."

When to the above we add that accounts have been received of an analysis of the fluids in cholera, performed by Rose, the distinguished chemist of Berlin, during the late prevalence of the epidemic in that city, and that not only Dr. O'Shaughnessy, but Dr. Turner, of the London University, has made a similar examination with regard to the blood and evacuations of those affected with the disease now raging in this metropolis, and that all these experimentalists have arrived at the same conclusions, nothing farther, we conceive, can be wanting to prove their accuracy, and the strong presumption in favour of the universality of the phenomena.

There yet remains one part of the "report" to be considered, viz. the pathological and therapeutic conclusions to be derived from the preceding statements; but upon these we cannot enter at present, though we shall recur to them in reviewing the work of Dr. Stevens. We may just mention that the curative indications pointed out consist chiefly in endeavouring to supply the blood with those principles in which it is deficient. When Dr. O'Shaughnessy read a paper at the Westminster Medical Society some months ago, the accuracy of the opinion which he maintained in attributing any superiority to the oxygenated salts, was freely controverted in this journal. It is but right to add, that with the candour of a man whose object is truth, not victory, he has abandoned the above position, convinced by his subsequent investigations that it was untenable. We cannot close our analysis of his present work without expressing our high approbation of the unostentatious and philosophical manner in which it is composed, and our conviction that the author has conferred a very important service on pathology by bringing the exact sciences to bear on the diagnosis of this obscure and most disputed malady.

MEDICAL GAZETTE.

Saturday, April 7, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

INJUSTICE OF THE DAILY PRESS TOWARDS THE MEDICAL PROFESSION.

IT would be difficult to conceive any thing more completely base than the conduct of the press generally has been towards the members of the medical profession, ever since the appearance of the cholera in

London. The tergiversation—the unfairness—and the falsehood displayed upon the occasion, have only been equalled by the almost incredible manifestations of ignorance and prejudice by which they have been accompanied. The worthy editors, indeed, seem, with few exceptions, to have proceeded on the supposition that the greater part of their readers were fools; and as regards those who may have given credit to their statements, or attached importance to their opinions, it would be uncandid to deny that the estimate may have been tolerably well founded.

Among their more intelligent readers, however, throughout the kingdom, are medical practitioners to no inconsiderable amount; and on them we call, ere the events are gone by, to give their attention to the subject for a moment, as it will enable them to form an estimate of the degree of knowledge and candour which the conductors of the press bring to bear upon medical subjects, as well as of the taste and judgment of those members of our profession who seek for fame in the columns of a newspaper.

It may be remembered, that when the present epidemic first shewed itself at Sunderland, its existence was denied by some of the inhabitants of that place engaged in trade, and that the London journals long and bitterly condemned the interested motives, and the short-sighted policy which had dictated the attempt at concealment. The unwelcome truth, however, soon became too apparent to be denied, and the subsequent arrival of the disease at each town and village which it visited, was immediately made known. At length it was rumoured, and soon after officially announced, that the malady had reached the metropolis. Could we, for a moment, forget the clamour which has since ensued, and carry our imaginations back to the period when a malignant disease was first declared to be among us, we should expect to find the public press eager to verify those pretensions to superior probity to which, by reproving their neighbours, they had so recently laid claim;—and anxiously using all their influence in reconciling the public mind to an evil which, by well-regulated preparations, might be mitigated, but which experience had shewn could not, by any human means, be altogether averted. This, we say, is what might reasonably have been expected of those self-constituted guardians of the public weal—both from their previous tone regarding Sunderland, and on the general principle of expecting men, when there is no obvious motive to the contrary, to act according to the dictates of common honesty and common sense. Or, if this be accounted Utopian, in reference to the daily press, at least it might have been supposed that they who had been most liberal in their censure of others, for attempting to conceal the presence of the disease, would themselves have been cautious in denying its existence, save on the clearest evidence—that they who had so readily discovered, and so unsparingly exposed, the mote in their brother's eye, would have taken care that there was not a beam in their own. We never certainly have entertained any very extravagant notions of the immaculate purity of the press, yet we confess that — had we not lived to witness it—we could not have believed it possible to exhibit such astounding assurance in conceiving, and such marvellous rapidity in the execution of, that literary *legerdemain* by which all previous facts, opinions, and asseverations were shuffled off, and a totally new aspect given to every thing. Some persons have thought our worthy contemporaries were themselves deceived; but they do injustice to their skill and tact in bamboozling the

public. When a certain portion of the press (for we are willing to admit one or two honourable exceptions), asserted that cholera was not in London, they knew the falsehood of their statements, and contemned in secret those who believed them, just as the knavish juggler laughs in secret at the gazing rustics who are deceived by his tricks. Others again have imagined, from the sudden and total change in their tone, that the conductors of the daily press in London, or those to whom they trusted for information, had made some unexpected discovery regarding cholera, and that a closer examination of the epidemic had convinced them that all the medical men who had studied it in the north, had been completely mistaken. Not so; these wise men did not wait for examination or inquiry; they met the pestilence at the very threshold by denial—they scouted the idea that it was cholera before they or their "eminent" counsellors had seen a single case of it; and, therefore, it is but reasonable to infer that their knowledge came by intuition. The arguments too by which the correctness of their opinions was proved, were of the most convincing kind:—"Cholera come here—impossible! How can *Indian* cholera exist in London?"

There is, indeed, one other explanation which has been given of the sudden light which burst upon the press, and which, perhaps, matter-of-fact people may be disposed to think had some trifling share in producing it:—The trade of Sunderland and Newcastle was found, as might have been expected, to have suffered much during the avowed prevalence of the epidemic in those places, and the merchants in London thought it was at least worth making an effort to avoid a similar evil. For this purpose they intimated to some of the leading papers (and especially to one which had been very liberal in communications from their own correspondent at Sunderland) that if a different line of proceeding were not adopted in the event of the disease reaching the Thames, all their support, in the way of advertisements and other patronage, should be withdrawn. Due attention was paid to an argument so important; the matter to be weighed was but truth against interest, and we need scarcely say that the former kicked the beam. In one instance, it is said that a regular consultation was held, and it was straightway resolved *that there should be no cholera in London.* Let not our more distant readers conceive, then, for one moment, that the disease in those districts of London, where it has raged, has presented any features to distinguish it from that which has prevailed in the north of England, or that there has been any other circumstance in its history here, to warrant the altered tone of our much-esteemed contemporaries—not at all: it was a mere question of pounds, shillings, and pence, with which truth and science had nothing whatever to do.

And now, the principle of denying the existence of cholera having been agreed upon, the whole machinery of the press was set to work to raise a hue and cry against the medical profession; to whom they attributed the incredible folly of "getting up" a pestilence among the poor, the immediate effect of which was to drive away the rich. Henceforward every man who honestly gave his opinion that the same disease which had successively shewn itself in India, in Persia, in eastern Europe, and in the north of England, had at length reached London, was held up as an ignorant blockhead or an interested hireling, while the sentiments of individuals and periodicals then, for the first time and probably the last, merging from their fitting obscurity, were quoted as of high authority. Many of these "eminents," and their sagacious patrons, stand irrecoverably committed to the opinion that

the disease called cholera in London is not the same as that so called in Russia or in India. They express no doubt of the malady being in Germany, in France, in Scotland, and in Ireland—that is, to the east, the south, the north, and the west of this metropolis—and yet they expect the people of England to believe that it is some other disease which is epidemic in London. Do they who have advanced this opinion themselves believe it? *They do not.* Let the reader but mark the manner in which the writers in the newspapers are gradually decreasing in numbers, and in the confidence with which they express their opinions; let them look to the itinerant orators of our societies, who now wish to avoid the question of the identity of the disease, and confine themselves to whether it be contagious or not?—an inquiry, be it observed, which has no connexion of any kind with the other: again, let him look to the tone of the newspapers, and he will find that it is quite altered;—they now talk of the "epidemic," and the "scourge," and sometimes inadvertently and indirectly refer to it as identical with the Indian cholera. The whole of them are trimming and turning, and looking out for some means of escape; but they are doing so in a shabby, unhandsome way, trying to sneak out of the dilemma without having the candour to admit the simple fact, that their opinions are abandoned, because found to be wholly untenable.

But though the "no cholera" party would gladly shuffle out of their awkward position, unfortunately the mischief they have done cannot be so easily repaired. It is only now that the effects of the systematic misrepresentation which has been practised is coming into full operation; and now are the public, and more especially the poor, beginning to reap the harvest which the press has prepared for them. Since we last addressed our readers on this subject, numerous instances have occurred in London and elsewhere, in which the misguided rabble have risen against their best, and, in the hour of the present visitation, their only friends. The medical men in visiting the sick have been insulted, threatened, and even assaulted, in such a manner as seriously to endanger their personal safety*; while the exhausted victims of the disease have been stopped on their way to the hospitals, and dragged from their conveyances, which have been broken in pieces and converted into weapons against their attendants. The effect of this has been to render medical men in general more backward than they would otherwise be in affording their assistance, while in some places they have felt themselves called upon to resign the charges they had undertaken, and to declare their resolution to attend no more in the capacity of "cholera doctors†." In fact, if there did not exist in the breasts of medical men nobler motives of action than their traducers give them credit for, the poor would be left without assistance; and in districts where the disease prevails extensively, the deaths would be too rapid and too numerous for speedy interment, so that the exhalations from the bodies would poison the atmosphere, as they did in some of the plagues of old.

It is curious to observe the well-feigned astonishment of the press at the outrages we have described; they are shocked at such excesses. Innocent souls! they never taught the rabble that cholera was a "humbug"—they never said it was "got up by the medi-

* We know of one instance in which a medical man narrowly escaped being thrown from a two-pair-of-stairs window; another in which a ferocious dog was set upon the surgeon; and a third in which an Irishman, armed with a knife, watched for the medical attendant, and was prevented from stabbing him only in consequence of having avowed his intention to do so.

† At Paisley above twenty district surgeons have sent in their resignations, and will attend no more of the poor affected with cholera, owing to the brutal treatment they have received in the discharge of their duties.

cal and surgical gentry"—they never called those who believed in cholera, "the lower branches of the healing art," who were "eager to realize great gains by it"—they never hinted that there were "impostors" and "hired inquisitors," who received "twenty guineas" a day for perpetuating the "imposition." All of these expressions, and hundreds of a similar nature, have been used by the daily papers, which yet express their astonishment that the poor misguided creatures, who believe their statements just as firmly as if they were true, should rise against those whom they have been taught to regard as unprincipled wretches, living by their destruction, and interested in perpetuating their sufferings.

Without wishing to exaggerate the matter, or to use any language of unbecoming strength, we must say that we have been very thoroughly disgusted with the usage which our profession has received during a period of great exertion and anxiety, and, in some places, of almost unparalleled devotion to the public service; nay, we will go farther, and add—of such advantage to the community as not all the united resources of the empire, without the assistance of the healing art, could have effected. We have therefore thought it might not be altogether without its use, to place the circumstances prominently before our readers ere the events be forgotten, that when they see questions of science discussed in our newspapers, or find persons of very questionable claims to their respect put forth as oracles, they may bear in mind the history of the present epidemic, and judge how far *they* are entitled to attention, who admitted the existence of cholera in other places all around, but denied its presence in London, and traduced those who devoted their days and nights to mitigate its ravages—because they were desired to do so by the merchants.

As to those weak brothers of our own profession, who, for a little temporary notoriety, chimed in with newspapers, we shall say nothing, because we would not willingly trample on the fallen; the figure which some of them at this moment make in the public eye is a warning to the prudent, which no remarks of ours could strengthen.

FRENCH MEDICAL REASONING.

The first cases of cholera in Paris, brought to the Hôtel Dieu, occurred on the 26th ult. Early on the 31st—that is, *at the end of four and a half days' experience*—a proclamation was issued, bearing the names of the physicians and surgeons of that hospital, of which the following is an extract:—

"That although, up to the present time, this hospital has received the greatest number of persons affected with cholera, we (the undersigned) have not observed any circumstance which authorizes us to suspect that the disorder is contagious."

March 31, 1832.

This document, which is rendered absolutely ridiculous by the shortness of the period which had elapsed, and which strikingly illustrates the impetuous rashness of the parties, is actually quoted in the London newspapers as highly satisfactory, and proving the great superiority of medical science in France. If they had contented themselves with praising the superior *quickness* of the French in forming opinions, there would have been some point in it.

TREATMENT OF CHOLERA IN THE HOTEL DIEU.

The progress of the disease in the French capital has been strikingly rapid. From its first outbreak, on the 26th ult., up to Wednesday last (4th), the total numbers have been—cases, 1,052; deaths, 395. The exertions of the faculty, backed by the cordial and effective regulations of the government, are described as unremitting, and deserving of all praise. We subjoin from the *Gazette Médicale* (just received) a

summary of the treatment adopted by the physicians of the Hôtel Dieu.

M. Dupuytren commences with making five or six scarifications of the epigastrium, each of them sufficient to deplete to the extent of two or three ounces; he then directs the patient to be vigorously dry rubbed with flannel, after which he administers a cup of the decoction of poppy heads, which is to be repeated every two hours. Meantime the patient gets, every hour, a dessert spoonful of a mixture of subacetate of lead (50 drops), in an infusion of mint (8 ounces), with syrup. A *demi-lavement* of the decoction is also given every three hours. The dry rubbing to be constantly employed.

M. Magendie, in the stage of collapse, places his reliance on strong punch made with alcohol and infusion of camomile. In the stage of reaction, he gives, instead, a simple *tisane*; and if there be symptoms of congestion towards the head, he applies ice, or iced water, according to circumstances.

M. Recamier adopts the system of cold affusion, and begins with water at the temperature of 16° of Reaumur. His internal remedies are infusion of mint, and, at intervals of a quarter of an hour, a spoonful of a mixture composed of tincture of opium (ʒiss.), sulphuric ether (ʒi.), and infusion of mint (℥vi.), with mucilage. The patient is rubbed, moreover, with camphorated liniment, having a fourth part of tincture of opium mixed with it.

M. Breschet gives acetate of ammonia in camomile tea—an ounce to the pint; and he prescribes, also, sulphuric ether (30 drops), with tincture of canella and syrup of quinine, to be occasionally administered. Laudanum injections; and ammoniacal frictions.

M. Chomel.—Frictions with spirits and acetic acid. Laudanum, in the dose of 20 drops to the ounce of mint-water and syrup.

M. Gendrin doses largely *(à haute dose)* with opium, and employs spirituous frictions, &c.

They all agree, it may be added, in prescribing the constant use of frictions, with the application of hot bricks, bottles, sand, and iron; and, in short, the prevailing theory among the French practitioners seems to be, that cholera is a non-inflammatory complaint, which is to be best treated with stimulants.

PROPAGATION OF CHOLERA.

Whether cholera be contagious or not is a point which may admit of reasonable doubt. For ourselves, we remain of our former opinion—that its history, on the great scale, would lead us to answer in the affirmative; but we confess that, in London, the evidence of contagion has often been entirely wanting. Probably the disease may exhibit the contagious principle more in some places than in others. We request the reader's attention, as connected with this subject, to the extracts which we subjoin—one from an interesting and instructive work on cholera, recently published by Dr. Haslewood and Mr. Mordey, giving an account of the disease as it prevailed in Sunderland; the other from official documents lately edited, in which the chief proofs in favour of contagion are brought together. These, and particularly the circumstance of so many medical men, at St. Petersburgh, having taken the disease, appear so conclusive, that we have heard only one mode of getting over it attempted—namely, by denying the facts. We thought, before we alluded to the subject, that the fairest way would be to communicate the doubt to one of the parties on whose authority the statement was made; and, accordingly, a gentleman connected with this journal having applied to Sir David Barry, he at once furnished the names of the deceased, with the memorandum which follows:—

The following are the names of the physicians and apothecaries who died of cholera at St. Petersburgh during the epidemic, from the 29th June to the 10th of October, 1831, according to the lists obtained from Sir James Wylie, head of the military medical department, and from the office of Dr. Rehmann, head of the civil medical department of Russia:—

Doctors—

Moudroff.
Elensky.
Sopelovitch.
Giggler.
Kistzer.
Saccovich.
Palchefsky.
Schultz.
Albrecht.
Meissner.
Blank.
Bremner.
Cantlemiroff.
Titerin.
Retz
Rehmann.
Souschensky.

Apothecaries—

Schevinson.
Kaltsmann.

(Signed) D. Barry.

HASLEWOOD AND MORDEY ON THE CONTAGION OF CHOLERA.

With regard to the first origin of this disease, the contagionists profess themselves to be as much in the dark as we confessedly are as to that of any of our acknowledged contagious fevers. From the facts stated in a tabular list of cases which came under our observation, we have no doubt that the disease has propagated itself in this town, and from this to neighbouring places, accordign to the laws of contagion. For, in the first place, in the great majority of cases direct communication was traceable. In most instances more than one person suffered in the same house, and these were attacked in successive days. This fact would have appeared still more obvious if the cases which were treated in the preliminary stage had been noted. Many persons took the disease after having been employed about the dead bodies; and it would appear either that the emanations from the dead are more actively infectious, or that mental affections have rendered the persons employed peculiarly susceptible.

2dly, To many of these cases it may be objected, that the subjects were residing in situations where the cause of the disease, be it what it might, was known to be active. To the following, however, the objection does not apply; the persons attacked never having been in the affected districts, but having taken the disease after communicating with those who had been removed from such situations.

On the 31st of October, two persons died of cholera. —— Rodenburgh in Monk Wearmouth, and Sprout in the Infirmary fever-ward. Both bodies were examined the following morning; the former by Dr. Haslewood, Mr. Torbock, and Mr. Mordey; the latter, in the presence of those persons and others, by Mr. Penman.

The nurse of the Infirmary, who assisted in removing the body of Sprout to the dead-house, and had no other communication with the fever-house, was attacked nine hours afterwards, and died.

Mr. Penman (who wounded himself in the dissection) was attacked the same evening with sudden giddiness and faintness, while walking; and with great difficulty got home. This was succeeded by violent watery diarrhœa, with cramps, and great prostration of strength. He recovered under the use of brandy, opium, and calomel; but continued in a state of low fever for several days.

On the 6th of November, the wife of Dr. Haslewood was attacked with cholera: the case is given in a former part of this work. She communicated with her husband on his return home.

Mr. Torbock was attacked with the disease on the same day.

A woman named Haswell was attacked on the 19th of November. She resided in the west end of Bishop Wearmouth: some of her connexions lived in the low part of Sunderland, but it is not clear in what way she took the disease. She was attended by her daughter, and a Mrs. Cummings, both of whom lived in Bishop Wearmouth. The daughter was attacked with diarrhœa, Mrs. Cummings with cholera, the disease commencing with sudden faintness.

Mrs. E. mother of a surgeon who was in constant attendance on cholera cases, resided in the house of her son, and never went out. She took the disease, and died.

The person who washed the clothes, &c. used by this patient, took the disease.

The cholera hospital is situated to the south of the town, in an airy, open situation; the wards were large and lofty. In this establishment, four females were employed as nurses and washerwomen; all, however, occasionally employed in administering to the patients. Of these, the principal nurse, who was indefatigable in her attention, took the disease in its worst form, and died in eight hours. Another, Elizabeth Snipes, was attacked with diarrhœa, and other preliminary symptoms. She recovered; but three days afterwards committed an error in diet, was attacked with cholera, and recovered.

The porter of the hospital was attacked with diarrhœa, and great nervous agitation: he quickly recovered under proper means.

The disease commenced at Houghton-le-Spring, in the person of a woman who had come from Sunderland, and had been travelling about the neighbourhood as a hawker.

The first case in North Shields was that of a vagrant.

From the facts above stated, which are in conformity with the great preponderance of testimony from the continent, and especially with that contained in the very interesting letters of Dr. Becker, concerning the facts observed at Berlin, we can no longer doubt that the disease propagates itself in strict accordance with the known laws of contagion.

"*Facts collected in St. Petersburg, during the late Epidemic, illustrative of the spread of Cholera amongst persons employed about the Sick of that Disease in Hospitals.*

"*July 12th. Merchants' Hospital.* — Superbly fitted up; some of the rooms small, and not freely ventilated.

"Attacked—one purveyor, two feltchers or barber-surgeons, four servants—one dead.

"*13th. Hospital of the Semenoffsky Regiment.*—Attacked by the disease—three feltchers, seven servants; two dead.

"This hospital took in civil as well as military sick, towards the middle of the epidemic. The whole number admitted, three hundred and fifty-two.

"*21st. Aboucoff Summer Hospital,* convert-

ed into a temporary cholera hospital.—Servants attacked, eight; died, three.

"*24th. Cholera Hospital at the School for the Sons of the Clergy.*—Of eight servants employed, two attacked.

"*August 9th. Hemp Merchants' Hospital.*—Of twelve servants employed, three attacked—two dead.

"*12th. General Military Hospital, Vibourg quarter.*—Physicians, three attacked—one died. Servants, twelve attacked—four died. Of twelve medical students employed *pro tempore*, all had diarrhœa and other slight symptoms.

"This hospital, at first purely military, and in the most perfect state of cleanliness and discipline (as, indeed, all the Russian military hospitals are), had few or none of its attendants taken ill. It was only after it had begun to admit civil cholera sick, and had become somewhat crowded, that the above casualties took place.

"*14th. Naval Cholera Hospital.*—Dr. Seidlitz, chief physician, states, that of forty-two attendants (two physicians) none were attacked. This hospital is composed of two detached buildings, standing in the middle of a field of about two hundred yards square, perfectly ventilated, and unembarrassed by other buildings on any side.

"*15th. Cholera Hospital of the Foundling Hospital.*—Of forty-two attendants, fifteen were attacked, four feltchers included, of whom three were seized.

"*Hospital for the Imperial Stables at St. Petersburg.*—Sick admitted, seventy-seven: of seven servants employed, three were attacked.

"*September 10th. Rogistevensky Hospital* established in two inconvenient houses.—Physicians, five, and all the attendants of every description attacked.

"Of two hundred and sixty-four physicians, the whole number in St. Petersburg during the epidemic, above forty were attacked by cholera, of whom nineteen died."

MR. LIZARS AND THE EDINBURGH "BOARD OF HEALTH."

In consequence of some observations contained in a pamphlet lately published by Mr. Lizars, of Edinburgh, and regarded as intended to apply to the Board of Health of that city, the author was requested to attend a meeting of the members; at which an angry discussion took place, ending in Mr. Lizars' resignation, and followed by the publication, on his part, of a lengthened statement of the transactions, in the newspapers. We must say, that some of the remarks in Mr. L.'s pamphlet appear to us calculated to impede the operation of those arrangements for the prevention of cholera which have reflected so much credit on the northern metropolis; but we cannot enter into the controversy.

DR. HALMA-GRAND.

We have received Dr. Halma Grand's letter, but cannot make room for it. He is considerably indignant with us, first, for accusing M. Delpech of having mistaken something else for the semilunar ganglion; and second, for stating that he had communicated to the *Académie de Médecine*, the *discovery* regarding the ganglionic nerves as his own. The former rests on the authority of Mr. Harrison, the well-known anatomist of Dublin, and of others, who were present; the latter on that of the Editor of the *Gazette Médicale* of Paris.

REPORT OF CHOLERA IN GREAT BRITAIN, UP TO FRIDAY, APRIL 6, 1832.

New cases in London since our last report	429
Deaths	233
Total number of cases in London since the commencement of the disease	2158
Deaths	1148
Total number of cases throughout Great Britain since the commencement of the disease	9574
Deaths	3618

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

March 1832.	Thermometer.		Barometer.	
Thursday . 29	from 23 to	54	29·90 to	29·95
Friday. . . 30	29	54	29·93	29·88
Saturday . 31	35	53	29·82	29·72
April.				
Sunday . . 1	36	51	29·74	29·89
Monday . . 2	28	59	30·06	30·15
Tuesday . 3	29	63	30·34	30·41
Wednesday 4	29	63	30 44	30·50

Prevailing wind N. E. Weather remarkably fine and clear, except on the first instant.

Charles Henry Adams.

NOTICES.

The papers of the Messrs. R., of L. and W., shall speedily be arranged for publication. We thought it better to keep them over for our new volume.

Mr. Wayte's letter has been unavoidably deferred.

Our Worcester correspondent will, we are sure, have excused us for not publishing his last paper. The points on which he touches have long since been made familiar to the public, and would seem to be scarcely susceptible of additional elucidation.

Mr. Greetham's paper is in hand.

Mr. Hamerton's subscription of 1*l.*, for the family of the late Dr. Nuttall, has been received.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 14, 1832.

LECTURES ON THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Part I.—Lecture XXVII.

Continued Fever.—Symptoms.

In continued fever, gentlemen, the general symptoms are the same as those I described as the constitutional symptoms usually accompanying local inflammation—the same symptoms which occur constitutionally in any intense local inflammation are those which, generally speaking, characterize continued fever. There is quickness of pulse, increased heat, diminution of the secretions. From the latter circumstance we have thirst, dryness of the skin, scanty and high coloured urine, and costiveness. There is usually restlessness and watchfulness. At first, as at the beginning of inflammation, and as in intermittent fever, there are the opposite symptoms of chilliness, even rigors; coldness, paleness, smallness of pulse; but these last in general only for a time. Andral says that chilliness rarely occurs on the first two or three days, according to some observations which he made on 229 cases of fever at Paris, in 1822. However, I have been attentive to the circumstance, and I know that people at the very beginning of continued fever in London, do frequently complain—more frequently than not, of chilliness, and those symptoms which are analogous to the first stage of intermittent fever.

If, however, these symptoms do take place—rigors, chilliness, and smallness of the pulse—they last usually but for a short time, and soon yield to symptoms of excitement, which continue throughout the disease, though perhaps after a time they are united with symptoms of great debility, just as is frequently the case in inflammation.

The general symptoms of continued fever vary from those which characterize an active inflammation down to those which accompany an atonic or passive inflammation or mortification; so that you have symptoms of activity and strength, and all the intermediate shades and degrees, down to symptoms of extreme prostration of strength, and even a disposition to putrescency in the fluids of the body.

From the first, in an attack of fever, there is more affection of the head than when the constitutional symptoms of mere inflammation occur. There is generally at first more or less confusion, giddiness, drowsiness; perhaps even stupor, or watchfulness. There is generally from the first pain of the loins, with a complete loss of appetite and a general sense of debility. The countenance, too, in fever is almost always expressive of heaviness and anxiety. The countenance of continued fever is exceedingly characteristic; the patient is at once very heavy in his look, and evidently labouring under a degree of uneasiness at the same time. Sometimes, under these circumstances, the face is pale, but still there is the heaviness and the anxiety. Sometimes the face is flushed, and the eyes look red, and there is frequently great heat of the head; the vessels feel to the patient to throb, and they feel so likewise to the medical attendant. There then is great pain usually felt in the forehead. The tongue is generally tremulous, whatever its appearance as to dryness or colour may be. The extreme feeling of weakness, and the aching of the loins, are no less characteristic than the countenance in the beginning of this disease.

When those symptoms which I have now mentioned occur quickly and acutely, they shew the existence of continued fever, more especially if there be the morbid appearance of the tongue, heat, quick pulse, and thirst, out of all proportion to any signs of local inflammation that may exist. Under these circumstances we consider that the patient has fever in the proper sense of the word—idiopathic fever—fever as distinguished from

mere pyrexia. Local inflammation frequently co-exists evidently at the same time, but whether there is always local inflammation present or not, is a theoretical question upon which I will not now enter. These symptoms, however, continually occur without any evidence of local inflammation, and are continually out of proportion to any that we may observe, and it is this circumstance that makes us consider the person as labouring under fever, properly so called.

Temperature.—As to the particular symptoms to which I have just now alluded, the heat is sometimes intense; the temperature of the body will sometimes rise to 104°, 108°, or 110°, and sometimes it is of that peculiar character which authors have denominated *mordant*—a pungent, or biting heat. Galen, Sir J. Pringle, and Sir G. Blane, all speak of mordant heat. Galen, when treating of autumnal remittent fever, says, that the great mark of it is the mordacity and acrimony of the heat, which erodes the touch, just as smoke does the nose and eyes. Maximum indicium est mordacitas et acrimonia caloris, quæ perinde ac fumus nares et oculos, sic ipsa erodere tactum videtur. This peculiarity of heat is not felt, he says, the moment the hand is applied, but is perceived on continuing the hand upon the patient for a certain time. Sir Gilbert says, that in ship fever there is a peculiar heat of the skin—a glow of heat imposed upon the palm of the hand of the practitioner who has grasped the wrist of the patient, and lasts some hours, if the hand is not washed sooner. He adds that he never saw this in the sporadic fevers of England, though he has been informed of its occurrence. Andral also mentions the same circumstance. He says, that in the continued fever of Paris the heat of the skin was in one case very high, acrid, and mordant, leaving a sense of heat for some time upon the hand of the practitioner —a sensation, he says, very nearly allied to pain. La chaleur de la peau étoit devenue très elevée, acre, et mordicante: en laissant quelque tems le doigt en contact avec elle, on eprouvoit une sorte de sensation penible, voisine de la douleur. Sometimes, however, in fever the heat is not increased, or if it be increased it is only partially, and varies in its degree at different times. Occasionally, in fever the temperature is below what it ought to be, and in the last stage you have sometimes absolute coldness.

Pulse.—The pulse, like the temperature, is generally increased. The pulse in continued fever may be full or hard, soft, small, or weak, or easily extinguished. Like the temperature, it may be of all degrees. As the temperature may be from 110° down to far below the natural standard, so the pulse may be quick, full, hard, and strong, or it may be more or less slow—it may be so feeble as to be nearly extinguishable—to be what is called a fluttering or a vermicular pulse, according to the tonic or atonic character of the disease, and according to the stage of the affection. In examining the pulse, if there be signs of debility, and the pulse be strong, we ought to examine the actual state of the circulation at the heart itself. We ought, if we be in any doubt, to resort to that method, for occasionally there may be the extreme debility of fever, with rather a strong pulse. The heart may be in a state of disease which you may not be aware of, in consequence of not having seen the patient before his present illness. The heart may be greatly thickened; and although it may be acting less forcibly than it did before the attack of fever, yet it may act from its thickness so as to produce a full pulse—such a pulse as might incline you to adopt active measures.

The quickness of the pulse may amount to about 200. Of course you cannot count such a pulse as this at the wrist, but you may count it with perfect ease at the heart itself. The usual range, however, of the pulse in continued fever is from 90 to 160. Occasionally it is not quick, just as the heat occasionally is not increased; nay, it is sometimes even slower than it should be. This takes place chiefly when the head is very much oppressed, and it occurs therefore sporadically from the state of the head, or from some peculiarity in the individual's constitution. It has, however, occurred epidemically: fevers have occurred epidemically in which one of the characters was slowness of pulse. De Haen mentions a sporadical case of very severe fever in which the pulse was only 44. Sarconi, in his account of an epidemic which prevailed at Naples, states that the pulse was hardly more than 40 in a minute in some.

Occasionally in fever the pulse is regular, while in health it is intermittent: many such instances are upon record. Andral mentions a case of fatal fever where the pulse at first was intermittent, but as the symptoms grew worse and worse, the pulse became more and more regular, till at last it was perfectly so. Rasori, the celebrated Italian physician, mentions the case of an individual in whom the pulse was regular till the fever was over, and then it became intermittent. He therefore inferred that his pulse in health was naturally intermittent. De Haen also mentions the case of a man who had an intermittent pulse at all times except when he was labouring under fever. Monro and Shenkius mention such cases; and Dr. Heberden himself saw two persons whose pulse was always intermittent, except during illness. I may mention that Dr. Heberden knew a person with intermittent pulse all her life, and who at last died of cancer of the womb, but in whose circulating system an able anatomist could discover nothing unhealthy.

Skin.—The skin in fever is usually dry,

but as a favourable change takes place, it becomes more soft, and the moisture is for the most part general. Sometimes, however, in fever there are partial sweats, which are by no means favourable, and when death is near at hand, then the sweats are generally both cold and clammy. Sometimes the sweats are offensive, especially if there be great debility. Upon the skin are not unfrequently seen spots—discolourations of various sizes. If they be exceedingly small they are called *petechiæ;* if they be larger they are termed *vibices;* if they be still larger they are denominated *ecchymoses.* These spots are of various shades, from a tolerably bright redness down to a purple hue. They occur particularly in the last stage of the disease, where there is extreme debility, but sometimes they occur where there is not great debility. At particular periods continued fever is characterized by them, and I have seen a greater number of instances of this description within the last three months than I ever saw before in the whole of my life.

Tongue.—The tongue in fever is usually dry. With respect to colour, it may be white, or yellow and loaded; it may be merely of a whitish brown, or really brown, it may be absolutely black. It is brown down the middle occasionally, and has a broad white band on each side, the edges perhaps being red. You will sometimes see it of a reddish brown. Occasionally it is red, glazed, smooth, and dry, and not unfrequently, under these circumstances, it is cracked also. It is very common to see it more or less white or brown on the back, with redness at the tip, or redness at the edges. Occasionally it is extremely pale. When the tongue is of a reddish brown, when it is really brown, or absolutely black, you usually have likewise black collections about the teeth and lips—*sordes,* as they are called. They are the result, no doubt, of vitiated secretion, and sometimes they are partly the result of a little effusion of blood which coagulates, and uniting with the secretion, contributes to their formation.

The tongue, you will remember, is generally tremulous, and frequently the extremities are in a similar state—so that when the patient attempts to move, his hands or his legs tremble; but when they do not, you may generally observe a tremor of the tongue. From the dryness of the tongue, and perhaps of the fauces, the patient necessarily suffers thirst. The breath is frequently offensive—much more so than the perspiration.

Urine.—The urine is of course scanty, and it is high-coloured, containing an excess of the lithates and the purpurates—chiefly the purpurate of ammonia, but also the purpurate of soda—together with the yellow colouring matter which naturally exists in the urine. As the fever declines, the lithates become super-lithates, and are precipitated, so that you have a red sediment. The urine has a strong animal smell; and not unfrequently it has a strong ammoniacal smell, and almost as soon as it is discharged it rushes into a degree of putrefaction. Occasionally the urine is very dark coloured, and sometimes it is bloody.

Stomach.—With respect to the stomach, the appetite, for the most part, is lost; but just as there are extraordinary instances, from time to time, in regard to the pulse—such as being regular in fever, while it is intermittent in health—so, in regard to the appetite, there are cases upon record in which, so far from its being lost in fever, it was much increased. You will find a case mentioned by Dr. Satterly, in the 5th vol. of the Transactions of the College of Physicians, of a boy who laboured under typhus fever, attended by marked inflammation of the head. The exacerbations of the fever were always attended by a voracious appetite; so that, in the midst of the fever, he would eat four meals a-day, each meal being fit for a stout labourer. Besides these four meals of meat and vegetables, he ate many pounds a-day of dry bread, biscuit, and fruit. He had no sooner eaten a meal than he denied that he had eaten any thing, so that the more he ate the more he desired;

—— Cibus omnis in illo,
Causa cibi est, semperque locus fit inanis edendo.

and if he were not fed the moment he requested it, he sucked the bed-clothes and bit his fingers, in this also bringing to one's mind Ovid's account of Erisichthon:—

Ipse suos artus lacero divellere morsu,
Cœpit, et infelix minuendo corpus alebat.

This boy discharged several very copious stools a-day, and he perfectly recovered. The appetite, however, usually returns as the disease declines, and when the disease is first gone, the appetite is for the most part voracious. Persons say there is nothing the matter with them, but they are very hungry.

The stomach, however, is frequently much more affected than with loss of appetite; even from the very first there is nausea, and in many cases vomiting. Sometimes the vomiting does not occur from the beginning—does not make its appearance till the disease has existed for a length of time. The stuff which is vomited may be mere mucus, or it may be bile, or it may be like coffee-grounds; and sometimes the quantity is very considerable.

Intestines.—The intestines likewise suffer in this disease. There is often more or less costiveness, but very often there is diarrhœa. Sometimes the diarrhœa begins with the disease, and sometimes will not come on till it has existed for a certain period. The character of the stools is as various as the character of what is vomited; they may be very watery—they may be mucous, or, as the common people say, slimy. They may be, in

colour, yellow, greenish, white—they may be exceedingly offensive, and perhaps bloody.

Head.—The functions of the brain are almost always affected; pain, vertigo, tinnitus aurium, are very common, and sometimes there are convulsions, dullness, stupor, usually great depression of spirits, delirium at night, terrific dreams, and watchfulness.

Respiration.—It is said, with regard to respiration, that more oxygen is consumed in the hot stage of fever, during digestion and exercise, than in health, and more carbonic acid formed; whereas, in the cold stage, as indeed after bleeding, and in dyspnœa, less oxygen is consumed and less carbonic acid formed.

Favourable Progress.—Now the disease, such as I have described it, may have all the symptoms of mere excitement, with no remarkable debility. There is always a feeling of more or less debility, but there may be sometimes chiefly excitement, and only that debility which is inseparable from fever. The fever may be subdued, the morbid changes which I have mentioned to occur in the secretions and functions may decline, and nothing but weakness and a degree of emaciation may remain; and from these recovery may speedily take place. The emaciation, I may observe, is proportionally greater after fever than after any other acute disease.

Debility.—However, this may not be the case; signs of debility may appear, the breath may become very fœtid, as also the sweats and all the discharges, whether from the stomach, the intestines, or the urinary bladder. What is vomited may be like coffee-grounds; the urine may be bloody, and so may the fæces. Even blood may be found in the mouth, or it may be poured forth under the skin. There may be extreme blackness of the tongue, and a great quantity of sordes upon the teeth, lips, and every part of the mouth. All the fluids may be secreted in so vitiated a manner that they may putrify as soon as they are discharged, and the body may become putrid directly after death. One cannot conceive the possibility of putrefaction of parts still alive, nor of the secretions at the moment of their formation; but they are often as near putridity as is compatible with life, the latter putrifying the moment they are separated from the body, and the body itself, as soon as it is dead, will frequently rush into great putridity. The discharges are sometimes so offensive that perhaps the very house is intolerable.

Under these circumstances, the pulse is very rapid and very weak; there is extreme prostration of strength, and a death-like faintness complained of by the patient. The face becomes ghastly—in fact, you have the *facies hippocratica;* the intellect is greatly disturbed; there is an inability to attend to any thing; a troublesome hiccup; catching and working of the fingers, which is called *subsultus tendinum;* perhaps convulsions—at any rate, cold clammy sweats; an involuntary discharge of the urine and fæces. Still, the debility and putrescency may decline, and a high degree of these symptoms be recovered from.

Divisions of Fever.

If the symptoms be purely inflammatory, purely those of excitement, with good strength at first, and do not afterwards degenerate into debility, or at least not into considerable debility, the disease is called *synocha.* If they degenerate into great debility it is called *synochus.* If, from the very first, great symptoms of debility appear, it has been called *typhus.* These are arbitrary names, the two first having the same etymology, but they do very well to express different characters of the same disease in different individuals. When it is called typhus, it is the same fever as many authors write of under the name *ship* fever, *hospital* fever, *gaol* fever, *putrid* fever, *adynamic* fever. If the symptoms be very severe indeed, then it is called *typhus gravior;* but if they be mild, though it even prove fatal, it is then called *typhus mitior*—typhus being divided into two varieties. There is every variety in continued fever, both as to the degree of excitement and the degree of strength; from the very highest excitement and a high degree of strength, down to the most absolute prostration that can be present, and from no putrescency up to a high degree of it.

Local Affections.

The parts most dangerously disturbed in fever are the head, the chest, and the abdomen; and the relative proportion of disturbance in these different regions is exceedingly various;—the head and the abdomen, however, suffer the most of the three, except where there is a particular epidemic character, or an individual predisposition to, or a local cause of excitement in, the chest. With these exceptions, the head and the abdomen suffer far more in continued fever than the thorax. It is in hot climates and in hot seasons that the abdomen is most affected.

Now the local disturbance, when very great, is usually of an inflammatory kind; at least it is usually inflammatory at first. But as there all degrees of the general affection, from tonic to atonic, so the local affections may be both of all degrees of violence, and vary from active tonic inflammation down to mere irritation or extreme loss of power.

Head.—To begin with the head. The local affection there, when very great, may give rise, as I have said before, to drowsiness, headache, convulsions, vertigo, watchfulness, tinnitus aurium; but it may amount to a higher degree than this, so that you have violent headache, such as distracts the patient; violent throbbing, which is visi-

ble when you look at the temples; intolerance of light; redness of the eyes, and violent delirium. The delirium may be constant, and it may be such as to require corporeal restraint. There may be loud, incessant, incoherent ravings; an ignorance of persons and things which before were perfectly familiar to the individual, and yet, in the midst of this, a person may be rational and sensible for a single moment; he may be momentarily recalled by some circumstance, but he is instantly off again. At the same time, there is a picking of the bedclothes, and tremor; and delirium of this description is called *delirium ferox.* On the other hand, the delirium is sometimes not of this ferocious character, but the patient mutters to himself; there are altogether slighter symptoms of disturbance of the head, and it is then called *delirium mite.* Occasionally, where there is great debility, there are no symptoms of vascular excitement in the head, no headache, no intolerance of light, no redness of the eyes, nor throbbing of the temples, but merely muttering delirium. Sometimes there is great stupor, and a comatose state, with or without symptoms of an inflammatory disposition in the head.

Stomach.—As to the stomach, there is almost always anorexia, vomiting, purging, or costiveness, but these symptoms are sometimes extreme. There shall be copious and intense vomiting of every thing that is taken, and of fluids of all descriptions. There may be violent diarrhœa, of all kinds of characters. The abdomen may be excessively tender to the touch, and even painful on slight pressure; it may be exceedingly distended; it may be painful, especially at the epigastrium, or in the hepatic region. There may be a sense of burning in the abdomen, more particularly at the epigastrium; it may be felt likewise up the throat, and be attended by extreme thirst. When the symptoms in the abdomen run high, they are, extreme vomiting, extreme purging, extreme tendernsss, perhaps violent pain even when compression is not employed.

Tongue.—The state of the tongue has been supposed to corrrespond with the state of the alimentary canal. Now the tongue is certainly often red, perhaps generally, either throughout, at the edges, or at the tip, when the internal coat of the stomach is inflamed; but the agreement in the two organs is not constant. The tongue may be red in fever and in other diseases, without any corresponding state of the stomach, at least without any evidence of it, and therefore the alleged universal correspondence is a mere assumption. A blackness of the tongue is rather indicative of the general debility of fever than of a gastric affection. Thirst may be commensurate with a gastric affection, but it may arise simply from the intensity of fever drying the fauces. Again, ulceration and inflammation of the stomach or intestines have been found, where the tongue had not been red during life, and it is said that neither they, nor any abdominal inflammation, have sometimes been discovered where there had been much pain of the abdomen experienced on pressure. Purging may be very violent without inflammation of the mucous membrane, which may be found healthy in consistence, and even pale, just as we have profuse sweating under various circumstances, without any inflammation of the skin. It is not a necessary circumstance that the tongue should indicate the state of the stomach or of the intestines; there is a certain correspondence between them, but it is by no means invariable; neither is it a necessary consequence that, when we see great irritation of the stomach, and great irritation of the intestines, that there should be inflammation. Frequently after such circumstances have occurred, inspections have not shewn that inflammation existed.

Thorax.—As to the chest it is commonly a little affected; in general we have the breathing more or less quickened, and there is a little cough; and, generally, if you employ the ear or the stethoscope, you will find in fever some degree of sonorous, or sibilous, or mucous rattle in the lungs. The symptoms may not be such as to attract the attention of the patient, or the practitioner, or to demand any measures; but if you listen with the stethoscope, I believe you will generally find the mucous membrane of the bronchiæ more or less affected in the disease. Sometimes, however, the respiration is very much affected; it becomes very rapid; there is violent cough; pain in the side, or at the front of the chest; great rattle; copious expectoration; and all the decided signs of bronchitic, pleuritic, or peripneumonic affection; so that at last there is blueness of the lips and cheeks, from congestion of the lungs. I believe that all along there is more congestion in the lungs than actual inflammation. Affection of the chest, though it generally exists, is far less frequently of an inflammatory nature than affection either of the head or abdomen; and of these two the abdomen suffers more.

Blood.—If we examine the blood we may find it buffed, perhaps also cupped, but frequently it is quite natural. Sometimes the coagulum is exceedingly loose, corresponding with the debility, and frequently it soon putrefies. In the typhoid stage of exhaustion and debility, it has been found lately that the chemical characters of the blood are very depraved—that it abounds more and more in serum, and less in its other constituents. As the disease is more and more characterized by debility, the blood at last resembles merely fibrin and serum; little or no chyle is formed, and it is deficient in carbonic acid.

and in saline materials. Dr. Clanny, of Sunderland, pointed out this circumstance, and Dr. Stevens made similar observations, which he is about to publish, and he made them in a distant country without knowing what Dr. Clanny had done, so that we have the unbiassed observations of two physicians unknown to each other.

Eyes, Throat, &c.—Besides those particular parts that I have now mentioned, others are sometimes very much affected; sometimes the eyes will become inflamed, and also the throat, bladder, or skin. Sometimes we have an eruption of minute vesicles of the size of millet seeds—sometimes we have large patches of inflammation—sometimes we have irregular pimples, papulæ—occasionally we have a great discharge of blood from the intestines and urinary passages, and perhaps all without any correspondent severity of the general symptoms. Sometimes we have inflammation of the parotid glands, ending in abscess; occasionally abscesses will form in different parts, and sometimes a great crop of boils will take place. Now and then mortification of some part occurs—not general mortification within the head, chest, or abdomen, the parts which are affected with inflammation in these cases—but of the extremities or of the loins. The debility is such in this disease, that from a patient lying long on his back the loins are much disposed to mortify; and so may one or both hips. There being universal debility, the effect of pressure is not resisted as it is when we are in health. If we lie upon a part for any length of time in health, it will not produce mortification; whereas persons in the debility occasioned by fever, from lying on their back soon have pressure sufficiently impede the circulation to cause mortification. After fever the mind will sometimes remain dull, or even imbecile for a length of time, sometimes for several months, and sometimes there is great depression of spirits, which the patient, although in other respects pretty well, cannot shake off. Sometimes after fever a hand or a foot, or both hands and both feet, or a whole extremity, will remain for a longer or shorter time bent, and little influenced by volition, so that a person is indeed perhaps a cripple for life. I have seen several persons whose hands or whose feet have been bent up in this way after fever, and this has ceased after a time; but I have seen others in whom the circumstance had continued many years after fever, and still continues.

Duration of the Disease.—The duration of the disease, whether active and inflammatory, or whether of great debility and even putrescency, may exist from one or a few days to several weeks, but it rarely extends beyond eight or ten weeks. Occasionally the disease will end with a discharge of blood, either from the nose, the intestines, or other parts, or with purging or sweating. Dr. Gregory said that he once knew a fever terminate by a great discharge of healthy urine. Andral says that he once saw a fever terminate with a profuse expectoration, and once with an alternation of sweating and expectoration. Suppuration after the disease, a general crop of petechiæ, and even the emaciation which follows the disease, you will find mentioned in authors as happy terminations—as critical circumstances which were closely connected with the happy result of the affection.

Critical Days.—Now these discharges, when they occur, are called *critical*, and the amendment is called a *crisis*. The ancients imagined that a crisis occurred particularly on certain days, and hence those days were called critical. During the first eleven days of fever these critical days were of a tertian character, so that the third, fifth, seventh, ninth, and eleventh days, were critical days, and supposed to be those on which the disease was disposed to terminate well. After the eleventh the type was supposed to be quartan, so that the fourteenth, the seventeenth, and the twentieth days, were then supposed to be critical. If a change took place, it was supposed to be most favourable and most complete if it occurred on these days; it might happen on other days, but then it was supposed to be less favourable. Some say that the testimony of Hippocrates and Galen among the ancients, and of Dr. Fordyce, Dr. Stoker, and Dr. Percival, among the moderns, in favour of these critical days, is too great to be withstood; but others imagine that it was a mere hypothesis, founded upon the Pythagorean doctrine of numbers, or that it was taken from the types of intermittent fever, which the ancients supposed continued fever must very much resemble. De Haen says, that of 163 terminations of fever mentioned by Hippocrates, 107—that is, two-thirds—took place on the critical days, viz. the 3d, 5th, 7th, 9th, 11th, 14th, 17th, and 20th; that none took place on the second or thirtieth; and only 18, *i.e.* one-ninth, on the other non-critical days, viz. the 8th, 10th, 12th, 15th, 16th, 18th, and 19th. Certainly the only types of fever now talked of here are the fourteen and twenty-one day; so that the common people will say that a patient has a fourteen or twenty-one day fever. Perhaps the vulgar, however, only retain notions which formerly prevailed among physicians; physicians may have inherited these notions from the ancients; and so they may have come down to us, and still linger among the vulgar. The remark may be correct; some persons say they do observe these things; but I cannot say that I have ever noticed the disease to terminate on one day in preference to another. The reason that we do not observe what the ancients did, is said to be, that we are more active in practice than they were.

We do not let nature take her course, but endeavour to knock a morbid process on the head; and we cure far more cases of fever than they did. We allow diseases to run on a much shorter period than they were accustomed to do; and some ascribe the want of critical days to our active practice. To shew the inefficacy of the practice of the ancients, I may mention that Broussais asserts that Hippocrates, in the first and third sections of his work on Epidemics, gives an account of thirty cases of acute disease; sixteen of the patients died, and the other fourteen suffered much afterwards. It must have been bad practice to lose sixteen acute cases out of thirty, unless in particular circumstances, such as in the eastern epidemic, improperly called cholera morbus. Sir Gilbert Blane says, that in the first and third sections of Hippocrates, forty-two cases of acute disease are mentioned, thirty-seven of which were continued fever without local affection, and five with local affection; of these twenty-five died, *i.e.* twenty-one of the thirty-seven, and four of the five. If nature had an inclination to perform her cures on certain days, she had there a fine opportunity; but at the present day we so interrupt her course, that I confess I have never observed critical days.

OF THE

AFFUSION OF A STREAM OF COLD WATER ON THE HEAD,

In the Treatment of Various Diseases.

By James Copland, M.D.
Consulting Physician to Queen Charlotte's Lying-In Hospital, Senior Physician to the Royal Infirmary for the Diseases of Children, &c.

It is my intention to confine myself at present to the consideration of the practice of affusing cold water on the head as a means of cure. The affusion of tepid and warm water on the head in certain diseases may afterwards engage my attention, if the subject be found sufficiently important; and I may, lastly, notice the advantages to be derived from affusion on other parts of the body, in various affections, and offer some remarks on the practice of affusion over the whole surface of the body—on the temporary vogue it at one time enjoyed—on its almost total exclusion from practice at the present day—on its advantages as a therapeutical agent—and on the states of disease which especially indicate the propriety of having recourse to it, as well as those which forbid its employment.

In respect of the practice of affusing cold water on the head in disease (to which form of affusion I must be understood as limiting my observations on the present occasion), I may state, that my attention has been directed towards it in practice since 1815; and that, during the last ten years especially, I have had very numerous opportunities, particularly in public practice, of employing it. In one institution I have frequently prescribed it in from ten to fifteen cases at a single visit; and at the moment of my writing this, I find that there are thirteen cases amongst the patients of that charity for which the cold affusion to the head forms one of the means of cure prescribed. I state this, lest my remarks may be viewed as proceeding from a partial and imperfect experience of the means recommended, instead of being general results, derived from long observation of its effects, and from numerous cases in which it has been employed. Being, moreover, altogether convinced of the great advantage of the practice, I think it right to place the results of my experience respecting it before the profession. Before, however, I proceed to state the practical conclusions at which I have arrived, I will premise a few observations as to the operation of this treatment, and add some directions as to the mode of employing it, so as to secure its good effects, under the very opposite circumstances in which it will be found beneficial.

I.—*Of the Mode of Operation of the Cold Affusion on the Frame, and of the intentions which it is calculated to fulfil in practice.*

When cold water is affused on any part of the surface of the body in health, a very evident constriction of the capillaries, and especially of the venous ramifications, of the part, with great diminution of the temperature, is soon produced. If the affusion of water, of from 35 to 55 degrees of F., be continued for even a short time, the temperature of the part will be very much reduced, and soon fall somewhat below that of the water which is affused. When the affusion is made in a continuous stream, and from a considerable height, this effect is more quickly produced, and more permanent in its duration, occasioning a very smart shock to the nervous system, followed by distinct chill, horripilations, and rigors, especially if the affusion be continued for a minute or two, or longer, and when the water is at a temperature but little above the freezing point. When thus

employed, the constriction of the surface, and of the cutaneous vessels, particularly the veins, first observed in the part on which the water falls, soon becomes general. But this constriction of the vessels is not gradually diffused from the part on which the column of water falls, but is sympathetically and rapidly induced throughout the circulating circle. The shock occasioned by the affusion, and the consecutive horripilations and chills, prove that its impression is immediate and powerful upon the nervous system of organic life.

That the cold affusion should thus immediately affect the nervous system of organic life, and, through this system, the blood vessels, is what *à priori* reasoning, founded on intimate views of the mutual relations of the nervous and vascular systems, would lead us to adopt. The conformation, intimate connexion, and mutual dependencies, of these systems, as well as their functions in health and disease, fully prove that causes acting either as depressants, or as excitants, of one part of the nervous circle of organic life, will soon have their effects propagated throughout the whole. We know that this nervous system is peculiarly organized and distributed; that it is devoted to those organs which are employed in digestion, assimilation, circulation, and secretion; that it extends itself from the centre of the body, generally in the course of the blood vessels, to all assimilating and secreting organs, forming peculiarly modified ganglia suited to the functions of each important organ, and extending minute ramifications in the course of the blood-vessels to the periphery of the frame; and that, not only are the assimilating, circulating, and secreting functions, under its control, but also those of generation and the production of animal heat. Hence it must be obvious, that impressions primarily made upon a portion of this important class of nerves—upon that part of the organization which is most intimately related to the manifestations of life, will produce co-ordinate effects upon the whole system, and mediately upon those parts which this system actuates or influences in health and in disease *.

The first effect or impression occasioned by the application of cold to any part of the body is depressing, as respects the state of the organic or vital nerves; the next effect, in consequence of the depression of vital influence of these nerves, is constriction of the vessels, and particularly of the veins. When the application is continued for a short time, and only to a part of the body, reaction soon follows; but if it be long continued, relatively to the state of vital action of the part, very injurious effects may follow, from the depression becoming too great for the powers of the constitution to remove, or predisposing to organic changes, as inflammation, &c. when reaction supervenes. These effects of cold, when directed to a part merely of the body, are more prominently evinced when the depression of temperature is occasioned by the cold affusion; and not only are these effects more marked, but they are attended with certain peculiarities, not readily appreciated or described, excepting in their relations to disease, as ascertained by careful observation. These peculiarities can scarcely be ascribed to any other cause than to the impulse with which the stream of water is made to fall upon the part, to the influence of this impulse on its nerves, and to the very quick transfer of its heat, owing to the rapid renewal of the cooling body. When the stream of water is considerable, and falls from some height upon the head, the effect upon the nervous system is often very remarkable, and approaches more nearly than any other phenomenon with which I am acquainted, to electro-motive or galvanic agency.

The effects produced by the affusion of a stream of cold water on the body are varied in a very singular manner, according to the thickness of the stream, and the height from which, and impulse with which, it falls; and they are most remarkably increased when directed upon the vertex of the head. I believe that a stream of cold water applied in this situation, even for a few minutes, would produce nearly fatal syncope in a healthy person; and I know that it would even occasion the utmost depression in a patient labouring under phrenitic mania. Perhaps the intensity of the effects of this agent, when directed to the head, can scarcely be explained without adverting to, and keeping in recollection, an opinion which I have long entertained and proved by anatomical research. This is the intimate connexion of the pituitary and pineal

* See my remarks on this subject in my Annotations and Appendix to M. Richerand's Elements of Physiology.

glands with the ramifications of the ganglial or soft nerves passing into the encephalon. The analogy which these glands present to the ganglia supplying other viscera, and situated in other cavities, in respect of structure, position, and relations to the organ which they supply; the fact that the chief ramifications of the ganglial nerves which enter the cranium proceed directly to them; the manner in which they are protected from injury, and from extension of disease from surrounding parts; and the fatal consequences of injuries and diseases affecting them, have long since convinced me that they are parts of the ganglial system, modified in their organization, according to the peculiar functions they have to perform, and placed in intimate connexion with the brain, in order to distribute the manifestations of organic life to the centres of animal relation, volition, and intellection. If this view of these parts be adopted, we may infer that the brain is related to the ganglial system in a manner similar to other viscera inclosed in the large cavities of the frame; and that agents which affect the conditions of this system, either directly or mediately through the vascular system, will have a more marked action, from being directed as near as possible to the part of the body which these ganglia, or organs of vital reinforcement, supply.

In whatever way this may be viewed, there can be no doubt that the affusion of a stream of cold water directed to the head produces very decided effects, both in health and disease,—effects which cannot be fully explained without some difficulty, and which vary considerably according to the position of the body whilst the affusion is being applied, to the temperature of the water, and to the height from which it is made to fall. If the person be erect, and the temperature of the water about that of newly-drawn spring water in this latitude, or but little below it, the effects already described are soon observed. The countenance becomes paler, the whole head colder, and the carotids beat much less forcibly and more slowly. If the affusion be continued even for a very few minutes, but at various periods, according to the state and constitutional powers of the individual, leipothymia, or even full syncope, will be produced, from the diminished afflux of blood to the brain.

Such being the effects of the affusion in health, its influence in certain diseases may be understood. When the patient is affected with congestion of the vessels of the encephalon, without serous effusion, occasioning, from the interruption to the circulation, and the benumbing influence of the pressure thereby produced, stupor, coma, &c. as observed after poisoning from narcotics, &c. the constriction of the blood-vessels, occasioned by the affusion, and sympathetically propagated to all the vessels of the encephalon, by means of the nerves, which, proceeding from the same ganglia, supply these vessels, removes the pressure, and at the same time the obstruction that the engorgement of the veins and sinuses produced. The return of blood being facilitated, and pressure removed, either reaction readily follows, if the affusion be not too long continued, or the vessels are gradually restored, without reaction, to a healthy condition. In all cases, therefore, of this description, when the object is to remove congestion, and particularly if the congestion partakes largely of the passive character, the affusion should be for a very short time, although it will be generally requisite to repeat it oftener than once, after short intervals. In every case the temperature of the head, and the action of the carotids, should be made the guide as to the manner of resorting to it. If the temperature and action of the carotids are much depressed, the affusion should be of short duration, and followed by the horizontal position. The head should be rubbed with a coarse towel, made dry, and warmly covered. These means will generally restore the healthy circulation of the encephalon, if accompanied with suitable internal remedies. If but little or no benefit result from the first affusion, a repetition of it, conducted in a similar manner, after one or more hours, or shorter or longer periods, according to the urgency of the case, will be necessary.

If the temperature of the head, and action of the carotids be increased, the affusion will be required for a longer period, and should be continued until both the one and the other are very remarkably diminished. When this state of the head proceeds from inflammatory action, it will generally be requisite to preserve the depression of temperature

and vascular action acquired by the first affusion, and prevent the supervention of reaction, either by repeated affusions upon the appearance of returning action, or by cold evaporating lotions applied to the head immediately after the affusion, and continued according to circumstances, or by both. The object to be kept in view in cases of this description, is very different from what is desired in cases of depression and congestion of the nervous and vascular systems. In these latter, reaction of the nervous forces and removal of congestion, are required; in the former, inordinate action is to be suppressed, and prevented from returning. These very opposite intentions, however, will generally be attained either by the practice now being explained, conducted so as to affect the vital energies in a different manner, or by a judicious combination of other means with it, so as to insure its effects. Whilst affusion, either momentary in duration, or continued for a very short time, and performed so as to occasion a very considerable shock, will generally accomplish the latter object, viz. reaction, the same agent employed for a longer period, more frequently repeated, and conjoined with the additional means alluded to above, and particularly with the semicupium, or stimulating pediluvia, will attain the former object, viz. depression of morbidly increased vascular action.

II.—*As to the particular mode in which the Affusion to the Head may be resorted*, it may be necessary to add a few particular directions. In cases of congestion of the encephalon, stupor, coma, or poisoning from narcotics, a morning gown, or any other article of clothing, if the patient be out of bed, may be drawn around the neck and shoulders, every thing else having been removed, and the patient's head should be inclined over a large vessel. An attendant should then pour cold water from an ewer, at a considerable height, in a full stream, upon the head, and continue the operation according to the views and intentions developed above, to the state of the circulation, and the effect produced. In cases of this description the affusion may be only momentary, or for a very short, seldom beyond the fourth or half of a minute. The advantages of pouring the water from a considerable height are often remarkable, they being much enhanced by the impulse of the stream on the head of the patient. The attendant should, therefore, stand upon a chair, or use other means of elevation, whilst he affuses the water. The practitioner ought in no instance to be satisfied with committing the practice to an attendant without his own superintendence, and particular regulation of the practice, according to its effects, and the varying state of the patient.

When the patient is in bed, his head may be held, in a somewhat elevated position, over its side, and the affusion will be performed with much ease. In cases of vital depression, the affusion may be employed either in the recumbent, or semirecumbent posture. But in cases of vascular excitement, it ought to be practised whilst the patient is either altogether erect or sitting; should be continued for a much longer time, and the head kept elevated afterwards, and surrounded, if the case requires it, with a thin cloth, kept moist with some lotion, until depression of the temperature, and of the vascular action, is carried as far as is required. With children the affusion may be performed by squeezing, at a considerable height above the head, a large sponge full of cold water, or in the way already stated.

When the vital energies of the frame are much reduced, and in cases of poisoning by narcotics, or in asphyxia, when the action of the heart is nearly abolished, care should be taken not to allow the cold water to run over the breast and shoulders, or down the body; the affusion should be strictly confined to the head. When it extends further, the impression occasioned is often too much for the powers of life in extreme cases. In these it will generally be advantageous to administer, internally, diffusible stimuli, and to employ stimulating frictions, externally, at the same time, with the view of rousing, by these means, conjoined to the affusion, the powers of life, and enabling them to react, so as to restore the circulation within the head.

On the other hand, when the affusion is employed in cases of acutely increased vascular action in the encephalon, those means which are the best calculated to elicit an afflux of blood to the lower extremities and surface of the body, may

be conjoined with it. Thus it will be extremely serviceable to prescribe the warm-bath, semicupium, or pediluvia, particularly the last, rendered somewhat irritating to the skin by the admixture of a considerable quantity of salt and mustard, and to direct the cold affusion to the head at the same time; and I have not unfrequently employed stimulating frictions, or mustard poultices, to the lower extremities at the same time with the affusion. In all cases of this description, these combined means should be either employed under the eyes of the ordinary medical attendant, or very particular directions given respecting them to an attentive and intelligent nurse.

Bulstrode-Street,
Welbeck-Street, Dec. 1831.

[To be concluded in the next Number.]

THORACIC HERNIA.

To the Editor of the London Medical Gazette.

SIR,

IF you deem the following case deserving any mention in your excellent periodical, it is much at your service, with any further information you may wish.

I am, sir,
Your most most obedient servant,
JAMES GREETHAM,
Surgeon, &c.

10, North-Street, Portsea.

— Brown, between thirty and forty years of age, a strong, muscular, healthy-looking man, applied to me for medical assistance. On the evening of October 19th, when I first saw him, he complained of excessive pain in the region of the stomach—had been drinking beer with a friend, which he thought not to be good, and to have occasioned the spasms. He informed me, that some years back he was drinking in a public house in London, when one of his companions, in a drunken frolic, threw a broad pointed knife at him, which passed through his clothes and entered his body on the left side, between the fifth and sixth ribs, and penetrated to a considerable depth. He was immediately removed to Guy's Hospital, where a piece of lung protruding from the wound was cut off; he remained in the hospital for some time, till he recovered, since which he has been subject to violent spasms in the region of the stomach, though never so acute as at the time of his application to me. He had been, since the time he left the hospital, to India several voyages, as steward of an East Indiaman. I found him rolling on the floor in extreme agony, with constant vomiting, and having had a motion from the bowels. I bled him, and sent an anodyne mixture, which quieted the pain for a short time; but the bowels becoming constipated, I gave him a smart cathartic, which had no effect, though repeated at intervals. This led me to suspect an internal stricture. I afterwards applied enemata, which were always returned discoloured. About the third day constant vomiting and hiccough came on, which continued throughout. I then injected about a gallon and a half of water, which was immediately returned. I next inflated the intestines to their greatest extent, and lastly, used tobacco fumes. For the last five days he vomited constantly very fetid stercoraceous matter, and died on the fifteenth day after his application to me.

Dissection.—On opening the abdomen there was no appearance of omentum; the intestines were inflamed in patches; the stomach was inverted forward, with the large curvature held tight up to the left side of the diaphragm, apparently by adhesion to it of some extent, as was likewise the transverse arch of the colon, which was distended to three times its natural size. On opening the chest the lungs were perfectly healthy, but the lower lobe of the left lung was much smaller than the cavity of the chest, and had an old cicatrix at its lower part, and was likewise adherent to the pleura and inner cicatrix of the wound between the fifth and sixth ribs. In the lower part of the cavity, on that side lay an immense mass of what at first sight appeared to be condensed lung, but, on inspection, proved to be the whole of the omentum perfectly condensed, which had passed through the tendinous part of the diaphragm, pulling up the large curvature of the stomach, and keeping it fixed in an inverted position. The whole was attach-

ed by very firm and old adhesions to the orifice in the diaphragm, the adhesions appearing quite tendinous. Through the fold of the omentum a portion of the transverse arch of the colon was drawn into the chest, forming a complete and irreducible stricture, no part of which was gangrenous; but the colon on the iliac side was much so, and had a small hole in it, through which air had passed into the abdomen, which was greatly distended, and exceedingly fetid.

The appearances on dissection confirm the truth of the man's statement. There can be no doubt that the diaphragm was wounded by the knife, and that the whole or part of the omentum had then passed into the chest, which may account for the repeated spasmodic attacks to which he had been subject ever after, and which he always found more severe when he had been eating or drinking more than usual. The occasion of the man's death is sufficiently obvious, in the passage of part of the transverse arch of the colon through the folds of the omentum into the stricture, which must have been very recent, although he had suffered from constipation of the bowels several times previously. I am sorry I have forgotten the exact time which he mentioned being at the hospital, as probably by a reference to the books his story may be corroborated. It certainly appears to be one of the most interesting cases of thoracic hernia on record, and deserving notice by the profession. I have the parts preserved in my possession, and send you a rough drawing of the same, for your inspection.

Back View of a case of Thoracic Hernia, in which the whole of the omentum, and part of the transverse arch of the colon, was drawn through the left side of the diaphragm into the thorax.

a, Back view of the stomach, with the œsophagus passing at the back of the cut edge of the diaphragm.

b, Part of the left side of the diaphragm, through which the stricture passes.

c, The strictured part of the transverse arch of the colon.

d, Continuation of the colon.

e, The mass of omentum passed through the stricture, laying in the thorax.

FURTHER OBSERVATIONS

ON

THE LATERAL OPERATION OF LITHOTOMY.

By R. B. Todd, B.A.

Lecturer on Anatomy and Physiology at the Medical School, Aldersgate-Street.

The following must be universally admitted as desiderata in the lateral operation of lithotomy :—

1. That the opening from the skin into the bladder should be a simple incised wound. 2. That section of the body of the bladder should be guarded against. 3. That both the rectum and pudic artery should be secured from injury. I believe no one can assert that these objects are completely attained by any of the usual methods of the lateral lithotomy.

In operating with the knife and staff (whether curved or straight), the incision in the prostate and bladder is arbitrary : hence we find as common occurrences, insufficient incision of the prostate, and the more dangerous error of too extensive an incision of that gland. The former may occur, as Scarpa observes, from the point of the knife being impeded in its passage along the groove of the staff; from the resistance offered to the cutting instrument by the dense tissue of the prostate turning it away from its course; or, in fine, because the gland often flies before the instrument, so that the surgeon imagines he has incised it to a sufficient depth, when he has scarcely cut its anterior part. The rectum and pudic can hardly be said to be free from danger in this operation, as both have been wounded even in the most experienced hands.

In cutting with the gorget, the opening from the skin to the bladder is not a simple incised wound, and both rectum and pudic are in imminent danger. The only advantage which this operation appears to possess is, that of security against *too free* an opening in the bladder.

The operation by the bistoury cachée frequently divides the apex of the prostate completely, while its base is only partially so; if we add to this the danger to which the posterior part of the bladder is exposed, we cannot hesitate, with Deschamps, to denounce this instrument as "peutêtre le plus dangereux." Here too the rectum and pudic artery can by no means be pronounced safe.

In my last communication on this subject I detailed the steps of the operation by a lithotome and straight director, employed with uniform success by my friend Mr. Peile. I would particularly urge the necessity of keeping the director well applied against the arch of the pubes, lateralized to an extent proportioned to the size of the perineum, and also of preserving the lithotome in the same relative connexion to the director when withdrawing as when introducing it. If these directions be adhered to, I have no hesitation in confidently declaring, that each and all of the above-mentioned desiderata are completely attained by this mode of operating. 1. A single trial on the dead subject must at once prove that the opening into the bladder must be by a simple incised wound. 2. The section of the prostate cannot exceed the diameter of the blade of the lithotome, and must equal it. 3. The rectum (if empty) cannot be wounded unless the director be lowered to an unwarrantable extent; and the pudic must be secure, even though the director be lateralized to but a slight degree. At all times Mr. Peile is in the habit of dilating the wound in the bladder, by pressure on its edges with the finger; if this do not suffice for the extraction of the stone (which rarely happens), he advises the director and lithotome to be re-introduced, the handle of the latter being slightly depressed. In this way, or by the introduction of a bistoury guided by the finger, the incision in the bladder may be enlarged; but Mr. Peile informs me that, in the course of an extensive practice, he never had occasion to enlarge the opening in the bladder otherwise than by dilatation, and that since he has adopted this mode of operating, he has not had an unsuccessful case.

I may mention that Mr. Ferguson, the instrument-maker to St. Bartholomew's Hospital, has made a set of these instruments; they can at all times be seen at his establishment.

March 13, 1832.

THE PRESS *v.* THE MEDICAL PROFESSION.

To the Editor of the London Medical Gazette.

SIR,

As a member of the medical profession, and as one who regards the objects it is appointed to investigate as well worthy the attention of any mind, however extensive its grasp, or however philosophic its turn, I cannot but feel injured by the public, when, in the zealous discharge of my duty, and in the ardent and energetic pursuit of medical science, I am branded with the accusation of selfishness and venality; and although, in an age so enlightened as the present, it would be difficult to preconceive so great an error in popular judgment, yet the experience of the last few weeks has been more than sufficient to place it beyond all doubt. Deep would be the indignation of us all at having our motives and efforts so mistaken, were it not for a feeling of shame that our judges had been misled by schisms in our own ranks, and that by gratuitous assertions, addressed to their natural prejudices, they had been induced to arrive at a verdict so disconsonant with the truth, and so derogatory to our character. You may see, sir, that I allude to the actual position of the profession with regard to the public upon the engrossing subject of cholera; and were this all the evil, willing would I be for one to bear with it, contented with the assurance that time would unfold what ignorance at present conceals, and that justice would reassert her right, and award accordingly: but it rests not here. In a place of such commercial importance as London, it is natural that measures, imposing restrictions upon trade, should be met by a reluctance in their adoption, and that opposition should be made to them, founded upon such views as might seem the most rational. Thus, the slow and insidious manner by which the metropolis was invaded by the disease, permitted minds inclined by interest to doubt the existence of cholera, and the emblazoned testimony of certain consonant opinions too hastily deduced, seemed for a time confirmatory of this opinion. I say too hastily deduced, because, if the facts relating to the march of this disease across Europe, and its subsequent appearance in the north of England, had not been sufficient to insure some expectation of its arrival here, still I know not on what grounds our exemption could have been anticipated—much less can I comprehend the nature of that mind, that would arrogate to itself a power of judging in the study upon a case, superior to that acquired in the presence of the patient himself. But thus it was; and hence has in part originated the injustice of the public, and the difficulty under which the profession is placed. That this difficulty is not visionary, every periodical and paper proves; and that in some parts of this metropolis it reverts upon the head of the community, by misleading the minds and damping the efforts of the benevolent, is as evident. The *existence* of cholera is denied; or (dispensing with names), it is asserted by numbers that "no new form of disease" prevails, and the public dissent of certain physicians is still adduced as authority for the opinion. Does it not then behove them, sir, who have thus added weight to the prevailing error, and who by this time must be surely undeceived —I say, sir, is it not imperatively demanded of them, to stand forth and declare their altered views, that justice may be rendered the profession to which they belong, and that the efforts of all may be lent undistractedly towards the one desired end, viz. the eradication of the disease from this country?

I am, sir,
Your very obedient servant,
Q.

London, March 18, 1832.

ACCOUNT OF THE METHOD OF TREATMENT ADOPTED IN CHOLERA,

At Holywell and Earsdon, and which proved eminently successful.

BY JAMES W. EARLE, Esq.

DURING the course of the last spring and summer I was engaged in investigating the nature of the laws which regulate the vital functions, with a view to elucidate the nature of hydrophobia.

I was thus led to consult various works bearing upon that subject, and more particularly those of Fontana and Dr. W. Philip, in order to ascertain the precise effects of stimulants and sedatives upon the nervous system. This will, in a great measure, account for the similarity between my opinions and those of Dr. W. Philip on the nature and cause of cholera. Upon the appearance of this disease in this country, I observed that the whole of the symptoms bore the closest analogy to the effects of the experiments which I had been so recently studying, and I felt anxious to ascertain whether a plan of treatment, founded upon these principles, would be successful. I took the earliest opportunity in my power of proceeding to Newcastle for this purpose, and through the recommendation of Dr. Smith, an eminent physician of that place, I went to Holywell colliery to assist Mr. Pyle, a gentleman residing at Earsdon, in which parish Holywell is situated, and had that colliery placed under my care, when the cholera was making its first attack, which has usually been the most fatal period: I there pursued the plan of treatment I had previously considered as most applicable, and the success I met with was fully equal to my expectations, for out of thirty-five cases which occurred to me at Holywell, and twenty-five under the care of Mr. Pyle, who pursued the same plan of treatment, making in all sixty cases, not one terminated fatally.

From a careful attention to the whole of the phenomena observable in the onset and progress of this formidable disease, it appears that there is a something emanating from the bodies of those labouring under it, which, upon being applied to a person in any way predisposed to be affected by it, makes a direct and injurious impression on his nervous system generally, and at once deranges all its functions. The effects of this impression will be more or less severe, and the disease more or less rapid in its progress, according to the degree of exposure, and the liability of the individual at the time to receive it. To be convinced of this we have only to observe what description of persons are most frequently attacked — namely, those of intemperate habits and irregular lives, whose nervous systems are in a state of debility, the consequence of over-excitement—those who have been weakened by illness of any kind—and women, whose nervous systems are less vigorous than those of men. In short, any thing capable of lowering the tone of the general health, by which, of course, the nervous system will be affected, is a predisposing cause of cholera.

As the whole of the symptoms of cholera are clearly referrible to a debilitated state of the nervous system, acting universally at the same moment, it is not of much importance which is considered first. In those persons in whom, from a natural vigour of constitution, this disease is not quite so rapid in its progress, the order of the symptoms, if carefully noted, will be generally found to be this—dizziness, purging, thirst, faintness, nausea, vomiting, cramps, affection of respiration, with pain at præcordia, cessation of the secretions from the liver and kidnies, coldness, lividity, blueness, cessation of vomiting, and of purging, thickened state of the blood, cessation of cramps, coma, death. The most prominent, however, of these symptoms most frequently invade with such rapidity and severity, that those less remarkable are not observed; but the order varies very much. I have known the cramps appear as the first symptom in three females, but I never could ascertain that they had appeared in men, previous to the purging*. However independent the sensorium may be of the nervous system, the dizziness which is so frequently noticed as the very first symptom, clearly indicates an affection of the sensorial power. As it is unaccompanied by pain, patients seldom mention it unless asked about it, and then those under my care have always expressed it as a "trouble in their heads." They are much disposed to fall into reverie; and during sleep, which is generally disturbed, dream of a thousand different things. Upon waking, they feel so little refreshed that they can hardly believe they have been asleep, but fancy they have only been *thinking*. I regard the state of coma, or rather of lethargy, which comes on after the cessation of the purging and vomiting, merely as the extreme of this affection, for it is not necessary that the in-

* I have since writing this heard of one man in whom the cramps appeared as the first symptom.

tellect should be deranged to prove that the sensorium is weak, and it should be remembered that even under a very great depression of the sensorial power, a patient is able to say where his pain is, or to express his wants.

The long-continued vomiting may be by some regarded as a part of the general spasmodic affection of the muscular system, but though vomiting is effected by the violent action of certain muscles, they are not cramped in performing this office. It is a natural action, though violent, requiring an union and consent of motion in them, which is excited by their sympathy with the stomach; and we observe that the whole of the abdominal muscles may be violently cramped without producing vomiting.

The feeble action of the heart and circulation is but another sign of the depressed state of the nervous system, and I am fully justified in saying, that if there is any one part of physiology capable of clearer demonstration, or which has been more satisfactorily explained in the experiments above referred to than another, it is the direct influence which the operation of stimulants and sedatives is capable of exercising upon the energy of the circulation through the medium of the nervous system. As the heart and larger vessels have, from their greater supply of nervous power, as is shewn by the anatomical disposition of the ganglions and plexuses around them, a greater power of action than the minute vessels, so they are the last to lose it. Thus we observe in cholera, that while the action of the capillaries has absolutely failed, that of the heart and larger vessels still continues, however feebly, and does not altogether cease, except with life itself. The congestion of these large vessels, which has been so much spoken of as one of the causes of this disease, is itself the result first of their feeble action not being sufficient to force the blood onward into the capillaries; and, secondly, of the entire failure of the action of these latter vessels rendering them incapable of receiving and transmitting it: therefore the blood must of necessity remain in the large vessels, and the more feeble their action, the greater will be the congestion—and such is actually the case in cholera.

The dark-coloured and thickened state of the blood is the result, first, of the failure of the nervous power in the lungs, and, secondly, of the profuse separation of its fluid part from the surface of the stomach and intestines, while the retention of the pabulum of the various secretions in it helps still farther to increase its consistency. If any argument be required to prove that the nervous system is the first which suffers in this disease, and that it takes the lead in all the other symptoms, the single fact of the blood being nearly unchanged in its passage through the lungs is sufficient, while at the same time it proves the fallacy of the doctrine of animal heat, which refers those changes to the chemical action of the atmospheric air on the blood. It is quite clear that such exposure in the air cells will not effect the necessary changes without a due supply of nervous influence. The deepening of colour, though rapid, is not sudden, and only takes place in proportion to the decrease of the nervous influence. The experiments of Mr. Brodie prove that the formation of animal heat does not take place in the lungs; and the experiments of Dr. Philip prove that it does take place in the capillary vessels, where the blood is in its most minute state of subdivision, and most exposed to the operation of the nervous influence —where all other secretions are formed, and consequently that it should also be regarded as a secretion. I have already shewn that the blood, on account of its altered condition, is not capable of affording any heat—that the capillaries are not capable of conveying the blood; and, even if they were, that there is no nervous power to effect the evolution of caloric. The plumpness and freshness of youth depend in a great measure upon the quantity of fluids in the body, and the shrunk and shrivelled state of age upon their absorption; but the prodigious discharges in cholera, amounting, as I have seen, to several gallons in a single day, will effect more than the work of twenty years. I have sometimes, indeed, been not so much astonished at the degree of collapse, as at the immense effusion afforded even by an old person. The collapse, therefore, is the necessary result of the continued operation of all the other symptoms.

It thus appears from what I have stated, that the whole of the symptoms observed in cholera, whether appearing slowly in due succession, or with such

rapidity as to render the order scarcely distinguishable, or however variously grouped together (depending upon the peculiar temperament of the individual), are the children of one common parent, viz. debility of the nervous system, the ultimate effect of which is a suspension of all the secretions; and, reasoning from the fact that all and every one of the symptoms may be produced artificially, by interrupting the supply of nervous power, I am fully justified in considering them to be the result of this universal debility; not only because we know of no cause capable of acting equally and to such extent upon all functions at one and the same moment, but more especially, as I shall presently shew, that a plan of treatment founded on the principles which this view points out—viz. to stimulate the nervous system, and to excite the natural action of each secreting surface and organ—is fully equal to the relief of all the symptoms, and consequently the cure of cholera.

After this consideration of the cause and nature of the symptoms, it will be evident that the indications to be fulfilled in attempting their alleviation and removal must be to stimulate the nervous system, and to excite the natural secretions.

A plan of treatment founded on these principles is all that is necessary for the cure of this much-dreaded cholera. It is not only sound in theory, but successful in practice; and I venture to assert, that the treatment of those who constantly keep these principles in view will at once become steady and determinate, the success will more than equal their expectations, and they will no longer range from one extreme of practice to the other, trying medicine after medicine of the most opposite kinds, till they abandon all in utter despair. There are various causes, however, such as great age, previous illness, &c. which render it impossible that any plan should be universally successful; and where these causes do not exist, medical aid frequently does not arrive until the temperature of the body has sunk perhaps fifteen or twenty degrees below the natural standard, and the purging and vomiting has ceased some hours, indicating so great a degree of debility of the nervous system, and loss of action in the capillary vessels, that no means we can employ will restore their energy.

The medicines I have used have been opium in small doses, so as to secure its stimulant effect, joined with calomel, because I know of no other substance capable of more thoroughly permeating the whole system, and consequently exciting all the secretions at the same time. I have used mustard as an external stimulant, because the action of the heart may be affected in this way long after any stimulant has ceased to remain on the stomach, or produce its usual effect even if it should remain. I have not made use of these means under the idea of their having any specific virtue beyond that of their known action; any other medicines that would have the same effects, but act more speedily, would no doubt be preferable. I have deemed it necessary to insist more particularly upon the importance of keeping the principles in view, because the substitution of any other medicine having a dissimilar mode of operation on the system, will be an abuse instead of an improvement. This was the plan of treatment I had determined to pursue, and I explained my intentions to two gentlemen, each of them holding important situations in different establishments in this metropolis, before my departure for Newcastle; and the gratification attendant on the success it met with, is not a little enhanced by knowing that it was not the result of accident, but of previous study. The minds of the medical profession, and of the public in general, have been so unfortunately preoccupied with the idea that the purging and vomiting are to be combated by brandy, by opium, and by astringents, that I fear it will be yet some time before any plan of treatment, such as the principles I have above laid down indicate, comes into general use; but the time will come when cholera, from its being better understood and better managed, will be no more dreaded than many diseases which have long been common among us. I have not to advocate the employment of any medicine which has not been already administered in this disease; and, indeed, it would be difficult to find any, from croton oil to chalk mixture, or from æther to tobacco, which has not had a trial.

The extremely feeble state of the

circulation, and the diminution of the animal heat, has led to the employment of many means which, in ordinary cases, are well known to have the desired effect. Ingenuity has been much taxed in various ways, to discover the most convenient and effectual means of applying warmth: but if the preceding observations are founded in truth, animal heat is one of the results of the action of the nervous influence on the blood in the minute vessels; and, in order that the temperature be maintained at its healthy standard, each must perform its proper part; the blood must be carried by the vessels, and in a state fit to be acted upon. In health the external application of warmth relaxes the skin, and allows more blood to flow into the part, and an increase of temperature is the result; but when the circulation has ceased in the capillaries, though we may relax the skin, we cannot increase the flow of blood, on account of the heart not having power to force it into the capillaries; nor could we, indeed, expect to restore the circulation merely by producing one of its effects. We have, therefore, every reason to believe that the application of warmth has been only beneficial in so far as it has prevented radiation, and economised the small quantity of heat which the circulation has been capable of forming. This, however, is very important.

Some have placed great reliance upon long-continued friction, and have strongly recommended it. By means of friction the blood may no doubt be mechanically forced through the vessels toward the heart; but, unless we can at the same time restore their natural action, and cause the performance of those changes which the blood should undergo in the lungs, friction can be but of little avail. Indeed the pressure, by more completely emptying the smaller vessels, as it can give no increase of power to the larger, will tend considerably to augment the accumulation already existing in them. The most powerfully stimulant medicines have been administered, with the view of restoring the circulation, but without effect.

I have already had occasion to allude to the influence which stimulants and sedatives are capable of exercising upon the heart through the medium of the nervous system, and it appears from the results of the experiments so often referred to, that the heart and vessels of circulation, being independent of the brain and spinal marrow, are capable of being influenced by stimuli applied to any part of them. Of course we cannot repeat these experiments on man. I wished therefore to ascertain, whether the action of the heart could be equally excited by stimulants applied to the extremities of the nerves terminating all over the surface of the body, which may be regarded as the other end of the nervous system. This I thought very possible, from observing that the capillaries in the web of a frog's foot are excited by spirit of wine. I found, by experiment, that the heart's action could be as completely accelerated by stimulating the sentient extremities of the nerves on the surface of the body, as when applied to the brain itself by Dr. Philip*. External stimulants, therefore, beyond their mere effect of exciting the skin, also stimulate the nervous system, and through it the circulation, so that we are able, by employing them, to strike directly at the root of cholera.

The operation of the moxa is the same in principle, but differs in degree. The same experiments have shown that as a considerable surface of the brain should be stimulated in order to affect the heart, so should a large extent of the surface of the body; and the quantity of excitement obtained from the application of mustard to a large extent of surface, will be far more than equivalent to the intensity obtained by applying moxas to a few small points, besides which there will be no troublesome sores to heal afterwards, as the mustard will have produced its effect before vesication takes place, the object for which it is applied being obtained when the redness of the skin is fully excited and the pulse becomes perceptible.

As it is not my object to swell my observations to the size of a book, I shall state, in as concise a manner as I am able, the particular circumstances under which the plan of treatment above recommended is applicable. The dizziness, as I have before observed, from its being unaccompanied with pain, is very seldom mentioned; add to this,

* This experiment is very simple. Expose the heart of a frog, and count the number of contractions per minute; then immerse his lower extremities in spirit of wine: in three minutes the increase will be perceptible. This experiment I performed the morning before my departure for Newcastle.

that the patient is scarcely ever seen when this is the only symptom; but if closely observed it will be found gradually to increase to faintness, and this again increases until connected with a sensation of nausea, when the vomiting quickly follows: this I think clearly shews, that the vomiting is produced by the continued operation of the same cause which produced the dizziness. When, upon being called to a patient, I find either some or all of the following symptoms,—"troubled in the head," purging, vomiting, great thirst, cramps, eyes sunk, and the hands assuming a peculiar shining appearance, or the skin slightly corrugated, and the pulse not yet considerably affected, nor the secretion of the kidneys stopped,—I have been in the habit of giving to an adult one grain of opium with five of calomel every hour, for three or four doses, and then waiting three or four hours until a darker appearance of the evacuations shows that the calomel is acting. The cramps and vomiting now disappear, and the purging ceases in the opposite way to that in which it began—each succeeding discharge being more consistent than the preceding. In two or three hours from the first sign of the action of the calomel, I have given some rhubarb with a little ginger, or castor oil, or any mild aperient, and repeated it according to circumstances. This simple treatment, with attention to diet, &c. for a few days, soon restores them. The dizziness and nausea, with occasional vomiting, are the last symptoms to disappear, and were always observable in the patients under my care, for they being poor could not be restrained from returning to their usual occupations. It was always, however, relieved by a recumbent posture, as faintness is, which fully satisfies me that congestion is not its cause. I received one day hurried messages from two of my patients, (one, Compson's child, a girl of nine or ten years, the other, J. Hall, a stout lad of nineteen,) to say, that they were as bad as ever, and dying. I found that the child had been allowed to sit up, and that the lad had been placed in a chair while his bed was arranged. I enjoined strict quietude and an horizontal position, and heard no more of the vomiting.

There were only four children attacked during my stay at Holywell. The symptoms in them yielded to calomel and rhubarb, or jalap; and I think any *purgative* treatment would have answered equally well, because from the greater activity of their capillary systems, as compared with adults, the secretions are more easily excited. I gave them no opium.

If the above-mentioned symptoms have continued long, or even if not, provided the pulse is much reduced in strength, I judge that its total cessation, and the complete collapse, are near at hand. There is then no time to be lost; the action of the calomel should be quickened by every means in our power, and we have none more speedy than that of blood-letting. A moderate quantity will have this effect, and the opium and calomel should be given as before.

Some persons may perhaps say, that most, if not all the cases so treated, might not have been cholera at all, but simple diarrhœa. To such I must answer thus:—Thomas Hope, æt. 24, and Thomas Fairlie, æt. 21, cousins, both living in the same house, and working in the same pit, were attacked with the same symptoms, but Hope first. These were among the early cases at Holywell, before persons were aware of the danger of neglecting the purging, &c. consequently their friends did not apply for assistance until Hope was completely in the state of collapse; his eyes, which were sunk deep in their sockets, had a dark livid circle round them; the feet and hands were cold and shrivelled; pulse barely perceptible, &c.—in short, it was a well-marked case of cholera, about which there could be no mistake. Reaction was effected by means of the application of mustard to the whole of the back, chest, and abdomen; the secretions were excited by calomel and opium, and the subsequent fever was remarkably mild. The mustard in this case was allowed to remain on longer than necessary, and caused a good deal of vesication, which occasioned considerable discharge, and retarded his recovery. Fairlie finding himself attacked with the same symptoms, and observing the effect of inattention to them in Hope, applied earlier, and was treated with calomel, opium, and aperients, with the most complete success. If there are any who still doubt, I must request them to suspend their judgment until they meet with such cases.

When a patient has not been seen by

a medical man until the collapse is far advanced and the pulse imperceptible, no medicine can have its proper effect; all act more or less as emetics in proportion to their stimulant power, on account of the incapability of the stomach, from its loss of nervous power, to bear any stimulus; no time should therefore be lost in restoring the action of the heart by means of external stimulants, which have the power of raising and maintaining the pulse long after all stimuli taken internally have failed. The longer the time since the failure of the pulse and cessation of the vomiting, of course the greater will be the quantity of stimulus necessary to affect the heart: for this purpose I have been in the habit of mixing the best flour of mustard with hot water, spread on a piece of hot flannel large enough to cover the whole of the chest and abdomen. In about twenty minutes, when the action of the superficial vessels is fully excited, a few beats of the pulse may be perceived; it then generally stops, and begins again. The mustard is now very painful, and I remove it to the back and spine, to continue the excitement already begun, and let it remain until the same effect is produced, when the pulse will be generally continuous, and may be counted. The brighter the colour of the redness which is produced, the more likely is the good effect to be permanent. The opium and calomel will now act, and the case should be treated precisely as if the pulse had never failed at all. Up to this time I have never given any thing internally, except extremely weak brandy and water; and as soon as the pulse is continuous, either plain water or thin gruel, in very small quantities at a time, lest it should excite vomiting, a great tendency to which remains sometimes for several days.

I have used mustard in preference to the rubbing in of any substance more speedily stimulant, because it is at all times easily procured, easy of application, and greatly saves the labour of the attendants. This is a point of immense importance in the poor man's cottage, where more than one attendant is scarcely to be procured, and often not even that one without much difficulty. It is of no less importance in public establishments, because many more patients can be attended by the same number of nurses. Perhaps a more quickly stimulant application might be made by mixing the mustard with hot spirits of turpentine, or adding a proportion of liquor ammoniæ.

In order to shew the efficacy of the plan I have recommended, as compared with the stimulant, opiate, and astringent plan, which has been so generally resorted to, I will state the following circumstance, which occurred very shortly before my leaving Holywell. A stout healthy lad, of the name of Brown, æt. 19, and his mother, æt. 40, eight months gone with child, residing at Backworth Colliery, were both attacked with all the characteristic symptoms of cholera. The means made use of were stimulants, opiates, an oleaginous mixture, bleeding, effervescing mixture to allay the vomiting, and enemata of chalk mixture and laudanum to stop the purging; but without effect. They both died in the state of collapse, although they were under treatment more than thirty hours before the commencement of the collapse. Hannah Brown, ætat. 50, sister-in-law of this woman, had also been treated in the same way, with a like want of success. Her friends hearing that no deaths had occurred at Holywell, were anxious that I should see the case (the first woman had died that morning—her son the day before). When I arrived the cramps had ceased; she did not vomit except when she took the effervescing mixture; an enema of chalk mixture with laudanum was being retained mechanically; the pulse only occasionally perceptible; feet and hands becoming cold; breath cold; the voice a whisper, and a copious clammy perspiration on the skin. Three quarters of an hour of precious time were wasted in endeavouring to persuade the medical man who had had the previous charge of the case to change his method of treatment; he then left the case in my hands, after being particularly liberal of his predictions of the fatal tendency of the plan I was about to pursue *. Another half hour was lost in send-

* I should mention that this gentleman is neither a member of the College of Surgeons, nor of the Company of Apothecaries. There were then, and I believe are still, two others of the same stamp treating cholera in the neighbourhood. Here is a new disease, requiring a first-rate education to enable a person to comprehend its nature, treated by those who have had little or none. If these gentlemen would confine themselves fo farriery, it might be as well for the poor pitmen.

ing to Holywell for mustard, &c. It was now past ten at night. Soon after eleven, however, the pulse was beating 84 feeble strokes in the minute, and when I left (at three in the morning) 108. The next day it rose to 120, but in the evening the evidence of the action of the calomel on the secretions was decided, and the pulse fell to 108. The only pain she complained of was from the peristaltic action of the intestines, and this was only occasional. She had taken thirty grains of rhubarb and ten of ginger, in two doses, during the day. The next day, at one, the pulse was 77 in the minute; no pain or symptom of congestion any where; evacuations feculent, though fluid; tendency to vomit nearly gone; tongue moist, the fur disappearing in parts, and a warm perspiration on the skin. In short, I left her, 37 hours from the commencement of the reaction, without any fever, and suggested the use of quinine, should it appear to be necessary.

As there have already been so many publications on the subject of cholera, some may, perhaps, be inclined to receive any proposal or account of success from treatment founded on a strictly scientific view of its pathology, with incredulity: I hope, therefore, I shall not be accused of being actuated by improper motives, in extracting part of a letter which I received from Mr. Taylor, one of the owners of Holywell, about ten days after my return to London, as being strong evidence of its having been found practically useful.

Eardon, by Newcastle-upon-Tyne,
February 27, 1832.

My dear Sir,—At a meeting of the owners of Holywell Colliery, held at Newcastle on Saturday last, your eminently successful treatment of their pitmen attacked by cholera morbus was taken into consideration; when it was unanimously resolved, that a gold snuff-box should be presented to you, with their best thanks, as a small token of their entire approbation of your valuable services, and which, as the organ of the owners, I feel great pleasure in communicating to you. It will, I doubt not, afford you much satisfaction to be informed that Mrs. Brown, of Backworth Colliery, who you attended and first saw in a sinking and supposed incurable state, is fast recovering by a continuance of the same mode of treatment you adopted. The whole of our people whom you cured are now following their usual employment, and seem grateful for your attention whilst they were under your care.

(Signed) THOMAS TAYLOR,
Resident Partner.

14, Old Broad-Street.

PEYER'S GLANDS ENLARGED IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

I SEND you the notes of a post-mortem examination taken in Sunderland, in the middle of last November. The case to which they refer was one of cholera, which proved fatal, in the hospital of that town; and my reason for sending them is, that the examination affords another example of the morbid development of the mucous glands of the intestines occurring in that disease. Previous to the inspection of Mary Ramsay, five cases had been opened after death, but in none of these was the same development observed. Besides the rarity of this appearance, it derives an additional interest, as Dr. Hope has justly remarked, from its having been found in the Clapham disease, the cause of which was determined with a certainty amounting to demonstration.

I need not describe at length the symptoms of the case; suffice it to say that the patient was a girl 16 years of age, who had been seized suddenly in the night. I saw her at her own house, about ten o'clock on the following morning, and she then exhibited all the most characteristic features of the disease in its worst form. In the middle of the day she was removed to the Cholera Hospital.

She died in twenty-one hours from the first attack; was examined thirteen hours and a half after death. Present, Dr. Cooke, of Durham, Dr. Miller, Mr. Mordey, and several of the medical practitioners of Sunderland*.

On turning back the omentum, which was loaded with fat, and observing the

* We have only space to insert the account of the abdominal viscera.—E.G.

bowels before they were disturbed, the large intestines externally were seen to be pale, the small of a reddish hue; several inches of the colon, between the transverse arch and the descending portion, were contracted. The stomach contained some ounces of a thick mucilaginous matter, partly of a bright green and partly of a bright yellow. The mucous membrane was thrown into numerous elevated folds, the ridges of which had the appearance of having been painted red by a careful and delicate pencil; an appearance that depended on the ecchymosis of florid blood beneath the membrane. The mucous membrane of the duodenum and jejunum was studded by innumerable glands the size of small peas, of a grey colour and fleshy consistence. None of them were ulcerated. The mucous membrane felt swollen and spongy, as well as the other coats of the small intestines; otherwise it was healthy. The remainder of the intestines, and the other viscera, abdominal and extra-abdominal, were natural; except that perhaps the liver was more vascular than usual, and the bladder was shrunk to the size of a fig. The gall-bladder contained a little dirty-green bile, which, on pressure, passed easily through the ducts into the duodenum.

Your obedient servant,
HENRY GAULTER, M.D.

Manchester, March 6, 1832.

REMOVAL OF A LARGE TUMOR FROM THE BACK.

To the Editor of the London Medical Gazette.

SIR,

I BEG leave to present you with the following case, which, if it meet your approbation, you will be good enough to publish in your highly-valuable journal.

Your constant reader,
W. M. WADE, M.R.C.S.
Surgeon to the Swanlinbar Dispensary, &c. Cocavan.

Thomas Maquire, æt. 30, a farmer, applied to me in May 1830, in consequence of an encysted tumor on his left side. It was perfectly moveable, and seemed to be of that species which Mr. Abernethy has characterized by the name of tuberculated sarcoma. It was of the colour of the skin, rather irregular on the surface, resisting pressure, and quite free from pain. The patient stated, that it had been growing gradually, in its present situation, for upwards of eighteen years. The man's health being good, I proposed an immediate operation for its removal; to which, however, he would not consent: so I lost sight of him till August 4th, 1831, when he again called on me, relative to the tumor, which had increased rapidly since my first examination.

The patient informed me that he had consulted several professional men in the meantime, all of whom declared that he must die if removal of the tumor were attempted. I again examined the diseased mass. The cutaneous surface now presented three very extensive and deep ulcerations, of a highly malignant character, discharging a thin acrid matter, of very offensive odour. Those parts of the tumor where the integuments were whole, exhibited a dark purple appearance, with a number of small tubercles disposed over the surface, particularly at its anterior inferior angle, which was composed of a number of small granular bodies, of a bluish colour. The tumor was moveable in part, and extended from the inferior angle of the scapula, about an inch of which it overlapped, to the upper margin of the crest of the ilium, measuring sixteen inches in length, and fourteen transversely. He stated, that since he left the County Infirmary, from which he had been dismissed as incurable, hæmorrhage had taken place, at different periods, to an alarming extent, by which his strength was considerably reduced. He was of a melancholic temperament; his health much impaired; body greatly emaciated from loss of appetite, nocturnal perspirations, anxiety of mind, and constant discharge from the ulcers. Bowels, however, regular; pulse 70, small, and easily compressed.

The patient now gladly submitted to the operation, which, with the assistance of Mr. Russel, of the Rush Dispensary, I performed in the following manner. The patient having been placed sitting on a table of proper height, and supported by an assistant, I drew two lines with a pen and ink, as guides, along the anterior and posterior

margins. I commenced the first incision at the upper point of the posterior margin, carrying it through the integuments to the inferior angle. Having reached the sac, I dissected cautiously, taking care to leave no diseased portion behind: in doing this, I was obliged to divide several large muscular arteries that passed into the tumor; these we tied, to the number of fifteen, having thought it advisable to apply a ligature to every vessel that was at all likely to bleed, as the man was exceedingly weak. By proceeding thus, he lost very little blood. Having completely reflected the tumor from behind, I then, with one incision, separated it from its anterior connexions. We had next to remove some diseased portions of muscle. Having dressed the wound with adhesive straps and compresses of lint steeped in spirituous lotion, we put the patient to bed, and administered thirty-five drops of tinct. opii. We left him to his repose, with orders to keep the lotion as above constantly applied.

Evening visit.—As well as could possibly be expected.

August 5th.—Complains he cannot pass urine. Ordered mucilaginous drinks, with Spirts. Nit. dulcis. No pain in wound; appetite scanty; no perspiration. Low diet.

7th.—Considerably better. Tongue moist and clean; pulse 70; bowels open by medicine; passed urine two or three times; had no perspiration since operation; wound looks well. Same dressing.

11th.—One ligature came away today. Every thing going on well.

November 7th.—The man is now quite recovered, the wound perfectly cicatrized, health restored, and not the slightest appearance of any return of the original disease, as apprehended by the gentlemen who declined operating on him on that account. The tumor, I may add, weighed 10 pounds, and has been presented to the Museum of the Royal College of Surgeons in Dublin*.

* We have omitted one or two of the daily reports, as we are pressed for space, and they did not appear to be essential.—E.G.

FRACTURES OF THE FORE-ARM.

To the Editor of the London Medical Gazette.

SIR,

I OBSERVE in No. 24 of the Medical Gazette, some remarks made by Mr. Lonsdale upon the treatment of fractures of the fore-arm; and as his sentiments and my own are at variance, I will, with your permission, through the same channel, make a few observations upon the subject.

Mr. Lonsdale remarks, "that in fractures of the fore-arm, when splints have been applied, and the arm kept in a state midway between pronation and supination, that after the bones have united, the hand cannot be perfectly supinated, because the lower portion of the radius being carried with the hand, when it is placed with the palm towards the chest, is in the position between pronation and supination, while the upper portion, not following it, remains in a state of *perfect supination*, so that when the bones are united, the hand can never afterwards be perfectly supinated, because the portion of bone upon which the motion of supination depends, is *supinated to the utmost*, and consequently cannot carry the hand further into the state of supination than it already is."

Now, sir, I beg to state, that having dressed at St. Bartholomew's Hospital during the last six months, I have been accustomed to place, and to see placed, simple fractures of the fore-arm, in a position midway between pronation and supination, and that I have not seen a single instance in which the limb has not done perfectly well, and the patient had the *complete power* of pronation and supination.

As Mr. Lonsdale does not appear to confine his observations to any particular part of the radius which may be broken, I conclude that he considers his remarks as applicable to fracture occurring in any situation of the bone; and I would ask, supposing a fracture to occur below the insertion of the pronator radii teres, is it not reasonable to suppose that the upper part of the bone, in consequence of the action of that muscle, would be *more inclined* to a state of *pronation* than supination? How, then, can Mr. Lonsdale's argument hold good?

But I will suppose the fracture to take place *above* the insertion of the pronator radii teres, and I am quite at a loss to imagine how Mr. L. can *prove* that the upper portion of the bone is in a *complete state of supination,* and what reason he can assign for the supinator radii brevis acting to its utmost extent.

I would beg to state, Mr. Editor, that in expressing my opinion upon this subject, I am actuated by no unfriendly feelings towards Mr. Lonsdale, neither do I wish to decry the mode of treatment which he so strongly recommends, not having given it a trial myself; but, on the contrary, if Mr. L. finds the method he has advised gives to his patients the perfect use of their limbs, and the means usually followed fail to do so, I should strongly recommend him to act up to his own views; at the same time he must allow me to differ from him "*in toto*" when he asserts, that placing fractures of the fore-arm midway between pronation and supination, renders the patient for *ever afterwards* incapable of turning his hand completely supine; and as long as I find that position succeed so well, I hope he will pardon me if I continue to adopt it in preference to the one he has endeavoured to substantiate.

It remains for me now, Mr. Editor, to apologize for having trespassed so much upon your time, but should you deem the observations I have made worthy a place in your valuable periodical, you will oblige me by inserting them.—I remain,

Your obedient servant,
SEPTIMUS RODICK.

Edmund's-Place, Aldersgate-Street,
April 2d, 1832.

MEDICAL GAZETTE.

Saturday, April 14, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

PROGRESS OF THE CHOLERA IN PARIS.

THIS horrible malady, which has hitherto baffled all speculation about its probable route and the laws which were supposed to regulate its future severity, has, by its invasion of the French capital, and its progress there, removed every appearance of verisimilitude from the hasty conclusions of certain of our "eminent" reasoners. In one point of view, we cannot help thinking that our continental neighbours have ground for congratulating themselves on the unexpected and decisive shape in which the disease has come among them; for it saves them from all those impertinent and mischievous intermeddlings of conceited quacks who would otherwise, at any expense to the public safety, attempt to purchase a little short-lived notoriety by presenting all the opposition in their power to whatever measures the better-informed guides of the community might have determined on. The game of those gentry is up in Paris: their intrusion on the public, which, were it once permitted, would soon prove more noxious than the pestilence itself, will not be endured in that capital; nor can any thing afford a stronger contrast to the headstrong silliness of some of *our* non-medical contemporaries than the manly, cordial, and straightforward spirit with which the French journals, without an exception, co-operate with the exertions of the Parisian faculty. The columns of the public prints are not thrown open to the contemptible effusions of fifth and tenth-rate practitioners, nor are the editors of those prints found to indulge in remarks as flippant as contemptible, at the expense of the "doctors." There is—there *can* luckily, as we said, be but one opinion on the identity of the disease in Paris with that of India, St. Petersburgh, Vienna, Hamburgh, and London; and the whole attention of the profession in the French metropolis is accordingly most properly turned to the one great object,—how best to meet their dreadful visitor.

The mortality, however, from the disease is already tremendous. In little more than a fortnight, nearly twice as many victims have fallen in Paris as

during the two months that the cholera has been here: and the numbers seized with the disorder have been almost thrice as great*. How is this? Is there any reasonable cause that can account for the far more frightful prevalence of it in the former locality than in the latter? That such cause or causes do exist, we hold to be beyond a question; and we shall briefly point out what has occurred to us while thinking on the matter.

It is generally known, that perhaps no city in Europe entered more early, or with greater diligence, on the investigation of the nature and proper mode of treatment of cholera than the good town of Paris: the *savans* of the French capital were decidedly the first in the field on the alarm being raised that the disease was approaching Europe: they organized committees of the Academies of the Institute, whose sole business it was to be to digest the heaps of information poured in upon them from all quarters, and more especially from those deputations sent out at great expense to Moscow, St. Petersburgh, Warsaw, and all the other principal resting-places of the disease: the reports drawn up on the evidence thus procured, and under the direction of some of the ablest physicians in France, were from time to time given to the public: the government spared no expense,—an almost unlimited grant was voted by the Chambers for the purpose of securing the public safety: a reasonably strict quarantine was enforced: and, in short, it was begun to be commonly hoped, from the route which the disease seemed to be pursuing, Great Britain being now invaded, and the complaint appearing to have little tendency to proceed southwards, that in all probability it would not reach Paris at all. Well, then,—so much for mere human foresight engaged about the extraordinary operations of nature,—Paris itself, the very strong hold of French science, diligence, and unequalled precaution, proves to be the very place of all others where the dreaded visitor would seem first to appear, and to announce its arrival with an overpowering degree of severity. How it came there we will not pretend to offer even a conjecture: but there are some circumstances connected with its new *locale*, and the proceedings of those who had to deal with the disease *in limine*, too remarkable to be omitted.

And the first thing that strikes us is what we will venture to call the extraordinary arrangements of the Parisian Board of Health: every London practitioner must acknowledge the singularly dangerous step that was taken by that body in the first instance. Here, in this great metropolis, partly on principle and partly per force, the system of seclusion was in a great degree adopted from the outset: the regular hospitals, which were under a far different form of government from those of Paris, were peremptorily and at once closed against the admission of cholera patients; and separate establishments were prepared for the exclusive treatment of those labouring under the new disease. With what success this system was attended—and it might, indeed, be far more forcibly exemplified by what has taken place in Edinburgh—facts would now seem most strongly to attest. In Paris, however, the system pursued has been very different. All the hospitals, without regard to their locality or legitimate purposes, have been thrown open to a certain extent: that is to say, two wards—one for males, the other for females—have been allotted in each hospital for the treatment of cholera. For those who are acquainted with the site of those hospitals, especially of the Hotel Dieu and

* From the cholera bulletin, dated Paris, April 10th, noon, we learn that the cases (within the walls) amounted to 5,908; the deaths to 2,235. The new cases, it may be added, from the preceding day, were 985, and the deaths 356.

La Charité, in the most thickly peopled and and ill-built quarters of the metropolis, no comment of ours will be necessary to convince them of the rashness of such a step,—while the internal arrangements about those cholera wards have been of such a nature as to induce us to think that the Faculty or the government, or both, have become utterly demented, perhaps by the precipitate, if not totally unexpected, appearance of the disease. They would seem at least to have lost all self-possession, if not common sense. The stories which have reached us from several quarters are scarcely credible; yet we dare not question their truth. It is positively asserted that there are from twenty to thirty dead bodies at once, lying unremoved, in each of these cholera wards; while fresh patients, already *cadaverized*, are pouring in to be put in their places! Then the crowd and bustle, not only from the physicians and their attendants, &c. but the *influx of visitors!* Hear what says our respected contemporary, the *Gazette Médicale*, in alluding to the state of things in the Hotel Dieu:—"*Tout en reconnaisant l'immense mortalité qui accompagne le cholera à son apparition au milieu de nous, disons combien a été funeste l'erreur qu'a commise l'administration en ordonnant que tous les choleriques fussent réunis dans deux salles seulement; l'une pour les hommes, et l'autre pour les femmes — mesure désastreuse pour les malades, désastreuse pour le service, pour les médications, pour l'humanité enfin, et dont on concevra les funestes effets, quand on pensera que dix medecins que dix services, sont sans cesse se croisant dans une seule et même salle, avec les nombreux visiteurs qui y affluent de toutes parts!*" If this be not the description of a perfect *nidus*, and a depositary for the sufficient supply of genuine cholera infection to the several quarters of Paris—for the same practice is applicable to all the other hospitals, as well as the Hotel Dieu—we know not in what consists the deficiency. Nor is this all. The doors are besieged by a rabble rout, filled with an insane curiosity, and more than half disposed to commit acts of violence; the streets are beset with other groups, furious with the suspicion of foul play, and raising the usual mad cry of "poisoning;" and, under this impression, actually flinging three or four men into the Seine, and tearing others to pieces at one of the Barriers:—whilst the members of the government, to quell these savage disturbances, and to inspire confidence, forsooth, proceed to walk the cholera wards with the heir apparent, the Duke of Orleans, at their head! Are we to be surprised then, after all this, that cholera in Paris is no respecter of persons? That the Prime Minister, as well as his coachman, have nearly fallen victims to it, and that his sister-in-law has actually perished? That other patients labouring under other diseases in the hospitals have caught *the* disease? That M. Petit, surgeon to the Hospital Gros Caillou, M. Leroux, Dean of the Faculty of Medicine, and several medical pupils, and many (*plusieures*) of the *Sœurs de la Charité*, have died? That, in short, the cholera has made its way among all classes in the French metropolis—though of course, as is usual in all pestilential disorders, the poor have been the principal sufferers?

So much for the measures by which the complaint has been met during its early sojourn in Paris; but there are yet some circumstances to which we cannot refrain from alluding, and which may serve further to help us in our inquiry into the causes of the excessive spread of cholera in that city. For its size the French capital is much more densely peopled than the British: though possessing a population short by nearly half a million of that of London, yet it is huddled up and crowded

to a much greater extent; the large proportion of narrow streets, lanes, alleys, *culs-de-sac*, and enormously high and ill-ventilated houses, all abominably provided with cabinets, sewerage, and drainage—a thing but too apparent to the senses of every visitor—must be allowed to create *fomites* for any contagion far more effectually than London in its improved condition can. Under ordinary circumstances, the comparative healthiness of the two cities is well ascertained, and sufficiently indicated by the respective rates of mortality—that of Paris being one in thirty-two, while that of London is no more than one in forty.

But all this, it may be said, may serve to account for the greater numbers attacked in the one city than in the other; but how can we account for the smaller *rate* of mortality from cholera in Paris than in our own metropolis? This, it must be confessed, is one of those difficulties which, we fear, we may not hastily presume to explain. The wide range of mortality which, in its progress from India to the west of Europe, the disease exhibited in various places, is owing, we suspect, to causes totally beyond the pale of our cognizance. It is not easily explicable why the mortality of London itself so far exceeds the average mortality of the disease throughout the other parts of the country; much less, then, can we pretend to assign reasons for its greater or less severity elsewhere.

Of one thing, however, we may assure ourselves,—that the mitigated mortality which they experience arises not from any superior therapeutical principle adopted by our professional neighbours; for there is no fixed principle, no predominant theory, no settled method of cholera practice adopted in the French hospitals. Every man there seems to have his own individual views, which he follows up by his own experiments, and is generally just as successful as any of his brethren—that is, when the disease does not prove fatal. If there be any difference or superiority observable as yet, it is decidedly in favour of M. Magendie and his punch.

In the hospital of Gros Caillou, in addition to what we have already stated, we find that not only is M. Petit dead, but the apothecary of the same institution is dangerously ill; and several of the nurses are either dead, or dying, of the cholera. In the same hospital, it is said that more than one-half the cholera patients have caught the disease in their wards,—persons labouring under pneumonia, scarlatina, and ague, being suddenly seized with the malady.

The number of medical attendants and assistants in the hospitals, and particularly in the Hôtel Dieu, falls short of what is required; and liberal offers are tendered to volunteers who may wish to serve.

On the 2d of April an infant only five days old was seized with cholera, accompanied by coldness and blueness of the surface; it died in six hours.

Cold affusion, as recommended by some of the Germans, has been tried in Paris during the cold stage; it has often appeared to hasten the fatal result, and never has been of any obvious service.

In numerous examinations of the semilunar ganglions made in Paris, they have been found in a natural condition, thus throwing farther doubt on the accuracy of M. Delpech. Some of these examinations were conducted by Andral.

Broussais, consistently with his doctrines, applies heat externally, and gives ice internally; bleeds from the arm if the patient has any strength left, and if not, applies leeches to the epigastrium. It should be added, that he gives opium also at a later stage, to the extent of several grains,—with what result we have not learned.

M. Dupuytren has abandoned the use of sugar of lead, mentioned in our last, and has adopted that of the actual cautery applied to different parts of the skin; but no better success seems to have attended the change.

M. Alibert, professing to follow up the idea of cholera being the *Febris perniciosa cholerica* of Torti, as first pointed out by Dr. Negri in this journal, has given his patients bark, the decoction for drink and for injection, and sulphate of quinine in pills every hour, beginning with three grains.

Sinapisms are also applied to the limbs. Three cases are reported in which the patients had improved, and there was every prospect of a favourable result.

Breschet has had acupuncture performed between the fifth and sixth ribs. The needle was three inches long, and only six lines of it remained outside; so it was believed to have been "buried in the heart,"—the object in view. Galvanism was then applied; the heart beat strongly, and the patient appeared to suffer a great deal, but no permanent advantage seems to have been derived.

Galvano-puncture has also been tried, but in a manner somewhat different, by M. Bally. One needle was introduced over the cervico-vertebral column, and another over the lumbar, or the stomach, but pushed somewhat obliquely, and not to any great depth. The battery employed was from ten to thirty-six pairs of plates, and the time of each discharge was about eight minutes. In the cases of four men and two women thus treated, there *seemed* to be some improvement: but, by the latest accounts, we learn that no permanent benefit was derived, and that the electro-puncture practice had consequently been abandoned.

EVIDENCE OF THE PRESS AGAINST THE PRESS.

The following extract from the Dublin Evening Post will shew that we do not stand alone (see our leader of last week) either in attributing the part taken by the daily press, on the subject of cholera, to the interference of the mercantile interest, or in denouncing the manner in which the motives and conduct of the members of our profession have been misrepresented. After expressly stating that the Editor had been applied to, "*by some mercantile friends,*" to conceal the existence of cholera, the article continues:—

"But the doctors, we are told, are silent. Heaven knows, and so they ought. They do not like to be badgered and mobbed; they know well enough that an honest declaration of opinion will raise a shout against them, as if it were their interest that cholera should visit this city. Infamous insinuations to this effect have been already circulated: the fact, however, being, that cholera would be more injurious to their interests than if the city continued in its usual sanitary state. A physician or surgeon in London, or Glasgow, is worked nearly to death—exposes himself to danger, in all its dresses—for seven shillings a-day; a hackney coachman's hire. But it is not for this the physicians work—they work because it is their duty; and that duty they have hitherto performed with a zeal, activity, and devotion, beyond all praise. What has been their reward? Even in London it has been deemed a high crime and misdemeanor for any doctor to say that there is cholera in that city."

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

The Dublin Journal of Medical and Chemical Science; exhibiting a comprehensive view of the latest discoveries in Medicine, Surgery, Chemistry, and the Collateral Sciences. No. I. March 1832. Hodges and Smith.

However tempting it might be devote a paragraph or two to the discussion of the oft-mooted question—why no scientific periodical—weekly, monthly, quarterly, or yearly—has ever yet succeeded in the Irish metropolis? we shall not yield to the temptation, nor shall we attempt to supply any answer to the question; but this we will freely confess, that we see no "exquisite reason" why periodicals should *not* prosper in Dublin as they do elsewhere—in Edinburgh, for example, and Glasgow. In Dublin they have their Royal Colleges of Physicians and Surgeons—an University of high character for its literature, and of considerable reputation for its medical arrangements—a School of physic as well appointed on the whole, perhaps, as any other in the empire—a number of hospitals, medical, surgical, fever, obstetric, lunatic, ophthalmic, &c. &c. all well ordered, and more or less complete in their several departments—and, finally, they have a most plentiful supply of practitioners belonging to every branch of the profession, high and low, resident in the city, who have their clubs, their consultations, their associations, and their reunions—and every thing, in short, but their *periodicals*. Whether they will have the good grace and nationality to support the one of which we have the specimen number before us, remains to be seen; if they do not, we can only con-

clude that the case is hopeless; for we are inclined to think that the hands that are now set to the work are as able as have ever been exerted in the same cause in Dublin—and withal, perhaps, better disposed and more cordially united. The impression on our minds, from an attentive examination of the contents of this first number, is strongly in favour of the vivaciousness of the new journal; nor do we augur the less favourably of its future success, that we find it has come out without any of those flashy and extravagant promises and pretensions which constitute the whole essence and very existence of the meteors which from time to time appear and vanish in the field of periodical literature. We give the editor (who, we understand, is Mr. Kane, the professor of chemistry to the Dublin Apothecaries' Company) every credit for judiciously abstaining from putting forth his full strength in his introductory number: he will win his way, we doubt not, by thus avoiding bootless comparisons with old-established rivals: not that we would be understood to intimate that the commodity offered us in this, his first number, is of inferior quality —we simply mean, that we *know* that most of his present contributors could have done more for him, had he needed it, and will do better, even though there were no other *collaborateurs* to back them on the editor's list, and always presuming that the latter is indulged with a fair and encouraging share of that without which the machinery cannot go on—the public patronage.

We shall now proceed to notice some of the principal papers in this number.

The first is by Mr. Hart, *on partial fracture of the long bones in children.* The injury of which Mr. H. here treats is a fracture which is sometimes met with extending through a part of the diameter of a child's bone—the femur, for instance—the remaining part becoming bent "in the manner in which a branch of a tree yields to an attempt to break it while it still retains its sap." The author advises that special care be taken in straightening the part, lest the fracture be rendered complete by using unnecessary violence. The application of splints and bandages will be required, but for no long period, reunion being generally effected in eleven or twelve days.

The next paper is a little contribution to natural history, by Mr. Scanlan, *on the effects of prussic acid on the tipula or cranefly.* The acid, we learn from him, may be said to *ergotize* the female, promoting her parturition by the strong convulsive efforts which it occasions.

In a short, but very good, article by Dr. Corrigan, *on the treatment of recent catarrh,* the author reconciles some apparent anomalies in the experience of different practitioners, some finding the complaint best managed by antiphlogistics, others by stimulants. Dr. C.'s view of the matter is, that there are two species of catarrh very similar in their general symptoms, but yet distinct in their pathology, and as in the case of croup, to be treated in different ways. In one species, the affection has extended no farther than the larynx and trachea; in the other it has seized upon the smaller tubes, and assumed the form of general bronchitis. The stethoscope readily indicates which species is present—the respiration in the first being natural all over the chest, while a distinct wheezing characterizes the deeper spread of the disorder. "When the inflammatory action does not extend to the bronchial tubes, but is confined to the trachea, as it very frequently is during the prevalence of an influenza, the exhibition of a stimulant will often remove the disease in twenty-four hours;" and "camphor mixture, with a proportion of a drachm of the ordinary tinctura opii, or a proportionate quantity of the camphorated tincture, in eight ounces, will form perhaps one of the best anodyne cough mixtures we can prescribe." But when the catarrh has become established in the bronchial tubes, (and the most cursory application of the stethoscope beneath the clavicles will be sufficient to ascertain this) the treatment, of course, will be that which is ordinarily recommended for bronchitis.

We now come to a paper which contains some very useful statements *on the composition of the Urine and Blood in Diabetes Mellitus,* by Robert J. Kane, M.R.I.A. &c. Mr. Kane informs us, that having some reason to doubt whether the quantity of urea in diabetic urine is so small as is generally considered, he performed some experiments which led him to the conclusion that "in this disease the urea is not at all diminished in quantity, but that the patient secretes, in a given time, as much of that principle as he does in the most perfect health."

"The circumstances which led me (says Mr. Kane) to the examination of the subject, were as follow: it was customary, in order to determine the presence or absence of urea, to add to the urine concentrated to a sirupy consistence, some nitric acid; the solution became dark-coloured, and after some minutes some few crystals of the acid nitrate of urea, of a brown colour, were usually deposited. One day, after adding the acid, the solution remained clear and almost completely solidified from the deposition of white nitrate of urea; this effect, however, was only momentary, and after some minutes the liquor became dark, a few bubbles of gas were disengaged, and the whole of the crystals disappeared. The solution then exactly resembled those which had always before resulted from the action of the nitric acid on the diabetic urine.

"So remarkable a circumstance induced me to try whether a larger quantity of urea could be obtained than that usually gotten, and to endeavour to find out by what reaction of the acid on the principles existent in the urine, the deposition of the urea under a crystalline form was prevented.

"I evaporated equal volumes of healthy urine, Sp. Gr. 1027.5, and of diabetic urine 1037, and added nitric acid to each; in the one a copious deposition of crystals took place, but in the other none. The colour of the healthy urine was, however, scarcely changed, whilst that of the diabetic urine, originally paler, became dark reddish brown, and its temperature was sensibly augmented; there was effected, therefore, between the diabetic urine and the acid, some process of decomposition.

"To avoid this, the experiment was varied thus:—the nitric acid was diluted with its own volume of water, and mixed with the diabetic urine in a phial, which was instantly immersed in a mixture of snow and salt; the process was successful, the colour of the mixture remained pale, and a copious crystallization of nitrate of urea crowned my most ardent hopes.

"To ascertain whether the presence of the sugar was sufficiently powerful to prevent the formation of nitrate of urea, the following experiment was made.

"To 1000 grains of healthy urine, Sp. Gr. 1027.5, 30 grains of sugar were added, and a similar quantity of the same urine taken pure. To each, after evaporation, was added the same quantity of nitric acid, the usual crystallization took place in the non-saccharine urine, but that which contained the sugar became dark-coloured, and but a very trivial quantity of the nitrate of urea was obtained from it."

Some analyses of diabetic urine are then given, in which the quantity of urea present varied from six to thirteen parts in 1000. The author's remarks we shall give in his own words:—

"It is evident that the idea of this disease, consisting in a conversion of urea into sugar, is untenable, and that the secretion of that vegetable principle goes on without influencing in any degree the secretion of the other constituents of the urine; at the same time, the quantity of fluid taken in, increases very much the volume of the urine passed, and of course reduces considerably the quantities of the normal constituents of the urine in any given weight of that secretion. This circumstance, coupled with the difficulty of protecting those easily alterable principles from the action of the bodies we use in such analyses, has been the cause of diabetes being considered to have as its pathognomonic character, a diminution in quantity of urea, and an excessive secretion of sugar; those two principles alternating in their quantities, and albumen appearing in the urine as an intermediate step in the chain of organization, when the sugar was changing back to urea, on the recovery of the patient, or when the urea was degenerating into sugar, if the disease was on the increase; while, in fact, allowing for the dilution, the urea, salts, and probably uric acid, exist in the healthy proportions, and the secretion of the sugar is a lesion of that function, perfectly primary and independant."

From his examination of the blood of various diabetic patients, Mr. K. could not satisfy himself of the presence of sugar in that fluid, though the delicacy of the process employed for the purpose may be estimated from the fact, that five grains of sugar dissolved in a little water, and mixed with a pound of blood previous to coagulation, were readily detected in the serum when the experiment was gone through. The author, however, admits that his negative results should have little weight in opposing so well an established physiological principle as that which teaches that secretion is merely a kind of elective transudation; and he concludes by justly observing, that such results should only serve to stimulate us to seek after more perfect instruments of research and more accurate methods of analysis.

Mr. Porter's *cases of aneurism*, we shall probably take another opportunity of noticing: nor have we left ourselves room to analyse, as fully as we could wish, Dr. Graves's learned *observations on secretion and the intimate structure*

of glands: some account of them, however, we must endeavour to give.

Dr. Graves, after opening his subject, discusses briefly the question, whether or not the glands merely separate from the other component parts of the blood, the animal principles which, combined, form the secreted fluid? and expresses his assent to the opinion adopted by Andral, and other eminent physiologists, that the blood contains all the animal principles and chemical combinations found in the secretions. With regard to the secretions of different glands, he moreover thinks that it may be laid down as a law, "that, in the same animal, the glands and their secretions form two series, mutually corresponding to each other, simple glandular structure always producing a comparatively simple secretion, while glands of a more complicated texture uniformly produce a more compound secretion." He then proceeds to investigate the different theories of the structure of the glands,—Malpighi's, Ruysch's, and Mascagni's, with those of Ferreinius, Schumlansky, and Mueller. We regret that we cannot find room for his critical observations on each, but the proposition by which he announces the present state of our knowledge on this subject, as well as some of the remarks which immediately follow, seem too important to be omitted.

"Besides possessing, like other organs, blood-vessels, lymphatics, cellular membrane, and nerves, glands enclose within their substance one, several, or what is most usual, an immense number of cavities, blind or shut at their terminations, but communicating freely with their larger branches, into which they open, and which again unite to form the ducts of the gland.

"In general, then, this peculiar system of cavities may be considered as forming the excretory ducts by their union, and are, in fact, minute subdivisions of its branches. These ultimate hollow ramifications of the duct, constitute therefore the distinguishing character of glands properly so called, and it is by means of them that the function of secretion is performed.

"The culs de sac in which they terminate, are variously shaped in the different glands; in some few they are globular, but generally the terminal dilatation is much less considerable, and forms a sort of vesicular extremity, just as the minute bronchial tubes terminate in the air vesicles. The lungs, indeed, present the most familiar and intelligible example of this structure; they may be considered as glands destined to secrete from the blood various gaseous bodies, or different species of air. The trachea is the great duct of the lungs, through which the gases so secreted find a vent, or are excreted. The bronchi, the bronchial tubes, and their subdivisions, are identical with the ramifications of the ducts, and the air cells represent the terminal culs de sac; in the air cells, the secretion of air takes place from their internal surface, and in the other glands, we have every reason to believe that the internal surface of the terminal cul de sac is the organ of secretion. Now, a membranous surface possesses the power of secretion only in consequence of its vessels, and we must therefore suppose that every where secretion takes place from the vessels distributed to the secreting surface. But as it is now acknowledged that vessels have no opening in their sides, and have no terminations but in veins, it is obvious that ultimately all secretions must take place through the parietes of vessels. The passage of fluids in the living body, through organized membranes much thicker than the delicate parietes of minute capillaries, might be proved by many facts, and can be exemplified with membranes entirely deprived of life by experiments similar to those of Dutrochet on endosmose and exosmose. Hitherto we have supposed the secreting vessels to be ramified on the internal surface of the culs de sac, and that the secreted fluids penetrate the vascular parietes and so find their way into the commencements of the ducts. If this be the case, we are not to conclude that it necessarily follows that we can therefore fill the ducts of the glands from the arteries which supply it with blood, for we know that our injections are stopped by obstacles which afford no obstruction to the passage of certain fluids in the living body; indeed it seems more than probable that when injections pass from the vascular system of a gland into its ducts (as from the vena portæ into the pori biliarii, &c.), it is always in consequence of the parietes of the fine vessels above-mentioned being ruptured, and not because any openings in the sides of the ducts, or any direct inosculations with the minute arteries, really exist in the living structure."

After this we are presented with an account of the discrepancies which exist between our author's hypothesis and that of Mueller; and the paper concludes with some able remarks on the structure of the testicles, the lymphatic glands, and the placenta.

There are one or two other articles among the original papers which we have not room to notice.

The bibliographical department of the journal seems to be fairly conducted; and the extracts, under the head

of scientific intelligence, are certainly well-chosen.

On the whole, this first number is a very favourable specimen of what may be done by the *elite* of the scientific force in Dublin. We have no doubt, but that by co-operating cordially and steadily for a season or two, and getting fair play from the public, they will attain, what we heartily wish them, all the success they merit.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

SANGUINEOUS TUMORS OF THE HEAD IN INFANTS.

Professor Græfe, of Berlin, states that he has lately met with nine cases of the above description. Where the tumor was not very large, it has been sufficient to apply a lotion, consisting of sal ammoniac ʒij. vinegar of squills ℥j. and six ounces of water; but where the extravasation has been more considerable, he has made a small incision with a lancet, and introduced a few threads of charpie into the wound, applying slight pressure by means of a compress and bandage. — *Journal für Chirurgie n. Augenherlkunde.*

LOCAL APPLICATION OF DOLICHOS IN PALSY.

M. Græfe, in his Clinical Surgery, also mentions that he has used the down of the Dolichos pruriens, as an external application to paralytic limbs, with advantage. His plan is to cover the part with a layer of the cowhage, laying paper over it, and maintaining the whole by means of a roller. After a few minutes a slight degree of itching is experienced, which gradually augments, and is soon followed by slight cutaneous inflammation. The application is then removed. After the inflammation has subsided the remedy may be used again, and a third time. According to M. Græfe, some of his patients experienced only a diminution of the paralysis; others were relieved only for a certain time; and some were permanently cured.—*Ibid.*

SINGULAR INSTANCE OF HERMAPHRODISM.

M. Geoffroy Saint Hilaire lately published the following account, taken from a Sicilian journal. Professor Ricco, in the course of last January, in dissecting a subject of his anatomical demonstrations, perceived that the genital organs presented such anomalies as to render it impossible to say whether the individual was of the male or female sex, notwithstanding that the deceased (who was about eighty years of age) had been married as a woman. Professor Sorrentino joined in the examination, and it was decided, after careful examination, that the external organs were those of a female, the internal those of a male. The parts have been preserved. A detailed account, with plates, is to be published. M. Geoffroy St. Hilaire says, that there is only one case of this kind on record, that of Hubert Jean Pierre, related in the Memoirs of the Academy of Dijon, vol. ii.—*Gazette Médicale.*

REPORT OF CHOLERA IN GREAT BRITAIN, UP TO FRIDAY, APRIL 13, 1832.

New cases in London since our last report	224
Deaths	113
Total number of cases in London since the commencement of the disease	2382
Deaths	1261
New cases in other parts of Great Britain since our last report	372
Deaths	187
Total number of cases throughout Great Britain since the commencement of the disease	10170
Deaths	3918

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

April 1832.	Thermometer.		Barometer.	
Thursday . 5	from 33 to 67		30·41	Stat.
Friday. . . 6	34	57	30·33 to	30·30
Saturday . 7	30	51	30·19	30·10
Sunday . . 8	33	54	30·10	30·11
Monday. . 9	30	57	30·11	30·16
Tuesday . 10	34	57	30·18	30·12
Wednesday 11	31	54	30·12	Stat.

Prevailing wind N. E. Except the early part of the mornings, generally clear.

Charles Henry Adams.

NOTICE.

It was altogether impossible to comply with Dr. Ayre's request, of giving immediate insertion to his paper on cholera. We have at least a dozen on the same subject lying by us, for one or two only of which we can make room.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 21, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Part I.—Lecture XXVIII.

Continued Fever, continued.

You saw, gentlemen, in the last lecture, that the *general* symptoms of fever might vary from those of the most powerful excitement, with sufficient strength, down to those of extreme depression, debility, and exhaustion, exactly as the constitutional symptoms may vary in inflammation from those which are always observed in active tonic inflammation, down to those which occur when the disease is attended by gangrene and complete prostration of strength. So with respect to the *local* symptoms. I mentioned that they existed chiefly in the head, chest, and abdomen; but they will vary from the symptoms of the most active tonic inflammation, down to the symptoms of mere irritation without any great marks of inflammation; and again, they will vary in other cases down to the most complete exhaustion of an individual part—down to gangrene. You may therefore, in fever, have general symptoms of every degree, from the most active kind, with strength, down to the most passive or atonic kind, with extreme debility—nay more, with putrescency. And with regard to local symptoms, they will vary from those of the most active kind, without much inflammation, down to complete loss of vitality—gangrene.

Morbid Appearances.—You may suppose, therefore, after death, that the appearances will vary exceedingly, and you will be prepared to expect that they are chiefly situated in the head, chest, and abdomen, occasionally in one of these parts, occasionally in two, and occasionally in all; and you will be prepared to expect that they will sometimes vary in all these parts, both in their positive degree and their relative proportion. You will also expect the appearances to vary from those of active inflammation, down to very slight marks of inflammation indeed, where the case has been one rather of irritation than of inflammation; and you will expect them to be those not merely of ulceration, but of gangrene, or an extreme degree of softening.

The morbid appearances are chiefly found in the head and abdomen, just as the local symptoms during life are principally situated there; and occasionally we find degeneration of the structure of parts, without much inflammation, exactly as I said was often the case independent of fever. We have softening, and various other changes, without any marks of inflammation at all proportionate to those changes.

Head.—We will begin with the head. We shall sometimes find in the brain more red points than usual; we shall sometimes find the arachnoid injected, both as to its superficial portion, and as to that portion which you know lines the ventricles. The veins and sinuses are frequently found turgid; and frequently there is too much serous fluid upon the brain and in its cavities. But you must remember that in all this there is great room for fanciful opinion. On opening the brain some will maintain that it is very healthy, and others will contend that there are more bloody points than there ought to be, that the veins and sinuses are too full, or that there is more serous fluid than natural. You must take care, therefore, to be quite sure that you are right, when you assert that any thing preternatural is found in the cavity of the head. It will continually happen in fever that there shall be rather more bloody points than usual, without the patient during life having shewn particular symptoms of an affection of the head. These appearances

will vary in different healthy individuals, exactly as appearances do in other parts of the body. To say that there are decided inflammatory marks about the head, we ought to see appearances respecting which no one would entertain a doubt. When two or three persons differ in opinion, we ought to be much upon our guard in supposing that there is really any morbid appearance. I know there is so much fancy in all these things, that it is absolutely necessary to exercise the greatest caution in drawing a conclusion. But sometimes you do find in fever more bloody points than natural in the substance of the brain; you sometimes see the vessels of the pia mater really in a state of congestion, sometimes the sinuses, and sometimes you find more fluid indisputably than ought to be there. This fluid is various in its appearance, as in all other cases of affection of the head. Sometimes it is clear; sometimes it is turbid; sometimes fragments of lymph may be seen floating in it; now and then there is even effusion of blood, or, at any rate, this serum is bloody. But very often in fever, after there has been strong phrenitis, or strong marks of excitement of the head, you will find nothing. I have repeatedly opened patients who have died with marks of affection of the head, in whom the appearances after death have been quite disproportionate to the symptoms which occurred during life; and others in which I doubt whether any one unprepared to expect to find any thing in the head, would have found any thing. Indeed, it is said by Andral, that the morbid appearances in the head are less than those in the abdomen; and I believe this to be the case.

Chest.—If the chest happen to be the part very much affected, and which is rarely the case, then you may have far more morbid appearances in the chest than in the head, notwithstanding many have said that the disease is situated in the head. If the thorax happen to shew marks of disease, you may find the lungs soft—you may find them filled with a frothy red fluid; and sometimes they will crepitate as in health, and sometimes not. Sometimes they will be soft, and quite impervious to the air, especially if there have been extreme debility; and sometimes, but very rarely, they are in a state of gangrene. It is very rare to find them in a state of solidification, or what is strangely called, *hepatization*. Because they are as solid as liver, this morbid state is named after that organ; but it is most improper, because no part can be converted into liver. It may have the common qualities of liver as to colour and consistence, but as to calling it hepatization, this is decidedly incorrect. This red *solidification* of the lungs, however, is rare. Now and then you will find them solid and grey in different parts—spots of grey solidification, and it is common to find an abundance of red frothy fluid in them. One of the most frequent appearances in the chest is redness and thickening of the bronchial tubes, because a slight degree of bronchitis is very common in fever. In almost every case, if you listen, it is said you may find more or less sonorous rattle, shewing a little bronchitis. Now and then the pleura is affected: an effusion is observed in the pleura, perhaps redness of the membrane, perhaps adhesions—at any rate an effusion of lymph. Now and then even blood is found. The appearances, in fact, bear an analogy to those which are found in the head. The blood which is found in the heart and large vessels is generally fluid, and of a very dark colour. As to the coats of the arteries being inflamed, that you will not necessarily find to be the case, though some have said that in fever they are always inflamed. The inner coats of the arteries now and then may be inflamed like other parts, but you may open scores of cases and find no such thing.

Abdomen.—If we come to the abdomen, we shall sometimes find marks of inflammation of the peritoneum, or at least of the sub-peritoneal cellular membrane, if not of the peritoneum itself, of the cellular membrane immediately under it, or we may find inflammation within the stomach and intestines. The intestines are in general contracted, whether they are inflamed or not, but inflammation in the intestines is very common, as well as in the stomach, and is situated in the cellular membrane under the muscular coat, or in the mucous membrane itself, in which we have inflammation and all sorts of appearances. It has been long known, that in fever the intestines are very frequently inflamed. Theophilus Bonetus, an old writer, says that inspections of those who died of intermittent fever shewed that the stomach and intestines were inflamed. *Anatome eorum qui febre maligna extincti sunt, docet ventriculum cum intestinis inflammari.* Bartholini bore testimony to the same fact. *In omni febri acuta, imminet ventriculi inflammatio.* Sydenham says that the intestines are frequently ulcerated in continued fever. The knowledge of inflammation existing in the intestines, and also other affections, is not altogether new, though the fact was neglected. When you come to open the intestines, you will frequently find very extensive redness, and the redness will sometimes end suddenly as inflammation of the skin will do, and sometimes it is lost insensibly in the surrounding parts. Occasionally you will find the redness extensive, running the length of a foot or so, but generally it is found merely in patches; occasionally this redness is arborescent, and has little red spots around it. The redness varies in hue, from a bright red to a brown, or even a pur-

ple shade, according to the degree of congestion; and sometimes it is a very dark red. In the continued fever the mucous coat is often thickened, and it may be thickened to a great extent or only in patches, and sometimes it is softened; and, indeed, the whole set of the coats of the intestines are sometimes in the same condition—softened. On the inner surface of the intestines—the mucous membrane—there are frequently small, red, conical elevations, pimples, like the fringes below the tongue, or little white conical projections, or with a central depression. These last appearances are observed much more in the inferior two-fifths of the small intestines than elsewhere; indeed, it is in this situation that we find the chief morbid appearances. In the colon they are generally pointed. Occasionally these white conical elevations are real pustules—or at least small follicles, containing real pus, so that you have what may be considered pustules.

The secretion of the intestines is, of course, diseased. You find the mucus of a thicker quality than it should be, sometimes almost as thick as fibrin, and sometimes it is bloody. The mucous membrane will frequently peel off, not from any affection of its own, but from the cellular membrane which attaches it to the next coat being so brittle, that this no longer forms a medium of union. Very frequently into the cellular membrane, under the mucous coat, is effused a quantity of blood; nothing is more common than to find ecchymoses—an effusion of blood into the cellular membrane, under the mucous. Ulcerations are continually found in the lowest third of the ileum, nearest to the cœcum; the nearer you come to the cœcum the greater is the degree of ulceration. These ulcers are frequently seen in the midst of red patches. I just now mentioned the frequent occurrence of red patches, and in the midst of these there is frequently ulceration, and then the surface beyond the patches again is frequently pale. It would seem that superficial inflammation had taken place locally, with great severity, and in the centre had proceeded to ulceration. But you will find these ulcers also frequently occur in the little pimples or pustules of the glands, so that you have in the intestines two kinds of ulceration,—the one superficial, giving you the idea of an abrasion, such as occurs in the mouth, throat, or on the organs of generation,—and the other occurring in the glands. These ulcerations are of all sizes and of all shapes. You sometimes see a portion ulcerated to a very great extent, and sometimes these ulcerations are sloughy, or at least you may detach a sloughy layer of something, perhaps sometimes a diseased secretion, and underneath you find an ulcer. Surrounding the glandular ulceration you will frequently see the mucous membrane more or less detached, and the ulcer of course extends to various depths. Sometimes it extends so deeply as to go through all the coats, and perforate the peritoneum. When this occurs, there is generally sudden peritonitis induced; pain is felt at a particular spot, and it darts from it in all directions, and speedy death generally ensues. Nature, however, sometimes prevents this, as I formerly said, by producing adhesion, or perhaps the perforation is blocked up by a piece of omentum, or something else; but if this do not occur, violent peritonitis takes place, though not always. Occasionally the inflammation which is produced, is of a slow, chronic form; the patient recovers from the fever, but continues to labour under peritonitis in a chronic form; occasionally without pain, but this is rare. Generally, when a perforation occurs in the peritoneum, violent peritonitis is induced, and the patient sinks. Now and then you will have an abscess in the substance of the intestines—in their cellular membrane, I presume. It occurs as it does in the cellular membrane any where else. The peritoneal and the mucous coats being sound, the matter is produced between them, and exists in the cellular coat. With respect to inflammation of the muscular coat of the intestines, I do not believe that such a thing occurs. Mucous, cellular, and serous membranes, are far more frequently inflamed than muscles; it is rare for muscles to be inflamed, and there is sufficient of these in the intestines to become inflamed without supposing that the muscular coat is the seat of inflammation.

The period at which these ulcerations may take place is very various. It would appear that occasionally they will take place rapidly, because persons have shewn them who have only had fever a couple of days; but frequently they occur slowly, and they are more frequently observed in persons who die of fever than of other complaints. The large intestines are seldom affected; at least far less so than the stomach or the small intestines; and of the latter, it is in that portion nearest the cœcum, the lowest third, the ileum, or at the utmost the lowest two-fifths, that you chiefly find these appearances. [The learned Professor then exhibited several plates, executed by Dr. Carswell, illustrative of the various morbid appearances he had described as occurring in the intestines.]

Now some have patronized the head, and determined that it shall be the seat of fever, and that fever situated there shall be inflammatory; whereas others have had a predilection for the abdomen, and have declared that it is seated there, and that it is inflammation of the intestines. It is

true you may find morbid appearances there, but it is also true that you may open cases of fever and find the intestines sound, or at least with no such appearances as will explain the symptoms of fever. I have inspected cases over and over again, in which there was no ulceration of the intestines—no inflammation; and in which, if persons had not been told it was a case of fever, they would not have known it. There might sometimes have been a little more redness in this intestine or that, or if even there were any thing morbid, still it was insufficient to account for the general symptoms of fever. The symptoms during life frequently depend upon local affection, but frequently you will find no disease existing in any particular part. Andral says (and I think him one of the soundest writers on medicine; he appears to have no theories, but to look out for facts, and make the most correct and philosophical use of them;—I confess that I have found all he has said in his Clinical Reports to be confirmed; and though it was not till lately that I read his book, yet I was delighted to find the coincidence in our facts and inferences)—he says, that of thirty-eight cases of fever which he examined, eleven only presented marks of gastritis enough to have influenced the symptoms during life; thirty out of the thirty-eight shewed some sort of intestinal affection, but only fourteen of these—that is, fourteen out of thirty-eight—exhibited morbid affection of the intestines which could explain any of the symptoms during life. He also says, that the changes which are seen in the nervous system are comparatively rare and slight. I do not think that fever is to be explained by morbid anatomy. Many of its symptoms, and many of the occurrences which take place, are to be thus explained: when there is phrenitis, you will usually have the marks of phrenitis; when there is bronchitis, you will usually have marks of bronchitis; when there is diarrhœa, you will expect to find ulceration of the intestines;—but frequently there is little or none of these symptoms, and after death there are no such marks as will explain fever, although there may be enough to explain the local symptoms that have occurred. Continually you will find in fever that the head is but slightly affected after the first few days; and continually you will find that the abdomen is scarcely affected at all.

I believe that in fever attended with extreme exhaustion, the muscles are generally soft and livid. The blood, I mentioned, is generally fluid and black in the large vessels, and the muscles are generally soft and livid; a fact altogether corresponding with the state of the blood. I mentioned, that if you analyze the blood, in a case of typhus fever, where there is great prostration of strength, you will find that it contains less saline matter than there should be; and it is said to contain less carbonic acid than natural; in fact, it is more like lymph than blood. The liver and spleen are rarely affected. The bile is often abundant, and sometimes very dark and thick; but in other cases it is just the reverse—pale and thin. Sometimes it is acrid, and sometimes not. The liver or spleen may exhibit marks of inflammation, or, if not inflamed, you may see the results of inflammation in a little suppuration, &c. They may be softened. But all these things are very uncertain. So far with regard to the symptoms of fever during life, and the morbid appearances after death.

Diagnosis.—As to the mode in which we form our diagnosis, and determine that the case is one of fever, it is, in a great measure, by observing that the constitutional symptoms are disproportionate to any local affection which may exist. The constitutional symptoms are, of course, influenced by a local affection, but they are out of all proportion to it, and frequently they are altogether of a different nature. For example, there is often no local sign of inflammation—or of any local disorganization to be discovered; but there is extreme debility from the very first—indeed, a sense of debility which is not observed in any mere inflammation, and the countenance, too, is peculiar—expressive of both anxiety and oppression; there is almost always, from the first, pain of the loins, and, nearly throughout the disease, tremor of the tongue. All these circumstances are very different from what we see in mere inflammation of the brain, or of the chest. You sometimes may have a local inflammation of the liver—a local inflammation of the stomach; but you have also disproportionate loss of strength, perhaps a feeble pulse, perhaps a putrescent state of the body, and symptoms of a different characer to those which arise from mere local inflammation. The countenance, however, and the sense of debility, from the very first are very characteristic.

Prognosis.—As to the prognosis, of course it must be taken, in the first place, from the severity of the disease; but you need not give an unfavourable prognosis because the constitutional symptoms of excitement are very violent: mere general excitement is by no means dangerous, but when it is accompanied by extreme local excitement, marks of severe local inflammation, then there is reason to apprehend danger. As long as you can discover no great marks of inflammation in the head, chest, or abdomen, the general excitement, however violent, is not in itself dangerous. But although you have no right to infer danger from extreme excitement when there is no great local affection, yet

symptoms of an opposite description are always dangerous; in proportion to the intensity of the signs of debility, is the degree of danger. Intensity of general excitement is not alarming in proportion to the excitement, but the danger is proportionate to the marks of real debility.

Now the marks of debility in fever, are great rapidity and weakness of pulse. Andral says that he never knew a person recover from fever whose pulse exceeded 140; but I believe it is well known that recovery will take place after a pulse of 160, however dangerous such a pulse may be. Dr. Heberden mentions recovery from fever after a pulse of 180. If rapidity of pulse be accompanied by weakness, then of course the danger is so much the greater. Involuntary discharges of the urine and fæces likewise shew great danger, because they prove extreme debility, or extreme local affection, of the head or abdomen. They will arise from the patient being unconscious of what he is about, from being in a state of stupor; and they will also arise from the patient being too feeble to make an effort to restrain them; either of which conditions is extremely dangerous. You will learn much, too, from the position of the patient. When a patient lies on his back, there is more danger than when he lies on his side, a greater effort being required to maintain the latter posture; but if the patient sink in his bed, the danger is greater still, for it then shews that he has very little power at all. A person can exert the muscles of deglutition long after he has lost control over those of the trunk, and an inability, therefore, to swallow is one of the most dangerous symptoms. The countenance, I have already intimated, is a good index to the degree of danger. Blackness of the tongue and blackness of the teeth is also more or less dangerous. We see patients recover every day whose teeth have been covered with sordes, and whose tongue has been black, but still, of course, it is a bad sign. The abundance of the discharges, too, is generally dangerous, unless the symptoms remit; and the danger increases in proportion to their depraved nature. Another very unfavourable symptom is the discharge of blood, or its effusion under the skin, forming petechiæ, vibices, and ecchymoses. Of course, the larger these spots the greater the danger, vibices being more dangerous than petechiæ, and ecchymoses more dangerous than vibices: still, however, the presence of petechiæ in itself is not dangerous, for some epidemics are characterized by it; but if it be very considerable, and accompanied by other marks of exhaustion, then it is dangerous. Hiccup, too, is a dangerous symptom, on account of it usually coming on towards the fatal termination of the disease; but, now and then, fever may attack dyspeptic persons, and you may have hiccup, not so much from fever as from the disposition to it, occasioned by the habitually weak stomach. Early debility is a very unfavourable sign. Debility, after the disease has existed for a fortnight or three weeks, would not be so dangerous as the same degree of debility at the beginning of the disease, because, if the affection be to last for a fortnight, and you have at the beginning the same debility which you have in other cases at the end of a fortnight, when that period arrives the debility may be such that life may be extinguished. The more intense the debility in the early stage of the disease, the greater is the degree of danger that is always to be apprehended. When a person has violent pleuritis, peripneumonia, enteritis, peritonitis, or any other local inflammation, of course the danger is aggravated, and it frequently rests solely upon this local affection. The age and constitution of the patient are important considerations. Individuals of bad habits, who have been badly fed, or addicted to the vice of drinking, or have suffered from the want of proper rest, are of course very liable to fall victims to the disease. We must always consider the character of the epidemic; for if we know it to be of a dangerous nature, we should always give an unfavourable prognosis even at the outset, before dangerous symptoms have come on. We are sometimes obliged to give an unfavourable prognosis from the state of the patient's mind. I have often seen patients die of fever solely because their mind was uneasy. I have seen two or three patients within the last six months who in all probability would have recovered from fever, had it not been for an unfortunate state of mind. If a patient have made up his mind that he shall die, or some real calamity presses upon him, then the medical man has a far less chance of success than if his mind be in a state of happiness and tranquillity. If you know this to be the state of the patient's mind, you should speak with more caution than you otherwise would, even though things are going on favourably; for you have an additional enemy to contend with. You must also give a prognosis with reference to the power which you have of doing your duty. Sometimes through the whims of friends, or the unwillingness of the patient, you are not allowed to do what you desire, and sometimes you are prevented by another practitioner from being quite as active as you wish in the use of means. These things are all to be taken into the account.

As to the grounds of a favourable prognosis, I need not say that they are the decline of all the symptoms; but you must be on your guard when the symptoms are generally declining, and ascertain whether all is

well throughout the body,—whether there may not be some local circumstance still existing from which danger may be apprehended. For example, fever will sometimes decline when the loins slough, and the patient may then die of exhaustion. A return of strength is one of the best signs. When a patient no longer sinks in bed, but is able to lie on his side, then the prognosis is favourable. You may judge much from the countenance and from the feelings of the patient himself. It has been thought that deafness is a favourable sign; at any rate, it is *not* an *un*favourable sign. Many authors have thought it favourable, but why it should be so, I do not know. In many cases of fever, patients become very deaf, and they recover just as though this circumstance had not occurred. The return of all the feelings of the patient to a state of health, must contribute to a favourable prognosis. Dr. Gregory used to mention as an instance of this an amusing circumstance, showing not that the stomach had returned to its duty, but that other parts of nature were also improved. On the doctor visiting one of his hospital patients labouring under fever, the man told him that he was much better, and added, that he longed to have his mistress: and some wag put down in the prescription-book, "Let him have his mistress." Dr. Gregory was too good-natured to be offended at any little joke of that sort, and used to mention it year after year with the greatest glee.

Predisposing Causes.

I will not enter upon the exciting causes of fever to-day, but will speedily run over the predisposing causes, and the first I will mention is mental depression. Many persons do not become the subjects of fever, though they are exposed to all the exciting causes, till their mind is depressed. I have known many persons go year after year to spots where fever prevailed from some local circumstance (I will not now enter upon the question of contagion) with perfect impunity, till some calamity happened which greatly depressed their minds, and then they became the subjects of fever. It has been observed with regard to the plague, that persons have not fallen victims to the disease till they have lost a relative, or been cast down in spirits, and then they have sickened immediately. Diembroek mentions an instance of this kind with regard to the plague. A person escaped it till he saw a funeral pass by, and, on inquiring whose it was, he found that it was one of his dearest friends. He went home, sickened of the plague, and died. The same circumstance occurs with regard to fever and all other complaints. Anxiety of mind will have the same effect;—downright grief, or anxiety lest misfortune should occur. Corporeal depression and over exertion of mind will have the same injurious tendency. Over muscular exertion, excess of venery, and debilitating circumstances of all descriptions, whether of mind or body, will lay the foundation for fever. Want of food and bad food are both predisposing causes. Famine and fever generally go together, and hence, perhaps, we pray in the same breath against "plague, pestilence, and famine." Want of fresh air also predisposes to it. Adult age, or, at least, the vigorous period of it, appears the most inclined to it; infants and very aged persons less frequently have fever than others. It is in adults, not aged, that we most frequently see fever. Besides this, there appears to be a peculiar susceptibility to it in some persons; they will, with no evident reason, become the victims of fever when placed under the same circumstances in which others escape.

ON THE COMPARATIVE VALUE OF IODINE AND VARIOUS IODURETTED PREPARATIONS, AND THE SULPHATE OF QUINA,

In Strumous Inflammatory Affections of the Eye.

By R. Middlemore,
Assistant-Surgeon to the Birmingham Eye Infirmary.

Many years prior to the appearance of M. Lugol's work on the effects of iodine in scrofulous diseases, I had employed that substance in various strumous affections of the eye, with, in the general, a certain degree of advantage. On the occasions to which I am referring, I had merely prescribed the tincture, taken in a little sugared, or distilled water, two or three times a-day, continuing its use for that length of time, when it appeared either to limit the increase of the disease, to afford some degree of mitigation of its severer symptoms, or to be succeeded by more decidedly favourable effects, or, on the contrary, to be inadequate to the production of any benefit whatever. The results of the trials thus made were so very unsatisfactory that I have always omitted to mention the use of iodine among the more important remedies which, on several occasions, I

have ventured to recommend to the attention of the profession through the medium of your journal, and that of the Midland Reporter, for the cure of the different forms of scrofulous inflammation of the eye. On the appearance of the valuable work of M. Lugol, and on carefully perusing the evidence brought forward by that author in favour of the administration of iodine in a particular form, in various scrofulous affections of the eye, and also the use of a collyrium containing the same substance, I felt anxious to determine how far the author's estimate of its value was correct; or rather, how far the use of the same substance in my own practice, employed in precisely the same manner as he has advised, was corroborative of the high opinion he has expressed in its favour. And I may here remark, that in order to possess the best opportunities for observing in the least hasty and unembarrassed manner the various important maladies of the eye, and noting accurately the effects of particular remedies upon them, I have been in the habit of permitting a certain number of patients to receive gratuitous attention at my own house, which, in addition to my infirmary practice, has presented me with at least a larger field of observation than under the ordinary circumstances of practice would have fallen to my lot. I hope it will be understood that I am only mentioning this circumstance to ensure some attention to my present communication, and to obtain for my opinions the dispassionate exercise of the judgment on the part of those who may feel inclined to imagine that I am dissenting from the opinions of a learned, zealous, and indefatigable member of our profession, without having given them due and adequate consideration.

It appears that M. Lugol has only recommended the administration of iodine, or the use of various ioduretted preparations, in what he terms scrofulous ophthalmia; and as far as I can ascertain from his translated work, has not distinctly defined any affection of the deep-seated textures in which the use of iodine has been succeeded by any advantage. I shall therefore assume, in the course of my remarks, that he has not extended, nor meant to recommend, the use of iodine, in its various forms, in any other inflammatory affections of the eye than those of the cornea, the conjunctiva, and the sclerotica. I may be permitted to remark, however, that the following extract from his translated work does not evince any very well-defined acquaintance with what are usually phrased scrofulous inflammations of the eye. "It being also well known that this ophthalmia (purulent scrofulous ophthalmia) *not unfrequently causes the destruction of the eyes in a few days*, and from my conviction that the disease was here of a scrofulous nature, I did not hesitate to have immediate recourse to energetic *local* treatment with iodine. Whenever scrofula exists, iodine must be called in to our assistance*." A purulent ophthalmia occurring in an individual sixteen years old, attended with great intolerance of light, and "hypertrophy of the soft parts, (excessive chemosis?) and characterized by its capacity to cause the destruction of the eyes in a few days," is not usually considered a scrofulous affection. It is, of course, very possible that acute catarrhal, gonorrhœal, or simple puriform inflammation of the conjunctiva, may supervene upon a strumous condition of the system, but the disease in neither of these instances would be termed scrofulous, nor treated otherwise than as the severe character of the local inflammation would require. We will suppose that a scrofulous subject becomes affected with gonorrhœal inflammation of the conjunctiva, which is exactly similar in its characters to that form of ophthalmia referred to by M. Lugol in the above quotation from his work—that is, it is an acute inflammation of the eye, attended by great intolerance of light, excessive chemosis, profuse puriform discharge, and disposed to cause the destruction of the organ in a few days: surely in such a case no one, with the exception of M. Lugol, would rely solely on "energetic local treatment" to effect a cure. A state of things such as that described by M. Lugol in his second memoir, Case 6, is one of a most severe character, and if it were capable of being cured by local baths of iodine, and the use of the ioduretted solution, I, for one, should feel much indebted to the discoverer of a means of curing, by such extremely mild measures, so acute and formidable a disease—a disease

* Lugol on Scrofula. Translated by W. B. O'Shaughnessy, M.D. page 102.

which generally calls for the adoption of the most active and energetic depleting, and other severe measures.

I may here remark that M. Lugol relies on the following preparations of iodine for the cure of what he terms scrofulous ophthalmia. 1. The ioduretted mineral water, composed of three-quarters of a grain of iodine, one grain and a half of the hydriodate of potash, and half-a-pint of distilled water: two-thirds of this quantity is to be drank by the patient during the day. 2. Iodine solution, containing two grains of iodine, four grains of the hydriodate of potash, and a pint of distilled water: this he directs to be injected beneath the eye-lids. 3. Ioduretted baths, prepared by mixing two drachms of water with half an ounce of the hydriodate of potash, and a sufficient quantity of water to form an ordinary bath. These are the remedies proposed by M. Lugol for the cure of those forms of strumous inflammation of the eye to which I have already referred, and he represents the curative process to occupy a period varying from six to twenty-one days. The mere announcement of this fact is sufficient to tempt any one to make trial of the measures he recommends, who has the management of children suffering from scrofulous inflammation of the eyes, which is, like scrofulous disease in other parts, generally tedious in its duration, and characterized by a strong disposition to relapse when it has once existed in a decided form.

I have adopted all the means of cure recommended by Lugol, with the exception of the ioduretted bath, which he does not represent as being essential to the cure of strumous inflammatory affections of the eye, in scrofulous inflammation of the conjunctiva, of the cornea, of the sclerotica, of the membrane of the aqueous humour, of the iris, and lastly, in instances of scrofulous photophobia simply; and although they have certainly appeared to exercise a beneficial influence upon the disease when situated in the superficial textures, no amendment whatever was perceived when given in the deep-seated inflammatory affections of the eye. I have cured many cases of strumous conjunctivitis by iodine merely, but the capacity of iodine to cure scrofulous conjunctivitis is not alone sufficient to entitle it to general adoption in preference to other remedial agents, which not only effect the same object in a much shorter space of time, but afford a more or less complete immunity from a recurrence of a similar disease. In endeavouring to arrive at any correct conclusions upon the subject, I have thought it highly important to fix upon some form or class of strumous individuals, to whom the administration of iodine in its various forms might appear to be best adapted; and with this view I selected those who were characterized by the delicacy of their organization,—those thin, weak, irritable children, who have generally a finely-tinted cheek, light hair, blue eyes, thin, or as it is figuratively termed, transparent skin, who possess in the general a remarkable precocity of intellect, and whose scrofulous diseases are generally developed in the eyes, and in the chest: when in the former, consisting, for the most part, of an affection of the conjunctiva, the membrane of the aqueous humour, or the iris. These subjects were certainly rendered worse by the use of iodine, or in a few instances, their disease remained stationary. The other classes of strumous subjects were severally tried, and the result of such trials elicited the fact, that those scrofulous children who possess no peculiar delicacy of form or organization, but who, on the contrary, are strongly and inelegantly formed, whose cheeks are often of a dark and uniformly red colour, whose eyes and hair are black, and whose mental manifestations are of the dullest and most inactive character, and lastly, whose scrofulous diseases chiefly consist of thickening of the upper lip, hypertrophy of the alæ of the nose and tips of the ears, enlargement of the extremities of the bones and of the lymphatic glands, with a thickened state of the tarsal margins, and an inflamed condition of the conjunctiva and of the meibomian glands, are those upon whom iodine exerts the most beneficial influence of any class of strumous subjects when administered for the purpose of curing some inflammatory affection of the eye.

Again, in endeavouring to render my conclusions as accurate and complete as possible, I selected those cases in which the disease was situated in a particular texture, to discover how far the administration of iodine was adapted to the inflammatory state of the eye when confined to some one part of that organ;

so that taking this fact in connexion with the condition of the physical organization, and mental development or manifestation, I might be able to point out more precisely than Lugol has done the circumstances connected with the strumous inflammatory affection of the eye, which called for the use of iodine; and with this object, and not with any desire to attempt to diminish the well-earned, and sufficiently established reputation of M. Lugol, I commenced the inquiries I am about to detail, taking as my text-book the work of M. Lugol, so well translated by Dr. O'Shaughnessy. If M. Lugol's representation of the efficacy of iodine in the particular class of diseases I am now engaged in considering, had been verified by the trials already referred to, his discovery would have been so important to the interests of the institution with which I am connected, that I should at once have recommended its introduction into our practice if no similar suggestion had been brought forward by my colleagues; for although* we experience little difficulty in curing a vast majority of the strumous inflammatory affections of the eye by the administration of the sulphate of quina, the great expense of that medicine is a serious drawback on the funds of our infirmary, and it would, of course, have been a source of great pleasure to have been been able to effect the same amount of benefit by less costly means. The use of the sulphate of quina in various forms of strumous inflammation of the eye, is now pretty generally adopted as a primary part of the treatment in those cases where symptoms of great debility exist; and without arrogating to myself at all unduly, I may be allowed to say, that several medical gentlemen, with whom I have no personal acquaintance, have informed me by letter that they were first induced to try it when the deep-seated textures were affected in such subjects by the perusal of my evidence in support of its paramount utility†. The comparative ease and facility with which the administration of this excellent medicine effects the cure of strumous diseases of the eye, has prevented me from furnishing you with other evidence than my own experience affords in proof of the very secondary and insignificant utility of iodine in the same affections. Without lengthening my remarks by the detail of cases, I will furnish you with a summary of the conclusions at which I have arrived, after having tried both the iodine, in its various forms, and the sulphate of quina, in cases of strumous disease of the eye, presenting, as nearly as possible, the same characters, and occurring in individuals closely allied in respect to their constitutional conditions.

1. Neither the external use, nor the internal administration of iodine, has appeared to exercise any control over strumous iritis, or strumous inflammation of the membrane of the aqueous humour.

2. Iodine, in the form of lotion, as advised by Lugol, has evidently relieved some cases of strumous conjunctivitis, and the cure of others has been accomplished by the use of the iodine collyrium, combined with the administration of the ioduretted solution; but the cure has proceeded more slowly, been accomplished less perfectly, and relapses have occurred more frequently, than when the removal of the inflammation has been effected by the administration of the sulphate of quinine.

3. In those cases of strumous corneitis in which I have prescribed the foregoing preparations of iodine, relief has been sometimes experienced, a cure rarely accomplished, and in the majority of instances no benefit whatever has resulted from their use. This disease is readily cured by the use of the sulphate of quina, under the circumstances pointed out in my last communication upon this subject.

4. And, finally, with regard to the effects of iodine upon the constitution of children suffering from scrofulous inflammatory affections of the eyes, I may state that it is much less beneficial; it does not afford that vigour of system, that increase of strength and flesh, that keenness of appetite; and, finally, it does not give the patient an equal immunity from a relapse of the disease, which the administration of the sulphate of quina so generally produces.

Now the conclusion at which I would arrive from the preceding statements is a very obvious one,—the use of iodine should not supersede the administration of the sulphate of quina in those strumous affections of the eye in which it

* Such, at least, is my own opinion, and I believe it to be that of my colleagues.

† See London Medical Gazette, vol. viii.; and Midland Reporter, No. 14.

has been hitherto so generally employed with advantage, when such affection of the eye exists, as the only local indication of a strumous diathesis.

But I am by no means desirous of restricting the treatment of strumous diseases of the eye to the administration of the sulphate of quina; on the contrary, I am fully convinced that it ought in many cases to be associated with other remedial measures, and in some instances should give way to more active remedies, such, for example, as bleeding and the administration of mercury. I have pointed out at some length, in my former communication, the circumstances connected with strumous children when the subjects of scrofulous disease of the eyes in its various textures, which would regulate the administration of the sulphate of quina; and I endeavoured in that communication to direct especial attention to the constitutional condition of such children, and to draw particular notice to the occasional occurrence of scrofulous disease of the eye, in its most decided form, in children who had none of the emphatically marked characters of constitutional scrofula, and also to the existence of common inflammation of the eye arising from accidental violence, and other causes, in children who were the obvious and unquestionable subjects of scrofula, according to the generally accepted definition of that disease. But I did not at that time fully explain the distinction between scrofulous inflammation of the iris and strumous inflammation of the membrane of the aqueous humour, and those other forms of these two diseases which require, not a stimulating, but a depleting, and often a mercurial course of treatment. However, before I enter upon the consideration of these subjects, I will just state what was the general mode of managing strumous iritis prior to the introduction of the sulphate of quinine, as one of the remedial agents employed for the cure of that, at the period to which I am referring, very obstinate and protracted disease. The few authors who have considered the strumous among the other varieties of iritis*, have in no instance recommended the administration of quina for its cure, but, on the contrary, have advised depletion and the use of mercury, given to the production of ptyalism. I had formerly many opportunities of observing, that when scrofulous children were the subjects of iritis, by which term I understand an inflammation commencing in, or extending to, the proper iritic substance which is enclosed by the serous membrane lining the chambers of the eye, the disease was often extremely obstinate—was not cured by the use of mercury, although its action was fully produced with the same degree of certainty with which its other more severe forms were controled, and very frequently relapsed. And I observed, also, that some of the parents of those children, who had been little or not at all amended by a long course of medicine had, tired of continuing medical treatment any longer, taken them into the country, from whence they sometimes quickly returned perfectly cured of the inflammatory affection of the eye. A moment's reflexion on the phenomena belonging to other forms of iritis, such, for instance, as the simple idiopathic or syphilitic, convinced me, either that the state of the iris, which I had termed strumous iritis, in scrofulous children, was not, as had been imagined, a condition of inflammation, or that there existed some circumstances of which I had previously a most imperfect acquaintance, which very materially modified its characters and history as contrasted with those of other forms of iritis. On referring to the phenomena connected with simple acute idiopathic iritis, I observed, that soon after the accession of the inflammation, the iris became dull, pretty uniformly and equally dull, being quite destitute of its natural polish, but that this dullness was soon superseded by a change of colour, which was not general; it was not, as soon as observed at one part, witnessed over the whole of its surface; it was first evinced at the pupillary margin of the iris; it was next observed at its ciliary border; and it was finally noticed in the intermediate part, at which stage the iris was pretty equally and uniformly discoloured. Now it will be seen that the earliest changes in the colour of the iris correspond to the situation of the two arterious circles which are formed in that membrane, and which are so beau-

* In the admirable essay of Schmidt, which appeared in 1801, the first, and one of the best dissertations upon iritis ever published, no mention is made of strumous inflammation of the iris.

tifully delineated in the splendid works of Sœmmering and Zinn. The minute vessels of the iris being principally concerned in its inflammation, are of course producing the effects of that changed state and altered action which the circulating system of parts, under an attack of inflammation, experience, in the most extensive and unequivocal manner in that situation, where they exist in the greatest profusion. This change in the colour of the iris, which is not always discriminated from a loss of its natural polish, evidently depends, not on an accumulation of, or change wrought in the blood contained in its vessels—not on any impediment to the free transmission of that fluid, but on the secretion or formation, and the effusion, of some new material into the proper texture of the iris, and is only most early developed in its ciliary and pupillary border, because the minute vessels, on which such changes depend, are most abundant there, and first experience the effects arising from an increased plenitude of, and (though only for a short period) an augmented action in, the larger series of arteries. It is evident, that before the discoloration in question can take place, the attack of inflammation must either be very active or long-continued, just in the same way as a similar inflammatory deposition occurs in other parts, only under the latter circumstances.

We know that the effect of mercury (when properly administered under favourable circumstances) upon an inflamed part, in which the deposition of lymph has just commenced, is, first to arrest its further deposition; and, secondly, to promote its absorption; and in the active form of simple idiopathic iritis, we have an excellent opportunity of witnessing these effects in the most clear and unequivocal manner. For in such a case we observe, that as soon as the action of mercury upon the system is fully evinced, the further deposition of lymph ceases, the pink circle around the margin of the cornea is rendered less dense and distinct, and that afterwards the colour of the iris returns; not, however, last in its ciliary and pupillary borders, corresponding to its arterious circles, but pretty nearly at the same time in every part. And we know that this return of colour is occasioned by the absorption of the lymphatic particles with which the iritic substance was blended, because we sometimes find, that at the same time that the iris becomes discoloured, lymph is effused upon its surface, suspended from, or attached to, its pupillary border, or deposited in the anterior chamber; and in such instances, the return of the colour of the iris under the influence of mercury corresponds with the absorption of this effused lymph.

We have thus seen that a diminution of brilliancy is one of the earliest symptoms of acute idiopathic iritis, and that the iris loses its polish before it becomes discoloured; that when it does change its colour, that change is first evinced at its pupillary, then at its ciliary border, and lastly, at the intermediate space; that such discolouration depends on some changed state, or altered action, of the capillary system of the part, whereby a lymphatic substance is secreted, and mixed with the iritic substance; and lastly, that mercury, given to the production of its specific effect, first changes the action of the vessels engaged in secreting this lymphatic product, so that it ceases to be effused; and secondly, promotes the absorption of that which had been previously effused.

On reviewing these circumstances, it will be perceived that there is a distinctly-marked stage of iritis, during the continuance of which no actual discolouration is witnessed; it has lost its brilliancy, its action is impaired, and the pupil is contracted, but its colour remains just as it was prior to the accession of the inflammation; and this stage may be of longer or shorter duration, as the degree of inflammation may be more or less intense. In common idiopathic inflammation of the iris, it is usually very brief, but in strumous iritis it is very much prolonged, and the iris may then be, and very often is, inflamed for a long period before any change whatever is observed in its colour. Now, when we consider the intention with which mercury is administered in the first of these forms of iritis, (idiopathic) we shall at once perceive that its action is much less requisite in strumous inflammation of the iris than in common idiopathic inflammation of that membrane. Our object is first to arrest the deposition, and secondly, to promote the absorption of lymph, the effusion of which is evinced

by a certain change in the colour of the iris. But the iris is not generally discoloured when affected with strumous inflammation unless the disease has existed for a long period, has frequently relapsed, is unusually severe, or is less decidedly and emphatically marked than usual. Hence we deduce a strong argument against the general use of mercury to the production of ptyalism in strumous subjects when affected with iritis, as distinguished from the evident propriety of its administration in almost all its other forms. But there are many more cogent arguments against the propriety of its administration to be found in the other circumstances, and particularly in the constitutional condition of those children who are usually the subjects of strumous iritis*. The administration of mercury in inflammation of the iris is then only imperatively necessary when it is characterized by certain circumstances, (I chiefly allude to change in its colour) which are generally absent when that inflammation is modified by struma. I have seen many cases of strumous iritis which, from the frequency of their recurrence, and the length of their duration, have effected very important changes in the colour of the iris; and yet the enfeebled and the emaciated state of the individuals so affected has prohibited the administration of mercury, and many of these cases have terminated favourably under the use of the sulphate of quina, with the adoption of other subsidiary measures; but the natural colour of the iris has not usually been restored; and this leads me to inquire how far the action of mercury does, under ordinary circumstances, influence the restoration of the iris when altered by inflammation. It has been repeatedly stated by many experienced surgeons† that mercury does not assist the restoration of the natural colour of the iris when changed by inflammation, and they account for its frequent return under the use of mercury by supposing that the administration of that medicine arrests the inflammation, upon which the deposition of that substance which produced the discolouration depended, and that the unaided efforts of nature accomplish the restoration of the colour of the inflamed part. I do not know how this question can be decided by the experience of any one individual, but certainly, as far as my observation has hitherto extended, many of the cases which have fallen under my notice, where one or both the irides have remained permanently discoloured after an attack of inflammation, either no mercury whatever, or mercury administered to a very trifling extent, had been employed for its cure. I admit that the natural actions of the part will sometimes restore the colour of the iris when changed by inflammation, but I am convinced that it is much more likely to be restored if mercury be freely administered.

It does not appear that an iris, which is simply discoloured from an attack of iritis, interferes, merely on account of such discolouration, with the perfection of vision; it may act as freely and perfectly as its fellow, which, I am presuming, to be in a natural state, and vision may be just as good as that of the opposite organ, but it gives a querity to the countenance which few persons are desirous of possessing. However, it is not right to limit the use of the sulphate of quina to those cases of strumous iritis which are not characterized by discolouration, for we do not want illustrations of the fact, that the administration of mercury is not indispensably required for the perfect cure of the most acute forms of inflammation of the iris*; but, undoubtedly, it is prudent and judicious practice to administer mercury, whether strumous or not, whenever it has proceeded to this extent, provided that the strength and constitutional condition of the patient will enable him to sustain its operation without material detriment, and that no circumstances exist, which, on general principles, forbid its use.

I shall not enter upon the other parts of the treatment of scrofolous inflammation of the iris; for, my object in this communication is chiefly to represent

* London Medical Gazette, vol. viii. p. 545.

† I do not allude to the class of persons termed oculists; for, with a knowledge that many of the most important diseases of the eye depend on either a disordered or a diseased state of the brain, the stomach and bowels, the uterus, &c. or on various constitutional maladies, such as scrofula, syphilis, gout, and rheumatism, we cannot but consider that man as a mere charlatan who pretends to understand, and to treat in a superior manner, the morbid affections of the eye without a due acquaintance with general pathology and therapeutics.

* See Dr. J. Thomson, in the Edinburgh Medical and Surgical Journal, vol. xiv.; and Observations on the efficacy of Turpentine in the Venereal and other deep-seated Inflammations of the Eye, by Hugh Carmichael.

the superiority of the sulphate of quina over the use of iodine, in the various strumous affections of the eye, and to point out the more important circumstances in the history of scrofulous inflammation of the iris, and its more acute forms, by reason of which the sulphate of quina is so well adapted to the former, whilst, in the latter, the administration of mercury, if not imperatively necessary, is by far the quickest and the most certain means of preserving the integrity of the part, and effecting the perfect restoration of sight.

In the course of my remarks I have frequently referred to the two arterious circles of the iris, and I will just again direct attention to this important circumstance as connected with the anatomy of this delicate part. It was my intention to have sent with this paper a drawing, illustrating the arrangement and profusion of the vessels of the iris; but it is impossible for me to exhibit a clearer or more accurate view of this subject, than may be obtained by any of your readers, by referring to the following figures, in table iii. in that excellent work of Zinn, entitled "Descriptio Anatomica oculi humani, iconibus illustrata:" — Fig. 2, "Arteriæ ciliares longæ et breves; circulus iridis;" Fig. 3, "Portio annuli minores iridis, ope microscopii visa et delineata."

It is quite evident from an inspection of these figures, and, indeed, from the examination of any well injected preparation, that the iris possesses a degree of vascularity much beyond what is required for the mere purposes of nutrition and secretion. In fact, the zonular arrangement and the great abundance of its vessels, appears to be a means of providing for its due supply of blood, under the varying circumstances connected with its mode of action and its degree of expansion; for, when the pupil is suddenly contracted, after having been for some time in a state of extreme dilitation, it is evident that the vessels of the iris would present an obstacle to the contraction of the pupil, unless they possessed a remarkably slight power of resistance, compared with that possessed by arteries in general, and would also be rendered, by elongation, too minute to convey an adequate supply of blood to the texture of the iris. We admit, therefore, that the peculiar zonular arrangement and great profusion of the vessels are necessary, not so much for the mere purposes of nutrition and secretion, considered abstractedly from its varying degrees of expansion, but chiefly to permit the most perfect freedom of circulation, under those varying states of the iris which the nature of its functions renders necessary. And this leads me to remark, that the contracted state of the pupil, under an attack of iritis, may take place, not solely, as has been represented, in obedience to the state of the retina, which is increased in its degree of sensibility to light, when, and because, the iris is inflamed, but chiefly because this expanded state of the iris (contracted state of the pupil), by elongating its vessels, diminishes their capacity, and thereby prevents the admission of a certain portion of blood into their cavity. Whenever the sensibility of the retina to light is increased, the pupil contracts to limit the quantity of light admitted into the eye, and there is present, of course, an evident degree of photophobia; but, in some cases of iritis, although the pupil is contracted, there is no intolerance of light. Now, if the pupil were contracted whenever the iris became inflamed, because the sensibility of the retina to light was then increased, which increase of sensibility was evinced by a certain degree of intolerantia lucis, this intolerance of light ought always to be present in iritis attended by a contracted state of the pupil, which is not, by any means, invariably the case.

EXTIRPATION OF THE EYE IN CASES OF FUNGUS HÆMATODES.

To the Editor of the London Medical Gazette.

Sir,

In Vol. VI. of the Medical Gazette, page 848, your correspondent, Mr. Middlemore, makes the following remark:—"We find that the eye has been successfully extirpated, when affected with fungus hæmatodes, sufficiently often to justify the practice even at an advanced stage of the disease."

This statement is so contradictory to what I have been taught in lectures, and have read in books, and the question of the utility of extirpation in fungus hæ-

matodes of the eye is so important, that I should feel obliged to Mr. Middlemore, or to any other of your readers, by a reference to the successful cases, if they have already been published.

I am, sir,
Your obedient servant,
A STUDENT.

OF THE

AFFUSION OF A STREAM OF COLD WATER ON THE HEAD,

In the Treatment of Various Diseases.

BY JAMES COPLAND, M.D.
Consulting Physician to Queen Charlotte's Lying-In Hospital, Senior Physician to the Royal Infirmary for the Diseases of Children, &c.

[Concluded from p. 43.]

III.—*Diseases in which cold affusion to the head has been found successful by the writer.*

FROM what has been stated respecting the mode of operation of the cold affusion on the body, and of the intentions it is calculated to fulfil, the advantages of the practice, in all diseases characterized either by augmented action of the vessels of the encephalon or by congestion of those vessels, must be obvious. In the former the affusion will be directed conformably with the explanation given above, until the symptoms are mitigated—until increased temperature is brought down, and the action of the carotid arteries is diminished; in the latter it will be employed momentarily, or for a very short time, and so managed as to produce a shock to the nervous system calculated to restore the tone and action of the vessels. The observations I have already offered on the mode of operation of this agent, according to the different ways of employing it, will be sufficient to guide the inexperienced in resorting to it in various diseases seated in the head. The benefits derived from the practice, when judiciously directed, in the early stages of *fever*, attended with symptoms of increased action in the brain—in inflammation of this organ, or of its membranes—in the form of active congestion constituting a variety of the apoplectic state—and in several convulsive seizures—are too obvious to require illustration.

The good effects of frequent cold ablutions of the heads of patients subject to *epileptic convulsions*, have long been recognized: but I believe that the cold affusion to the head, during the paroxysm, has scarcely ever been resorted to. I have usually prescribed it, both in cases of epilepsy and hysteria, for a number of years, and always with benefit. It has generally tended to diminish the duration and frequency of the epileptic paroxysms; even when it failed, with the other remedies resorted to, entirely to remove the disease: and the comatose state usually terminating the fit has been either altogether averted or rendered uncommonly short and slight by it. The advantages to be derived from this, as from other means, chiefly depend upon the pathological conditions of particular cases, and upon the appropriate employment, in conjunction with the practice here recommended, of the various methods of treatment usually resorted to against this affection.

Puerperal convulsions are at all times a dangerous disease, requiring the utmost decision of the practitioner. Several cases of the disease have come before me, and, in all that I have seen within the last twelve years, cold affusion to the head and blood-letting have been employed; the duty of the accoucheur in such cases—viz. to procure the expulsion of the fœtus by every means in his power, to exhibit the ergot of rye without delay, and to resort to the use of instruments, if they can be employed, when the ergot fails—being also strictly attended to. I have not met with a case, during these 12 years, which has terminated fatally to the mother. Several years ago I was called by the house-pupils of the hospital, to two cases of convulsions, occurring during early convalescence from puerperal fever. In both, the attack (which was identical with convulsions occurring during parturition) had been induced by enormous distention of the urinary bladder. I fortunately recognized this cause, instantly had the urine drawn off, and employed cold affusion to the head. The recovery was rapid in both.

In respect of the cold affusion on the head in cases of *hysteria*, the practice is unequivocally beneficial. In many cases of this affection, little more in the way of treatment is required. It should,

however, be kept in recollection, that hysteria may assume a severe character, and may even run into epilepsy, when either left to itself or improperly treated. And epilepsy may give rise to, as well as proceed from, permanent lesion of some part within the cranium. Now I know of few remedies which have a more marked influence in preventing these consequences than the judicious employment of cold affusion to the head; particularly when prescribed in addition to other rational and appropriate remedies.

In *cerebral croup*, an affection which occurs in children, and is characterized by spasm of the glottis threatening suffocation, with convulsions and spasm of the extremities, the cold affusion to the head ought never to be neglected. This affection has been much misunderstood, and has occasioned some discussion in recent times. It was first noticed, with any degree of precision, by Dr. Clarke, and has been denominated spasmodic croup by some, and *cerebral croup* by others. The best account of it which has been given may be found in the twenty-third volume of the London Medical Repository. It was furnished me, when editor of that work, by Mr. Cox, who was house-surgeon and apothecary to the Royal Infirmary for children for several years of the time that I have been physician to that institution, and is distinguished by those sound pathological and therapeutical views of the diseases of early life, which are to be expected from a well-educated and a most experienced and able practitioner. This affection is often connected with dentition, and occasionally with a disordered state of the prima via; but it is still more intimately dependent upon inflammatory irritation, or active congestion of the brain and its membranes, particularly about the base of the organ. The treatment which I have found most successful in the numerous cases which have occurred to me, particularly in public practice, may be succinctly stated as follows:—Leeching, or cupping, behind the ears, or about the base of the head and nape of the neck; the cold affusion to the head; purgatives, aperients, and alteratives; scarifications of the gums and the semicupium; with other subordinate measures—as blisters or irritants behind the ears, antispasmodics combined with soda, gentle anodynes, enemata, &c.

In all the cerebral affections of children, the cold affusion, in some one or other of its modifications, constitutes an important part of the means of cure. The remarks which have been made with reference to the cold affusion to the head in the above affection, are equally applicable to other forms of convulsion in childhood, excepting when the convulsion proceeds from inanition or exhaustion, when the affusion of warm or tepid water is sometimes beneficial.

In the *true inflammatory croup*—inflammation of the trachea, with albuminous exudation—the practitioner will often be foiled if he confide chiefly in depletion, or principally in calomel given often or largely. He will not always succeed even by means of a judicious combination of the above means; and the addition of the warm-bath, of emetics, and of various other measures which have been recommended, will not always be followed by relief. This disease, in its advanced states, may be complicated with congestion, or other disorder in the head; it may be characterized chiefly by spasm, either excited by the morbid secretion as it rises to the larynx, or proceeding from irritation or inflammation having extended to this part. It may also supervene to other diseases in the vicinity—as to inflammation in the fauces, to scarlatina with angina, to inflammation of the pharynx, and even to measles, &c.; and it may be, as it often is in London, complicated with bronchitis; this last affection being merely an extension of the inflammation down the trachea to the divisions of the bronchi. I make this brief and imperfect allusion to the complicated forms of croup, with the view of rendering the states of the disease in which cold affusion may be resorted to more intelligible to the reader.

After depletions have been carried as far as the discretion of the practitioner will suggest—after having endeavoured to arrest the morbid formative process, and to procure the expulsion of the membranous exudations and viscid secretion, by the frequent exhibition of calomel, camphor, soda, sulphate of potash, &c. with diluents, and the inhalation of emollient vapours; it will often be observed that very severe paroxysms, threatening the suffocation of the patient, will occur. In such cases, or rather states of the disease, emetics are one of our chief means of cure; but

they will not always succeed either in preventing or in relieving the paroxysms, particularly when the disease is associated, as it often is in such cases, with inflammatory irritation or congestion of the brain or its membranes, and which the depletions were insufficient to remove, owing to the influence of the paroxysms of cough and suffocation in preventing the return of blood from the encephalon and to the physical condition of this organ. Thus circumstanced, we are often at a loss for a remedy which presents any rational hopes of benefit; and it is precisely in this state of the disease that I have, on several occasions, derived remarkable advantage from the affusion of cold water on the head, and particularly on the occiput and neck. When these paroxysms are attended with much exhaustion, as they very frequently are, from the measures employed to remove the disease, the tepid affusion will be found preferable to the cold; and, in all cases, antispasmodic enemata should be brought to their aid, with anodynes, &c. given by the mouth.

There is a form of croup which is almost altogether *spasmodic*, (*spasmodic croup*) and which is different from the convulsive affection described above on the one hand, and from the inflammatory croup on the other. It generally comes on during the night; often during the patient's first sleep; is unattended with spasms of the extremities or general convulsions, unless sometimes towards the termination of fatal cases; is, upon the whole, not so acute or dangerous a disease as the affection already alluded to, and is much less so than the true inflammatory croup. This disease is the same with that described by the name of acute asthma, by Dr. Millar, and which has been a subject of great discussion and even dispute on the continent, where it has generally received the name of Millar's asthma. It has been confounded very generally in this and foreign countries, both with the spasmodic affection already noticed, and with the true inflammatory croup. It is, as I have already noticed, unattended with those marked symptoms and effects of inflammation accompanying the true croup, although frequently attended with, or followed by, an increased secretion of phlegm, and is much more under the control of medicine. The treatment which I have generally prescribed for it are the cold affusion to the head, local depletions, in some cases attended with vascular plethora, antispasmodics, and purgatives, given by the mouth, and administered as enemata. When it has been connected with dentition, free incisions of the gums have never been omitted, and the alvine secretions and excretions have received scrupulous attention.

I have entered into these distinctions because these three very different diseases are often mistaken for one another; and, consequently, very inappropriately, if not injuriously treated. I will briefly recapitulate the distinctions. *Spasmodic croup* was first noticed as a distinct disease by Dr. John Millar in 1769, under the denomination of the asthma of children, from its occurrence in paroxysms during the night, with intervals during the day, and the absence of inflammation. *Cerebral croup*, with spasms of the extremities and convulsions, was first noticed by Dr. John Clarke, about twenty years ago, and is characterized by the frequent and irregular recurrence of the attack, particularly through the day, by the marked signs of cerebral affection, and associated disorder of the digestive tube, by the spasms, &c. of the extremities, &c. *Inflammatory croup*, or inflammation of the trachea, with exudation of albuminous lymph, was imperfectly noticed by several writers, and first described with accuracy by Dr. Home. It is a much more acute and rapid disease, scarcely remitting, and never altogether intermitting, unless at the commencement of convalescence; is always attended with symptoms of local inflammation, and presents its results upon the inspection of fatal cases—a circumstance which does not obtain in respect of the other two diseases. Whilst cerebral croup has been generally confounded with spasmodic croup, the latter has been as often mistaken for the true inflammatory disease; and all of them have been too generally viewed as simple and primary affections, instead of being considered as associated or consecutive ailments in many cases, and traced to their morbid connexions.

When *hooping-cough* is unattended by inflammatory affection of the lungs and air-passages, and more especially if it be complicated with cerebral irritation or congestion, and threatens to superinduce hydrocephalus, the cold affusion, particularly on the occiput and upper

part of the neck, has proved in my practice most beneficial. Of course, it should be resorted to in addition to other measures, such as the application of leeches or cupping, semicupium, aperients, diaphoretics, &c. When the disease appears to be more purely spasmodic, the affusion has been equally serviceable. It should, however, then especially be employed frequently, but each application should be extremely short, and the patient's head afterwards ought to be wiped dry, a precaution which is quite unnecessary when inflammatory action, or active determination of the blood to the head, is present. Indeed, when such states of the cerebral circulation exist in a very marked degree, cold epithemata to the head, in the intervals between the affusions, are generally necessary. On the other hand, when stupor or coma is present, or when the disease is chiefly spasmodic in its nature, although the affusion may be employed for a longer or shorter time, according to the circumstances of the case, and at longer or shorter intervals, it will generally be found preferable to wipe the head dry after each affusion, as I believe that, in these cases especially, the succeeding effusion makes a more marked impression on the disease when this has been done.

In respect of the efficacy of the practice in cases of poisoning by opium, the profession has long been in possession of sufficient evidence, furnished first by Mr. Wray and myself, and subsequently by writers in various parts of Europe and America. Reasoning from the effects I have long since observed to follow its use as a popular remedy in cases of drunkenness, and from its influence in removing the effects of opium, I ventured to recommend the affusion of cold water on the head in cases of poisoning by prussic acid, belladonna, and other narcotic substances*. Two or three years subsequently to this recommendation, the same advice was offered by Dr. Herbst and others in Germany and France, but without any reference to the original source whence they derived their information. But it is not only in cases of poisoning by the narcotic drugs taken into the stomach that the cold affusion is serviceable, but also in cases of asphyxia from the vapours of charcoal, and from sulphuretted or carbono-sulphuretted hydrogen gas.

In cases where hurtful effects result from the incautious use of digitalis, belladonna, opium, spirituous liquors, &c. in cases of stupor, incipient coma, leipothymia, and lethargy, the cold affusion on the head is extremely useful, when employed in an appropriate manner.

Bulstrode-Street, Dec. 1831.

* See London Medical Repository, vol. xxiv. pp. 43 and 148.

ON THE COLDNESS OF THE TONGUE IN MALIGNANT CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

The professional world being cloyed and saturated almost *ad nauseam* with the subject of cholera, its causes, pathology, and treatment, it may almost appear to be a work of supererogation to attempt the relation of any symptom which has not yet been investigated in studying the character of this malignant disease. Whilst many individuals are endangering, by their contentions, the reputation of an art which has been always held in esteem and respect by the truly good and honourable portion of mankind, in all ages, I am particularly struck with the careless manner in which the dogmatists, who are now raving about the identity of the present epidemic with former visitations of severe spasmodic disease, noticed by Sydenham and others, have withheld their attention from one symptom of the present disease, which would seem to characterize it in a most remarkable manner; I allude to the peculiar coldness of the tongue. This coldness has never been witnessed by me in any epidemic or endemic disease of this country, nor have I been able to find any description of a similar symptom in any authority, ancient or modern, which I have consulted. During a recent visit of a few days to the metropolis, I had ample opportunities of ascertaining the general character of the present pestilence, and I must conscientiously say, that nothing conveyed a greater conviction to my mind of the present disease being essentially different from any which I had previously seen or read of, than the

cold, fleshy sensation, conveyed to the finger of the observer, when he is investigating a severe case of the present cholera in a state of collapse. Sir W. Russell, Sir D. Barry, and Dr. Lefevre, at St. Petersburg, and Dr. Hamett, at Dantzig, have respectively noticed this symptom of the cholera in those different localities. It formerly fell to my lot to witness some severer cases of the English cholera than I have seen lately: I resided at that period in an agricultural district of this county, where it was not uncommon for four or five strong, robust, harvest-men, working in the same field, to be seized with purging, vomiting, and severe cramps: these symptoms came on from the extreme heat of the sun, or from their drinking imprudently stagnant water from clay-pits, or sometimes from the cold, hard nature of the water, springing out of a chalky or gravelly stratum. This form of cholera, more severe than you will meet with in cities or large towns, easily yielded to diluents and opiates, as recommended by Sydenham. But that peculiar physiognomy or cast of countenance by which, as Mr. Orton says of the Indian cholera, "the present epidemic may be generally distinguished," I never found to exist in the above cases. After a careful perusal of the description of cholera given by Aretæus, Sydenham, and Frank, one might, *primâ facie*, believe from the non-bilious evacuations, violent spasms, and livid surface of the body, that we were studying a disease identical with that which is now committing its ravages among us. To me it appears evident, that we are now becoming acquainted with a disease which, though generally accompanied by the severe symptoms detailed by the above authors, has superadded to them other symptoms, so essentially distinct from any previously noticed in Europe, that if we will pertinaciously resist the notion of its being a *novus morbus*, we must come to the rational conclusion, that it is a disease, the essential symptoms of which have never yet been described in this country. Besides the complete silence of the above eminent practitioners with regard to the coldness of the tongue, in their description of cholera, I was rather surprised on consulting M. Landrè-Beauvais's *Semeiotique*, and Dr. Marshall Hall's *Semeiology*, to find that they had not noticed this remarkable condition of that organ. I am led to believe that the coldness of the tongue in the cold stage or collapse, is one of the most prominent auxiliary symptoms in establishing a correct diagnosis of the present malignant epidemic, and when combined with the *facies*, as well as the *vox cholerica*, we have presented to us a form of asphyxial disease, call it what you will, of which we have no record of its previous existence in this country; and to confound which with the congestive fever of Armstrong, the European endemic cholera, or the aggravated dysentery of Ireland, will remain a memorable instance of the prejudices of those who pretend to derive their opinions from the study of the book of nature. If any intelligent correspondent of your valuable journal will communicate his having noticed this peculiar coldness of the tongue in British endemic diseases, he will oblige

Your obedient servant,

Wm. England, M.D.

Physician to the Norwich Guardians' Dispensary.

April 7, 1832.

N.B. Since drawing up the above remarks, for the purpose of eliciting information, I have ascertained that Mr. Thackrah, of Leeds, mentions the existence of "a moist, cold, and furred condition of the tongue," in a case of severe endemic cholera: notwithstanding the identity of this symptom with the subject of this communication, I cannot avoid being sceptical as to the perfect identity of the cases detailed by that talented gentleman, with those of the present epidemic, the character of which can only be appreciated by an eye-witness, who will then bear testimony to the truth of Dr. John Gregory's axiom, that "whatever a man sees makes a stronger impression on his mind than what he learns by description."

THE SECOND SOUND OF THE HEART *NOT* VENTRICULAR.

To the Editor of the London Medical Gazette.

Glasgow College, March 15, 1832.

Sir,

The notice which I have just read of Dr. Hope's book in the Medical Ga-

zette, recals my attention to one of the subjects which that gentleman has discussed, and on which I request a very brief space in your next number to express my dissent, not only to Dr. Hope's, but to all the recent conclusions respecting the sounds of the heart.

Laennec supposes that it is while the ventricles are contracting that the first sound is heard; the second sound he apprehends to be that of auricular contraction; but the nature of either sound is not explained by this author.

Drs. Corrigan and Haycraft conceive the first sound to be caused by the sudden arrest given to the blood as it impinges against the fully dilated ventricle. They imagine the second sound to occur at the completion of the ventricular systole, when the motion which has been communicated to the blood is suddenly suspended, and to be generated by that arrest. These gentlemen, therefore, refer both sounds to the ventricle, only in opposite states.

But Dr. Bond is disposed to modify the interpretation just cited of the cause and seat of the second sound: he conceives it to originate in "the sudden arrest given to the further ingress of blood into the auricle by the complete occlusion of the auriculo-ventricular orifice, at the instant of the ventricular contraction.

And Dr. Hope differs, it appears, from the conclusions of all these gentlemen. He agrees with them, indeed, that the first contraction is that of the auricles, but then he thinks the auricles produce no sound during their action. The first sound, according to Dr. Hope, coincides with, and is produced by the ventricular systole. When the ventricles contract, an impulse is given to the particles of fluid in contact with them, and this being conveyed from particle to particle, generates the sound." The second sound, or "that of the ventricular diastole, is generated by the reaction of the ventricular walls on the particles of blood, when their course is abruptly arrested by the completion of the ventricular diastole."

My object is to vindicate the accuracy of Laennec in his description of the sounds of the heart, and to prove, in as few words as possible, the fallacy of the views adopted by Drs. Corrigan, Haycraft, Bond, and Hope, who would refer the second sound either to the ventricular systole, the ventricular diastole, or the diastole of the auricles.

In order to do this, I must be permitted to refer the reader to a case under my father's care last year in the Glasgow Royal Infirmary. When published in your Gazette, June 11, 1831, the patient still lived, but the fatal termination which took place some months afterwards, fully verified (as I am able to say on the authority of a friend who witnessed the inspection) the diagnosis I had given. Now I beg particularly to recal the reader's attention to this remarkable case, (for though Laennec had, it appears, seen examples of similar anomaly, I am not aware of any such having been noticed in this country) in which every peculiarity on which I would dwell, as tending to illustrate this much controverted point as to the order of succession of the cardiac sounds, was familiar to a great number of attentive observers. The first sound, with its concomitant impulse, having been as usual followed by a second, this second sound was instantly reduplicated. In describing this case, I thought it well to express the time occupied by the sounds by musical notation, which I observe has afforded material for a rather indifferent pleasantry, of which Dr. Johnson is welcome to all the merit, and the magazines to all the publicity. On some occasions we observed the reduplicated second sound to be again repeated, occasionally even to the fourth time, before the first sound was again heard. Now the first sound, being synchronous with the pulse at the wrist, could be no other than that of the ventricle, and the radial pulse, as I have elsewhere stated, being remarkably slow—often only 45, the observation was made with the greatest facility, and without any liability to confusion.

As there can be no reason for supposing the order of the heart's sounds to be ever inverted in cardiac disease, and as the sounds here were perfectly natural, each after its kind, that is to say, a clear sound coming after an obtuse one, or rather several clear sounds after an obtuse one, I proceed to inquire how this statement of facts agrees with the respective theories of Dr. Haycraft, Dr. Corrigan, Dr. Bond, and Dr. Hope.

If the second, or clear sound, in this case should be attributed to the "com-

pletion" of the ventricular systole, with Drs. Corrigan and Haycraft, it is quite sure that it could not have been reduplicated, far less repeated three, or even four times, since every complete contraction implies a preceding dilatation, and therefore the second sound could not be repeated till the previous recurrence of the first. A similar objection applies to Dr. Bond's theory of the second sound arising from the "sudden arrest given to the farther ingress of the blood into the auricle by the complete exclusion of the auriculo-ventricular orifice at the moment of the ventricular systole." In such a supposed state of parts there can still be no reduplication of sound. Neither will Dr. Hope's attempted solution obviate this difficulty; for the "completion of the ventricular diastole," like the completion of the ventricular systole, can only take place *alternis vicibis;* in short, after a full diastole, it is obvious that a full systole must succeed before the ventricular cavity can again expand to its greatest area. What then remains but to consider the second sound as concomitant with auricular contraction, since it is found to disagree equally with any of the suppositions which refer it to the perfectly dilated, or to the fully contracted ventricle, or to the diastole of the auricle?

Other considerations lead also to the same conclusion as to the second sound being of, and belonging to, the auricles. 1. It is loudest over their situation; 2. The renewal of an uncompleted effort in disease, as in this case, is more reasonably to be referred to a part of which the natural action is always less perfect than that of the ventricle. The contraction of the auricle never completely emulges the cavity. In the case to which reference has been made, the ventricular cavities were found dilated; but as they are filled from the auricles, the relative capacities of which were here so much smaller than those of the ventricles, it was even probable that to receive their full charge the auricular systole would require to be repeated. 3. The clearer sound should proceed from the thinner medium—the auricle is the thinner. But, 4. What renders the second sound beyond dispute auricular, and not ventricular in this case, and so in all cases, is that there came but one pulse at the wrist, and but one impulse at the chest, however often the second sound was repeated; and this impulse at the chest, and pulse at the wrist, attended the first sound. There were forty-five pulsations of one of the cavities of the heart, corresponding to as many pulsations at the wrist: that cavity of which the pulsations produce the pulse at the wrist, is the ventricle. The forty-five sounds, therefore, which were heard, were the sounds elicited by the contraction of the ventricle; and the second sound, which in this case was repeated, could be no other than that arising from auricular contractions.

I conclude, therefore, that Laennec is right when he assigns the second sound to the contraction of the auricles, and the first sound to that of the ventricles; and I conceive that neither Dr. Hope, Dr. Corrigan, Dr. Haycraft, or Dr. Bond, have at all succeeded in explaining the cause of either of the sounds.—I remain, sir,

Your obedient servant,
DAVID BADHAM, M.B.

WOUND OF THE CAROTID ARTERY, FOLLOWED BY ANEURISM, AND CURED BY LIGATURE.

To the Editor of the London Medical Gazette.

SIR,

SHOULD you think the following case of carotid aneurism, successfully treated by operation, of sufficient interest to find a place in your useful periodical, it is very much at your service.

Your obedient humble servant,
EDW. FRAS. DEHANE, M.R.C.S.

Wolverhampton, March 10, 1832.

Louisa Newell, a delicate girl, ten years of age, on the 4th January last, in the act of going down stairs with a dish in her hand, slipped down, and in her fall broke the dish, a sharp point of which punctured the neck. On my arrival, a few minutes after the accident, I found the little girl bleeding profusely from a wound about an inch above the clavicle, in the course of the carotid artery on the right side. About two pounds of blood had been already lost, and she appeared rapidly falling into a state of syncope; her lips being palid, her skin cold and clammy, and the pulse at the wrist not to be felt. The wound in the neck might be large enough to admit the end of the finger.

Being doubtful whether the patient would revive, I immediately applied a compress of

lint over the wound, and caused it to be retained there by pressure, and in the meantime prepared ligatures, &c. to secure the bleeding vessel; but was surprised, upon its removal, to find that there was no hæmorrhage, notwithstanding she had very much revived from the state of syncope she had fallen into. I, however, waited some time, under the expectation of its renewal, but which did not follow, owing, as I suppose, to the obliquity of the puncture. I therefore secured the compress, and, having placed my patient in bed, left her. It was not until the fifth day afterwards that I removed the compress, when I found that the external wound had healed, but perceived a small pulsating tumor a little above the puncture. I desired the child to be kept quiet, and continued the pressure both upon and below the tumor, as tight as it could be borne; it, notwithstanding, gradually increased, but not so much as to be very perceptible till the night of the 19th, when it suddenly became enlarged to the size of a walnut, pulsating under the sterno cleido mastoideus muscle, and extending beyond it, and evidently in the line of the carotid, upon pressing which all pulsation in the tumor ceased. At my visit on the following morning, I proposed to take up the vessel, which was immediately assented to by the parents of my little patient.

Having requested the attendance of my brother, Dr. Dehane, and Mr. Thompson, surgeon of this town, they agreed with me in opinion that no further time should be lost in performing the operation.

The patient being placed on her back, with her head inclining over the left shoulder, I commenced an incision from the base of the tumor, following the course of the sterno mastoideus muscle along its inner edge down to the clavicle. More difficulty was experienced in getting at the vessel than I had anticipated, the space between the base of the aneurism and bone not exceeding one inch, which small space was crossed by the thyroid veins, and moreover the depth of the cellular membrane was considerable, so that I could barely feel the vessel with the point of my finger by forcibly pushing back the sterno mastoideus muscle. I therefore found it necessary to extend the incision in a direction upwards and backwards, somewhat above the tumor; by doing which, I was at length enabled to get a sufficient space to pass my ligature round the vessel. This, however, I was unable to effect with the common aneurismal needle, but succeeded with an eyed probe, which I bent to an acute angle and passed under the artery, armed with a single silk ligature; this being drawn tight, I had the satisfaction to find that all pulsation stopped in the tumor. The operation was thus concluded without any further loss of blood than the trifling quantity which followed the first incision; I placed my patient in bed, and, having exhibited an anodyne, left her. At my visit in the evening, I found her tranquil and free from pain; the right side of the face, which had become cold on tying the ligature, had resumed the natural temperature, and the pulse at the wrist was regular.

January 21st, 9 A.M.—Patient has slept well, but complains of a little thirst; pulse 125; skin rather warmer than natural; no pulsation in the tumor. At 10 P.M. has had more refreshing sleep; tongue slightly furred. A saline aperient given, but has not acted; to be continued 3tiis horis.

22d.—Slept well; skin more cool; pulse 120; bowels have not yet acted; wound looking well.

To take Ol. Ricini, ℥ss. 2dis horis.

23d.—Bowels have acted—has had two motions; skin cool; pulse 120; tongue less furred; has passed a good night.

24th.—Slept well; pulse 110; wound uniting at upper part; slight watery discharge from the inferior part.

Cont. Mist. Salin. Aper.

25th.—Going on well. From this time to the 31st, when the ligature came away, no bad symptom had occurred. Pulse is now 90; discharge more healthy, and much less in quantity; wound nearly closed, and the aneurism diminished to about the size of a Spanish nut. About a fortnight after the last report the wound had quite healed, and the aneurismal tumor scarcely perceptible.

PUERPERAL CONVULSIONS.—VENESECTION—INSTRUMENTAL DELIVERY—RECOVERY.

To the Editor of the London Medical Gazette.

SIR,

IF you think the following case worthy a place in your excellent periodical, I shall feel much obliged by your inserting it.

I am, sir,
Your obedient servant,
J. HAMERTON.

Halifax, Yorkshire,
April 3, 1832.

A. T. wife of a bargeman of this place, ætat. 22, of short stature, muscular make, and florid complexion, was seized with labour of her first child about 3 A.M. of the morning of the 14th last December. When I was summoned to her I found the pains rather frequent, with the os uteri dilated to about the size of a sixpence, and I left her,

desiring to be called in a few hours if the pains should increase.

Hearing nothing of my patient till nine o'clock A.M., I called upon her, and found the os uteri progressively dilating, with a slight increase of pain; and having other urgent cases to attend to, I did not again see her before 12 A.M., when the os uteri was dilated to nearly its fullest extent, with the pains forcing down and the head descending into the lower portion of the pelvis.

Under these circumstances I thought it right to remain with my patient, satisfied that if the pains increased, three or four hours must terminate the labour. From the above period till about 4 P.M. the pains continued to increase in frequency and force, when suddenly one of the assistants, in a tone of alarm, exclaimed, "Oh! what is to be done with your patient?—she is about to expire!" and, in fact, she became instantly the subject of one of the most violent convulsions I ever witnessed. I immediately had recourse to venesection, and abstracted about twenty-eight ounces of blood, which appeared for a moment to give relief; temporary, however, for a second convulsion speedily supervened, when the blood was again allowed to flow to nearly the same amount. A general relaxation of the whole system ensued, along with the restlessness so frequently attendant on severe loss of blood. On directing my attention to the mechanical cause, I found the perinæum relaxed as thin as parchment, and the head of the child partially resting upon it. I resolved to apply Heighton's short forceps, which was effected with difficulty, from the great jactitation under which my patient laboured; however, this was accomplished, and the birth of the child expedited. From this period every unfavourable symptom gradually subsided, and my patient, with her offspring, eventually did well, without the intervention of any untoward symptom save a transient darting pain through the head, with some degree of stupor, which were relieved by leeching, cold stupes, and smart cathartics. I have sent this case for insertion in your columns, not because it involves any novelty either in its nature or mode of treatment, but because it appeared to me a matter of some practical importance that the younger branches of our obstetric practitioners may be reminded of the powerful effect of large abstractions of blood, in cases of puerperal convulsions, as a relaxant, not only to the general system, but to those parts more especially connected with the parturient state.

SMALL-POX AFTER VACCINATION.

To the Editor of the London Medical Gazette.

SIR,

IN order that this much-disputed point may receive all the elucidation its importance demands, I think it the duty of every medical man to give publicity to any cases which may fall under under his notice.

No one sets a higher value on vaccination than myself; and even were I certain that cow-pox would be invariably succeeded by small-pox, still I should consider the Jennerian discovery one of the greatest blessings vouchsafed to mankind.

In your number of the 10th inst. there is related minutely, by Dr. Rankine, a case of this description, (small-pox after cow-pox) with some remarks on a letter of Dr. Sander's to Dr. Howison; and I am sorry to have it in my power to add another of the same character, which has just now occurred in my own practice.

Harriet Gandy, æt. 17, living with her parents in Park Road, Liverpool, was vaccinated by a gentleman of this town when a year old, and her mother tells me she had a very fine pustule on each arm: the cicatrices are at this time both perfectly distinct. She has been in constant attendance on her brother, a boy, twelve years of age, who was seized on the 22d of last month with the usual precursory symptoms of casual small-pox, (never having been vaccinated) which terminated in a very extensive eruption, and of which he died on the 27th.

Eight days after his death (March 6th), she (Harriet) sickened, and had a moderate, though generally diffused crop of pustules, accompanied with considerable fever and great pain in deglutition, but is now convalescent. At the same time her sister, five years old, who slept with Harriet, was attacked, and is now labouring under the disease in a very severe form, being, as was the case with her deceased brother, literally covered from head to foot. She also had not been vaccinated, and of her recovery I am very doubtful.

I have not thought it necessary to be more particular in the account of this young woman, as no impartial person, I should think, would doubt of its being genuine variola, corroborated as it is by other members of the family being simultaneously affected. I certainly agree with Dr. Rankine in considering "modified small-pox as the genuine small-pox in a mild or softened form."—I am, sir,

Your obedient servant,

FRANCIS BROWN LLOYD.

Liverpool, 16th March, 1832.

MEDICAL GAZETTE.

Saturday, April 21, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

FRENCH PHILOSOPHY OF MEDICINE.

THE Parisians seem to be catching at any straw by which they may hope for a little safety. They now blame the wind (the newest theory!) which they find has been blowing east and north-east for several days, for the terrible mortality by which they have been afflicted. The wind has changed a point or two—the number of deaths has diminished from the average of above three hundred a-day to some units less—and they are confirmed in their present hypothesis. Vain reasoning, and wilful shutting of the eyes to all that the book of nature has for years past been teaching them! What is there, we would most gladly know, peculiar in the blowing of the said wind of late from this particular point? Has it never blown from the same quarter before? Is there, either, any thing so different in the very recent habits and modes of life of the people of Paris, or so unfavourable in the structure of their buildings and the medical police of their metropolis, that, in conjunction with the blowing of the said north-easter, the cholera should all of a sudden, and quite newly, be generated amongst them? But any thing for novelty, and to avoid the plain, homely, straight-forward fact! Any thing for the sake of supporting the over-head-and-ears opinion so preposterously adopted in the outset! It *could not* be contagion, for it spread too rapidly (and in Edinburgh, be it remembered, it could not be contagion either, for there it spread *too slowly*); *no* chain of connexion could be discovered, because it was not discoverable in a moment—in the midst of alarm, and the most idiotic mixture of rashness and pusillanimity.

We last week took occasion to point out what appeared to us to account for much of the rapid spread of cholera in Paris. How the disease reached that capital, we did not pretend to explain, nor did we think it very necessary to inquire; the circumstances, however, were manifestly far less obscure than those which attended its arrival in many other capitals of Europe; but once there, and the extraordinary arrangements made for its reception considered, what is there marvellous in the catastrophe? The hospitals without reserve were thrown open to it (no doubt with a view to improving upon the London plan), but, probably calculating upon the not very formidable number of cases here, the accommodation afforded was dangerously circumscribed: a ward for males, and a ward for females, in each hospital; while all the medical men and their attendants in each establishment were obliged to give their services in those same crowded apartments. Thus, though the disease was nominally kept apart, there was the freest communication between all the saloons of each establishment; nor was there the slightest restriction imposed upon the intermixture of the medical people with society. Well, the cholera spread *surprisingly*, and so generally that it was *impossible* contagion could account for it. In four days (or, to speak more correctly, *four and a half*) the faculty of the Hôtel Dieu decided that the disease was a true epidemic, and not propagated by contagion! And, by the way, what a delightful subject for a sneer against English practitioners did this decision afford to some of our sagacious journalists! This unanimity, said they, if it had occurred *here*, would have been next to miraculous; and forthwith they raise a hymn of laudation in honour of the "glorious" French. Fools! not to see that per-

haps to this very variance of opinion and cautious prudence of English practitioners, they—even they, those sagacious philosophists—owe most of their safety and exemption from the disease. But what has come of this famed unanimity of our continental friends? What are its fruits? What are its palpable consequences? The history of the cholera in Paris will, indeed, be memorable, and assuredly it will display those traits of all kinds which are announced as the chief topics of a forthcoming national work on the subject: *traits de mœurs, traits de courage, traits de cruauté, d'insouciance, d'ignorance, de faiblesse, d'ingratitude—tout s'y reproduira* (continues the historian), *et formera un recueil curieux de l'état de cette civilisation qu'on avait tant vanté, et qui a eu, il le faut avouer, ces jours ci des cotés bien miserables**. It would be easy to add a few more *traits* to the list, but we trust the writer is a medical man, and that he will do justice, in the proper place, to his lively professional brethren.

The heir-apparent of the Crown of France walks the wards, with a train of courtiers at his heels, and M. Casimer Perier among the number. Well, what purpose does this serve? "To present more victims to the cholera," say we, "and to spread it more extensively among the highest circles in Paris."—"Nay, rather," cry our French friends, "to inspire confidence," (fatal confidence!) "and to prove how little danger is to be apprehended from the contagiousness of the malady." What! are we come back to the old story of Desgenettes again? How often will the French have to be reminded of the delusiveness of these bits of romance?—these experiments with two handles, equally commodious for the non-contagionists and their adversaries. Suppose the Prince became *cholerique*, as M. Perier did,—*tant pis*, say the former; *tant mieux*, their opponents; and while both might lament the national mishap, each would, no doubt, account for the circumstance satisfactorily upon their own special theory of causation. But it is not by isolated experiments of this kind that the question of contagion or non-contagion must stand or fall. Small-pox, measles, scarlet-fever, and plague, would be accounted mere epidemics if particular cases of insusceptibility after contact were to be allowed to warrant general conclusions of immunity.

We will not be so ungracious as to impute to the better-informed among our French brethren the false logic which such conclusions would imply; but we cannot help thinking that they have suffered themselves to be overhastily bewildered by some gross fallacy or other. In India we know that similar conclusions were similarly arrived at by numerous practitioners from Europe. Surrounded by the disease, in the thick and heat of the mortality, like rank and file in a battle, they lost all clue to the reasons for what was going on about them: *why* these were cut down, and those escaped, it was impossible for them, by reason of their too great nearness, to discover; but there were those at a convenient distance whose vision was unobscured, and whose faculties were unobstructed by the turmoil and smoke and bustle of more immediate action—and those luckily were the officers whose duty it was to write the despatches. In India, to speak less figuratively, it is well known that the general conclusions of the Boards were the very reverse of the *ex-parte* inferences drawn by most of the individual practitioners in their employ: and fortunately has it been so,—we have no hesitation in tracing to this source many of the soundest prophylactic principles by which the security and safety of Britain have been strengthened.

* Messager des Chambres.

And so we should hope it will be with the more enlightened portion of the French faculty ere long. When the panic has ceased, and the confusion is allayed, sober reason, we doubt not, will resume her seat, and the hasty inductions of the precipitate will be overhaled and examined by the scrutiny of a cooler judgment. Of the final result, we entertain no fears.

SUCCESSFUL TREATMENT OF CHOLERA.

We beg to refer our readers to two very remarkable documents on the subject of cholera—one in our last number, from the pen of Mr. J. W. Earle, the other subjoined. In the former the writer states (nor do the facts rest on his testimony alone), that small doses of calomel and opium, accompanied by the application of very large sinapisms, and followed by *purgatives*, succeeded in about sixty cases in which these remedies were adopted at an early period. The latter paper speaks for itself, and represents forty-four out of forty-six cases, as cured by the exhibition of certain salts. Granting, as was probably the fact, that the patients thus treated were chiefly in the early stage of the disease, enough remains, if the results be verified by future observers, to make the subject one of vital importance.

NON-PURGATIVE SALTS IN CHOLERA.

[The source from which we have received the following communication leads us to place implicit reliance on the fidelity of the statements.—E. G.]

The following circumstances relative to the treatment of Cholera in the Prison at Cold Bath Fields, are of great importance:—

The first twelve cases occurred in the Vagrants' Ward, and the patients were attacked soon after some prisoners had been admitted from St. Giles's, and other infected districts. The first case that was reported as cholera occurred on the 5th of April. This man was suddenly attacked, and died after a very short illness with all the symptoms of the prevailing epidemic.

When the first cases occurred there were in all about twelve hundred persons in the Prison; but, up to the beginning of this month, they were not afflicted with bowel complaints, nor, in fact, with any other epidemic disease, being as healthy as they generally are at that season of the year.

The first four cases were treated in the common way, with brandy and opium, an ammoniated mixture, ginger, sinapisms to the region of the stomach, the hot air bath, &c. &c., and all of them died after a short illness.

Since the 4th of April, up to this date, (April 17,) forty cases in all have been under treatment. Of this number, nineteen were admitted into the Observation ward with the premonitory symptoms of cholera. All of these had bowel complaints and suspicious ejections; some of them complained of severe pain in the abdomen, sickness of the stomach, and in several cases these symptoms were attended with cramps, chiefly in the lower extremities. The whole of them were immediately treated by Mr. Wakefield with non-purgative saline remedies, recommended by Dr. Stevens, and in general they were convalescent in one, two, or three days, from the commencement of this practice. From this we may infer that where the disease is attended to early, and *properly treated*, the state of collapse may be prevented in nineteen cases out of twenty.

We must state, however, that as the numbers increased it became necessary to dismiss those that appeared to be least ill, on purpose to make room for others. Of those that were dimissed as convalescent, two were re-admitted soon after in a state of collapse, and though every attempt was made to save them, yet they both died after a very short illness, with the symptoms of cholera in its most virulent form. With the exception, however, of the two that died, none of the cases, (seventeen in number) were reported to the Central Board, partly, we believe, from a wish to avoid spreading alarm with respect to the prison, and partly because the disease was checked in the beginning; consequently, the patients had not *all* the symptoms of cholera, such as occur in the worst cases, or in the last stage.

In addition to the above seventeen which were not reported, there were twenty-one cases where the symptoms of cholera were very distinctly marked. Of this number, four of the early cases were treated in the common way, with diffusible stimuli, &c. &c. and all of them died after a short illness. These, with the two cases of relapse from the Observation ward, make in all six deaths. Mr. Wakefield, however, having lost all faith in the common treatment, changed the practice:—at the request of Dr. Stevens, the other fifteen cases were put under the saline treatment, and all of them recovered.

When the patients were first admitted, the following powder was immediately given, either in half a tumbler of tepid water, or occasionally in a little thin, clear, beef-tea.

Supercarbonate of Soda, ʒss. Muriate of Soda, ℈j. Chlorate of Potass, grs. vii.

The above was given every hour, and continued until the patients were recovering from the state of collapse; after which it was diminished in frequency, in proportion as the re-action increased.

In all these cases, the outline of the practice was nearly the same; but in several instances the treatment was varied according to circumstances. When the stomach, for example, was extremely irritable, it was found that the carbonate of soda, given by itself, or the tartrate of soda, in a state of effervescence, were the most effective remedies that could be used on purpose to allay the irritation, so as to enable the stomach to retain the stronger salts.

During the progress of the disease, an enema, with a large table-spoonful of muriate of soda, dissolved in warm water, was administered with or without sugar, starch, &c. every three or four hours, at as high a temperature as the patients could well bear it. Sinapisms were also applied as early as possible to the region of the stomach, betwixt the shoulders, &c.; and in the cold stage, frictions were also frequently used with warm towels. Of the seventeen cases that were treated in this way two died, (namely, the two patients who were re-admitted in a state of complete collapse), making in seventeen cases, two deaths, and fifteen recoveries. But including the whole of those that were under the saline treatment, the total amount is, in thirty-six cases, two deaths, and thirty-four recoveries.

The cases in question were under the care of Mr. Wakefield, the medical attendant of the establishment, and during his absence they were attended to by Mr. J. Wm. Crooke, who kept notes of the cases, and saw that the medicines were properly administered. We may add, also, that Mr. Wakefield, with a degree of fairness which does him great credit, invited Dr. Stevens to attend along with him to witness the effect of the saline treatment, which has here, we may say, for the first time been fairly tried in this disease.

We can also state, that the cholera made its appearance about the same period amongst a small colony of Italians, who live in a narrow lane within a few hundred yards of the Prison. Of these, eleven were attacked. The three first cases were treated by bleeding, brandy, and opium, all used at the same time, and they all died. The other eight cases were attended by Mr. Whitmore, a surgeon in the neighbourhood, who, having witnessed the effects of the saline treatment in the Prison, adopted it. All his patients speedily and completely recovered, except one, who, on the 13th, was so ill that he was not expected to live many hours; even he, however, is now in a state of convalescence. *Thus there have been in all fifty-three cases, seven of which were treated in the common way, with diffusible stimuli; and out of this number seven died; while, of the forty-six that were under the saline treatment, there were two deaths and forty-four recoveries.*

PROGRESS OF THE ANATOMY BILL.

This luckless bill, our readers may recollect, was introduced to the House of Commons in December last; and it is now, if we remember aright, much above two months since it was first committed; but delay after delay has occurred, and not until Wednesday week was there any effectual step taken towards advancing it a single stage. The discussion which took place in the Committee on that night, however, we look upon as sufficiently important to warrant our giving a report of the proceedings, condensed from an authentic source*; and the rather, as they have been almost wholly overlooked by the ordinary journals. Next week we pro-

* The Mirror of Parliament.

pose to offer some remarks on the amendments adopted.

The Speaker having left the Chair,

Mr. Warburton proposed, as the first amendment, that Ireland should be included in the Bill.

Mr. Ruthven thought that the measure would be highly injurious to Ireland, destroying, as it must, the moral feelings of the lower classes in that country. Let it first be tried in England. No doubt the surgeons of Dublin would be very glad to participate in the Bill, as it would enable them to procure bodies at a cheaper rate than at present.

Mr. Warburton believed, on the contrary, that the gentlemen in question would rather abide by their present mode of obtaining subjects; and he had heard that it was the intention of the Irish College, on second thoughts, to petition *against* the Bill.

Mr. Spring Rice was convinced that this Bill would put an end to the horrid crime of "burking;" and that, if not now, it would ultimately be necessary to extend the measure to Ireland. He would support the amendment with pleasure.

Mr. Crampton knew that a practice had recently grown up, of exporting dead bodies from Ireland; and he was persuaded, that if this bill were to be confined to Great Britain only, that the system of exportation to both Scotland and England would greatly increase.

Mr. Shiel also thought that the extension of the Bill was the only remedy for the exportation system.

Sir Charles Wetherell made several objections to the bill generally, at the same time that he admitted he was not prepared to propose any thing better. The phrase, "in lawful custody," he considered as dangerously indefinite.

Mr. Warburton deprecated the discussion of the general question, which had already been long and often enough entertained by the House. He would only beg to add, that the object of the Bill was not only to promote science, but to afford facilities of diffusing it, and of extending the benefits of it to as large a circle as possible, so that the poor man, as well as the rich, should enjoy the benefits of it; and further, besides making science cheap, it would have the indisputable effect of putting a stop to the commission of crime.

For the amendment, 43; against it, 3; majority, 40.

Sir Robert Inglis moved the insertion of a proviso, requiring the application to the Secretary of State for permission to open a school of anatomy, to be countersigned by *two magistrates* of the place where the applicant should reside, and requiring that the applicant be a fellow, or licentiate, of some College of Physicians or Surgeons, or a medical graduate, or in some way lawfully qualified to practise medicine in any part of the United Kingdom. This, he certainly was aware, would limit very much the numbers qualified to open schools of anatomy; but he thought greater evils likely to arise to humanity, and to society, from permitting a human body to be brought into the private house of a private individual, than could possibly arise from the proposed restriction of the numbers able to hold licenses.

Mr. Warburton objected to this amendment, as its effect would be to restrict dissection to schools only, and to prevent all private dissection. At all events, if licenses were to be made necessary at all, they should be extended to professors, teachers (many excellent ones among whom were not licentiates), and to students attending any school of anatomy.

The amendment, with this alteration, was carried, after some desultory debating, by a majority of 44.

Mr. Wason moved the addition of this proviso:—"Provided always, that no dead body shall be taken to any private dwelling-house, for the purpose of anatomical dissection."

Mr. Warburton thought that this would amount to a complete prohibition of a country practitioner from reviving his acquaintance with anatomy, or dissecting at all.

Mr. Hunt, Colonel Sibthorp, and other honourable members, delivered their sentiments, but wandered widely from the question. At length,

Mr. Shiel rose, and observed, that by the provisions already adopted, no surgeon could receive a dead body unless licensed by two magistrates under the authority of the Secretary of State; then notice was to be given of the place where anatomy was to be carried on; and, finally, there should be a certificate and a return on the receiving of the body. Here were three distinct checks. Could more be suggested? The amendment, in his opinion, might very well be dispensed with.

The amendment was put, and negatived.

Sir Robert Inglis now proposed that the license should be yearly; that twenty pounds should be paid for it the first year, and five pounds for its renewal every year after; and that for every pupil entered in the school, one pound annually should be paid to the Secretary of State. This was to insure the respectability of the parties, and to relieve the public from the expenses entailed by the Bill.

Mr. Warburton gave his decided opposition to this proposal.

Mr. Hunt thought the sums "ridiculously small." It was his intention hereafter to move that every anatomical teacher in London, Edinburgh, and Dublin, should pay one hundred pounds license, and in the country parts fifty pounds. It was not too much.

The amendment was negatived without a division.

Mr. Hunt proposed to multiply the number of inspectors, and to put them on the footing of coroners of counties; but the original clause was ultimately allowed to stand. Another amendment, however, of the honourable member for Preston was adopted, —namely, that the returns of the inspectors shall be monthly, and shall specify the age, name, and sex of every subject dissected at the hospitals and schools under their charge.

Mr. Weyland proposed, as an amendment to the next clause, that the inspector should have the power of visiting *any private house* where he *heard* that anatomy was pursued.—Negatived.

The next clause was then read, as follows:—"That it shall be lawful for his Majesty to grant to any such inspector such an annual salary, not exceeding one hundred pounds, for his trouble, and to allow such a sum of money for the expenses of his office as may appear reasonable; such salaries and allowances to be charged to the consolidated fund of the United Kingdom, and to be payable quarterly; and that an annual return of all such salaries and allowances shall be made to parliament."

Sir Robert Inglis said, that he would take this opportunity of repeating some of his principal objections to the clauses of the present Bill; indeed the initiatory clause was the only one of which he could decidedly approve. He objected most strongly to that which authorizes, in the most general terms, any person having lawfully the custody of a dead body to dispose of it for the purpose of anatomical examination; for suppose any person should die at an inn, the inn-keeper would be enabled to dispose of the dead body—*he* has the "lawful custody" of the body, and there is nothing in this law to prevent him from doing so. From the operation and provisions of this Bill, he would exclude—1st. Prisons; because he would dissociate the idea of disgrace from dissection, and because he thought it important that places like prisons should be free from all suspicion connected with the death of parties dying therein. 2dly. Hospitals and Infirmaries; because those whose duty it was to cure the patient, should not be supposed to have an interest in killing him. As a legitimate source of supply, he would propose that persons dying in workhouses in London, should be made available: this would be a supply that science could scarcely exhaust; and a selection of this source would be attended with one great advantage. It should be a principal object to increase the reluctance to go into the workhouse, and lessen that of resorting to the hospitals. He understood, that, in the course of the last year, 603 persons were buried at the expense of one parish, and 599 in another—that is, 1202 bodies were buried at the public expense in two parishes alone. On no account whatever should money be taken for a dead body. At a future stage of the Bill he would propose, that if any person, not duly licensed, should dissect—or any person licensed dissect in an improper place—he should pay a fine of five hundred pounds. This, he thought, would be sufficient to attain every end in view. There was one other clause which he would propose—namely, that in case a body is found, and not claimed within three days, it may be given up by the parish officers for dissection.

Mr. Warburton replied, that if the main proposal of the honourable member for Oxford were adopted—namely, the dissection of the poor who died in workhouses—it might then, indeed, be said that a line had been drawn in favour of the rich. As to hospitals, all who resort to them are aware, that, in case they die there, they may be anatomically examined; and if this practice were abandoned, we should lose some of the richest opportunities of improving medicine and surgery. Nor could he see any reason for exempting gaols. To the appropriation of bodies found, he had no objection. But with regard to the proposal that no money should be given, or taken, for a dead body, in any case whatever, he would ask, how could it be prevented? Could we prevent presents being made? *He* would not be guilty of the *hypocrisy* of introducing a clause which he knew would be broken through on every occasion. No human power could enforce such a regulation.

Mr. Fane thought it possible to stop the sale of bodies by fine and imprisonment. The clause in question would destroy the character and moral feeling of the lower classes.

Mr. Briscoe and Mr. Geo. Dawson expressed strong impatience that the further discussion of the bill should be postponed. They had now been above seven hours occupied with the various clauses of the bill; and this one was too important to be disposed of at so late a time of night.

Sir Robert Peel considered that the whole principle of the bill was involved in the present clause, the question being, whether there should or should not be a legislative permission for the practice of dissection in this country. All were acquainted with the crimes and dreadful consequences which had ensued from refusing a legal sanction to the study of anatomy. It was to be feared that the same dreadful system still existed, and would continue to exist, unless something of the nature of this Bill were granted by legislative authority. Exhumation was even a light evil compared with the horrible practices that had been carried on in this country and in Scotland. Long before these circumstances

were brought to light, or the slightest suspicion existed of such acts being perpetrated, he (Sir R. P.) recollected having a conversation with two eminent surgeons, who told him that although there was no proof of murder having been committed, yet the price of subjects was so high, from twelve to sixteen guineas each, that they never paid for them without a strong feeling of remorse, considering that the payment of such sums held out a positive inducement to the crime of murder. The result proved the correctness of their apprehensions—a result *occasioned by the refusal of Parliament to legalize the sale of bodies after death.* It was impossible to conceive a greater evil than the present law. The honourable member for Oxford had submitted a proposition, but it was one to which a very formidable objection might be made: why, it would immediately be asked, should dissection be confined to the poor, and not extended to the rich? If, then, it were impolitic and unjust to confine the operation of the Bill to one class of society only, would it not seem clear that there was no alternative but to apply it to all? and this object was, he thought, fully practicable by the clause now submitted to the committee. He could see no other rational course than to adopt it; and would therefore give his fullest assent to the passing of the present Bill, on the principle laid down in the clause before them; and he hoped that its adoption would not be longer postponed, as in the present state of public excitement such procrastination would be tantamount to the rejection of the measure.

Mr. Hunt was astonished at the speech of the Right Honourable Baronet. For his part, he was sure that the effect of this clause would be to brutalize the human race and human nature. (Laughter.)

Mr. Briscoe persisted in pressing a motion of adjournment.

The Chancellor of the Exchequer spoke at considerable length in favour of the clause, and encouraged Mr. Warburton to proceed.

The Attorney-General followed in eloquent terms on the same side; but Mr. Dawson and Mr. Briscoe pressed for a division, in which, however, they were overruled by a majority of 48 against the proposed adjournment.

Mr. Warburton, after complimenting the Right Honourable Member for Tamworth (Sir R. Peel) for his speech, in which he had put the subject in the proper point of view, said he would leave it to the gentleman connected with office whether he should now proceed further with the clause in question.

Lord Ebrington thought that though there might be reasons for postponing the consideration of this particular clause, yet there could be no reason for not proceeding with the other parts of the Bill.

Mr. Warburton, however, thanked his honourable friends, and would not then press their attendance further.

The Committee reported progress: to sit again on the following night.

The account of the rest of the proceedings of the Committee is briefly told. A week elapsed before the further consideration of the Bill was resumed. On Wednesday night (18th) the House having resolved itself into Committee,

Sir R. Inglis repeated his objections both to the gift and sale of bodies for dissection, and proposed, as an addition to the clause, that governors of hospitals and gaols should be prohibited from giving over the bodies, of which they should have legal custody, to dissection.

The amendment was negatived by a large majority; and on a division, there appeared for the original clause 46, against it only 6.

Mr. Hunt then moved three amendments, which were successively negatived. 1. That the inspectors should personally examine the bodies previous to dissection; for the amendment 1, against it 49. 2. That a penalty of 100l. should be inflicted on all persons engaged in buying or selling dead bodies; ayes 11, noes 49. And 3. That it should be imperative on judges to order the bodies of persons convicted of murder to be dissected; for the amendment 1, against it 49.

The rest of the clauses were agreed to, and the Bill passed through Committee.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

POISONING BY GOOSE GREASE.

On the 2d of April, 1829, Dr. Siedler was called to attend MM. H——, and their children. On his arrival he found the two brothers H——, one aged thirty-one, the second twenty-eight years, and the two children of the first, one a girl, æt. four, and the other a boy, æt. two and a half, all presenting the following symptoms:—cold sweat, anxiety, vertigo, general paleness and prostration of strength, eyes sunken, and pupils dilated; burning pain was felt in the lower part of the belly, increased by pressure; violent vomiting, succeeded by ardent thirst, for which the patients had drunk large

quantities of milk, which was thrown up without producing any effect; tongue dry, involuntary discharge of urine and fæces.

The eldest brother was insensible for six minutes; his respiration was scarcely visible, his pulse imperceptible, and the heart's action exceedingly weak. The second brother had vomited blood several times, but he experienced less abdominal pain than the other. In the little boy the globes of the eyes were turned upwards, the lips livid, and the pulse scarcely sensible. Lastly, the symptoms in the little girl were the mildest of all. M. Siedler suspected at once that these accidents were occasioned by the use of a certain quantity of goose-grease, which had been employed in the preparation of some meat, of which the four patients had eaten shortly before the symptoms began. An emulsion, containing hyoscyamus, was prescribed, and on the 9th of April all had recovered.

The vomited matters were subjected to chemical analysis: they were strongly acid, but contained no metallic poison; but the following facts induced Dr. Siedler to attribute the illness to the effect of sebacic acid. The lady of the house had made use of goose-grease to dress some veal, and all the persons who partook of the dish fell quickly sick. The lady herself, who had barely tasted it, felt it so disagreeable that she took no more. None of the grease which was suspected to have caused the accident remained for examination, the pot which contained it having been entirely emptied and cleaned out; but on examining the same kind of grease contained in three other pots, it was found to exhale a strong repulsive odour, and it reddened strongly blue paper tinged by turnsole. Three ounces of this grease were given to a vigorous, well-formed dog; an hour after, his extremities became violently convulsed; he cried piteously, he refused to eat, his eyes were suffused, pupils dilated, skin cold, and arterial pulsations scarcely perceptible. In this state he continued for thirty hours, after which he slowly recovered *.

NEW METHOD OF DESTROYING RATS.

M. Tenard lately read a note to the Academy of Sciences, in Paris, in which he recommends the following as a simple and effectual method of destroying rats. If there be several of their holes, begin by closing them up: those in their most frequented routs will soon be reopened, and in this way their chief resorts will be discovered. Introduce into one of the apertures, (again closing the others,) the mouth of a glass retort, and make it air tight with common luting. Sulphuret of iron is then to be introduced into the apparatus by the tubulure, and diluted sulphuric acid poured cautiously upon it. Sulphuretted hydrogen is evolved, which, entering by the hole, penetrates all the crevices into which the rats may retire, and speedily destroys them.—*Gazette Medicale.*

NEW MODE OF EFFECTING INSTANTANEOUS VESICATION.

Cut a piece of linen, cotton, or paper, of the desired size and shape; dip it in spirits of wine, eau de cologne, or even in strong brandy; wring it, or wipe the surface, that none of the fluid may trickle from it to the adjoining skin; lay it on the part intended to be blistered; apply a lighted candle or paper, carrying the flame rapidly over the whole surface, that it may all take fire at once. The ignition does not last a minute, and the cuticle will be found to be detached and easily separable from the cutis.—*Bulletin de Therapeutique.*

THERAPEUTIC EFFECTS OF SUB-NITRATE OF BISMUTH.

The sub-nitrate of bismuth has been used pretty extensively during the last twenty years, but to this day no very definite ideas seem to have been arrived at as to its real effects. Dr. Lombard, of Geneva, with a view of setting the question at rest, lately instituted a series of experiments, of which the following are the results. That in doses of a drachm and upwards it acts as a corrosive poison; that it may nevertheless be exhibited with safety in quantities of from twelve to seventy-two grains in the course of twenty-four hours; that it acts as a sedative on the gastric nerves; that gastralgia, with vomiting, is more easily and more speedily cured by this than any other remedy; that pyrexia and gastrodynia rapidly yield to it; that acute vomiting may frequently be arrested by its means, and that it is useful in the vomiting which accompanies cancer of the stomach; and lastly, that it may be of service in convulsive cough.—*Ibid.*

BARRENNESS SUCCEEDING THE USE OF IODINE.

Dr. Robert H. Rivers, of Hardeman County, Tennessee, in a letter to our esteem-

* Hufeland's Journal.

ed collaborator, Dr. S. Jackson, of this city, communicates two cases in which barrenness followed the use of iodine. The first case was that of a lady affected with goitre, and who was married at the age of seventeen. During the first three years of her marriage she gave birth to a child annually. At the end of this period, her husband thinking that the goitre increased in size, applied to a physician, who administered iodine. Under the use of this medicine the goitre decreased, and with it the breasts also, so as almost entirely to disappear. From the time of her commencing the use of iodine to the present, a period of eight years, she has never become pregnant.

Another case, similar to the above, is also said to have occurred, in which the female became barren soon after commencing the use of iodine.—*American Journal of Medical Sciences.*

EXTIRPATION OF THE ENTIRE PAROTID GLAND.

By Dr. Mott, of New York.

Our friend, Dr. Vaché, in a letter to us, dated July 13, states that Dr. Mott the day previously succeeded in extirpating the entire parotid. "It was," says Dr. V. "perhaps double the size of a hen's egg, and although thought to be scirrhus, proved to be melanosis, and the most beautiful and perfect specimen of the disease I have ever seen. We have had a drawing taken previous to the operation, and of the tumor after it was removed. The patient is doing as yet uncommonly well, and bids fair to recover. —*Ibid.*

THE USE OF SULPHATE OF MORPHINE IN OPHTHALMIA.

Dr. Charles A. Lee, in a paper in our cotemporary, the New York Medical and Physical Journal, recommends the employment of a solution of sulphate of morphine in acute conjunctival inflammation, with intolerance of light. In one case of this description, attended with constant pain and itching, the relief is stated to have been immediate. Dr. Lee uses the solution usually of the strength of two grains of the salt to one ounce of water. It should be applied tepid.—*Ibid.*

ROYAL INSTITUTION.

Friday, April 13th, 1832.

Irritability as related to Respiration.

A LECTURE was given upon the relation between the respiration and the irritability in the different species and forms of the animal kingdom, by Dr. Marshall Hall, F.R.S. &c.

Dr. Hall described mere organic life as resulting from the impression of stimuli upon parts endued with irritability. The principal stimuli in nature are air, food, and heat; the principal and appropriate organs of irritability, the heart, the stomach, the muscular system in general.

The animal series consists of beings variously modified by the varied quantity of stimulus applied to them, and of the degree of irritability of their organs. Throughout the whole, these observe a ratio which is inverse: whenever the quantity of stimulus is high, the irritability is low; and whenever the stimulus is low, the degree of irritability is high.

As this law is most readily traced in regard to respiration and the heart (although it equally holds in regard to the stomach and its appropriate stimulus, food), it was in regard to the former organ and function that it was chiefly traced.

The oxygen of the atmospheric air is the more immediate and essential stimulus of the heart. Taken up in respiration, it is brought into contact with this organ by means of the blood, which may be considered as the carrier of this stimulus, as it is of temperature and of nutriment to the various parts of the system. As the oxygen is the principal stimulus, the heart is the principal organ of irritability in the higher orders of animals. If the first be removed, all animals perish in a greater or less period of time.

The quantity of air consumed, and the degree of irritability of the heart, are, in different species, and in the different forms of the same species, inversely proportionate to each other. An apparatus was shewn and explained, by which the quantity of respiration, or of oxygen gas, consumed, is accurately and readily ascertained. The degree of irritability is known by observing the duration of the beat of the heart, exposed to a given degree of stimulus too small for the indefinite duration of life; as in an animal in which the spinal marrow is divided so as to suspend the acts of respiration, and exposing the heart to the action of the atmospheric air merely.

It is invariably found, that when the quantity of respiration is high, the beat of the heart soon ceases; and that when the respiration is low, the contractions of this organ continue for a great length of time.

Dr. Hall first compared the individuals of the bird tribes, the mammalia, the reptiles, the batrachia, in different species. He then compared the fœtus, the egg, the tadpole, the larva, &c. with the same animals in the perfect state, as different anatomical forms; and lastly, the conditions of lethargy, of torpor, and of the privation of food, as examples of physiological changes. The

different species illustrate the law, by affording a high respiration and a low irritability, or the reverse, in the order in which they are here enumerated. Changes in anatomical forms appear always to be from lower to higher; and changes in physiological condition, from high to low.

But the most extraordinary illustration of the law, that the irritability is inversely as the respiration, is, in our opinion, presented by the double heart of the bird tribes and mammalia itself: the left side of such a heart receives respired blood—it is an organ of high respiration; the right side receives venous blood, and is an organ of low respiration. Accordingly, on suspending the respiration, the left side of the heart ceases to beat much sooner than the right.

As effects of this law, Dr. Hall pointed out, that where the respiration is high, or rather the irritability low, the degree of necessity for air and the animal temperature are great, the tenacity of life small, and there is greater power of bearing augmented than diminished stimuli. On the other hand, where the respiration is low, but the irritability high, the power of sustaining the privation of air and of food, and the tenacity of life, is extremely great, whilst there is a low degree of animal heat; and there is greater power of bearing diminished than augmented stimulus in general.

On the same principle, Dr. Hall arranges and explains the two beautiful series of experiments of Legallois and of Mr. Edwards, which hitherto remained but as isolated facts.

SPURIOUS LECTURES.

Several gentlemen have addressed us on the subject of a somewhat novel and vexatious kind of nuisance, to which they have lately been exposed, namely, that of being represented as contributing to a contemporary journal, with which they have no connexion whatever, directly or indirectly. The productions attributed to them are versions of their Lectures, so much garbled and so exceedingly incorrect, as to render it difficult for them to recognise their own property, except by seeing their names prefixed. One gentleman informs us that, in the space of only two pages, professing to contain a lecture of his, there were not fewer than twenty-nine essential errors. The proceeding complained of is consummately impudent, to be sure, but we believe there is no redress in law; the very curtailing and mutilation of the lectures, which deprives them of all value, likewise rendering it impossible to prove the degree of resemblance to those delivered, which is deemed necessary in a court of justice. On the other hand, we warn our correspondents, that nothing would serve the purpose of the parties so well as an action against them, which they think might possibly give them a little of the notoriety they stand so much in need of. Two circumstances ought to prevent our correspondents from taking any trouble about the matter; first, that a little patience will rid them of the grievance; and, secondly, that while very few ever see the lucubrations attributed to them, fewer still are so ignorant as to believe they really uttered the trash which is put into their mouths.

REPORT OF CHOLERA IN GREAT BRITAIN, UP TO FRIDAY, APRIL 20, 1832.

New cases in London since our last report	95
Deaths	40
Total number of cases in London since the commencement of the disease	2477
Deaths	301
New cases in other parts of Great Britain since our last report	431
Deaths	221
Total number of cases throughout Great Britain since the commencement of the disease	10716
Deaths	4179

M. BERZELIUS.

A report of the death of this distinguished chemist made its way lately into some of our literary journals. We are happy to perceive, however, by the most recent arrivals from Stockholm, that M. Berzelius was in the active discharge of his duties, and, we should hope, in perfect health.

BOOKS RECEIVED FOR REVIEW.

A Practical Treatise on Uterine Hæmorrhage, in connexion with Pregnancy and Parturition. By John T. Ingleby, Member of the Royal College of Surgeons, London, &c.

The Dissector's Guide; or, Student's Manual. By Edward William Tuson, F.L.S., &c.

NOTICES.

Dr. Wayte's letter shall appear next week.

We shall be happy to hear from Glasgow again, and to the same address as before.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 28, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Part I.—Lecture XXIX.

Contined Fever, continued—Predisposing Causes.

I mentioned, gentlemen, in the last lecture, as the predisposing causes of continued fever, mental depression, grief, and anxiety of mind, as well as corporeal depression, over exertion of either mind or body, excessive venery, debauchery, want of good food, want of fresh air, and a number of circumstances that throw the body out of health,—more or less into debility. It is found, I mentioned, that the extremes of age are less liable to continued fever than the adult period, short of old age. Perhaps, also, a peculiar susceptibility exists in some persons; for many fall into fever from causes which certainly in others produce no such effect, although the circumstances are in all respects *apparently* the same.

Some of the predisposing causes may, by their continuance, become exciting causes. It is possible that the continuance of debauchery may, without any additional exciting cause, increase the disposition so much that at last the disease itself begins, requiring nothing at all in addition to produce it. One cause certainly of one kind of continued fever is exposure to cold, especially when the body is over heated. Many cases of fever appear to have nothing to do with any other exciting cause than exposure to cold, especially when this is united with wet, and more particularly when the body is overheated and fatigued. Besides these very common causes of continued fever, there is generally allowed to be one of a peculiar description—contagion. Before, however, considering this point, it will be necessary for me to make some remarks upon the subject of contagion in general.

Contagion.

Definition.—By contagion, I believe, is generally meant, either a peculiar matter generated *in*, or a depraved secretion *of*, a living system under disease, capable of producing the same disease in others when there is no indisposition to it, and more especially if there be a predisposition. This, I conceive, is what is generally understood by contagion.

I have used two expressions—either a "peculiar matter" or a "depraved secretion," because in the greater number of instances of contagion it is a depraved secretion. Very frequently it is pus—pus frequently in small-pox, pus in syphilis, pus in glanders, pus in a variety of diseases. In the case of hydrophobia it is either mucus or saliva; and in other instances it is apparently mucus. In the case of the skin it sometimes appears to be sweat, but for what we know, it may be sometimes separated from the body unconnected with its real natural secretions. It is possible that an emanation of some kind may take place from the surface of the body or the lungs, and infect the air independently of the aqueous fluid which is separated from those organs, but whether such is the case I do not know; yet to avoid the objections of those who admit such a thing, I think it better to say that it is either a peculiar matter or a depraved secretion. I have said *living system*, to avoid the objections of those who might urge that contagion occurs in vegetables as well as in animals; and of course it is the characteristic—the necessary attribute of a contagion, that it should be able to produce the very same disease in others—not that it can produce it in *all* others, because there

are some that will not take a particular contagion, but able to produce it in others who have a predisposition to it, or rather, I should say, who have no indisposition to it. I said *another*, referring to the antecedent phrase *living system*, because contagions which are produced by one species have, in many instances, affected other species. There are several diseases of the inferior animals, or brutes, which may be communicated to the human body: it is, therefore, necessary to say, produced in a living *system*, and capable of exciting the same disease in another living *system*, not of the same species merely, but another living system, speaking at large.

Acute and Chronic.—Now when diseases are so produced, they may be in the first place either acute or chronic. What are called the exanthemata are acute diseases, as scarlet fever, measles, small-pox, chicken-pox, cow-pock, hydrophobia, typhus (if it be a contagious disease), and plague; whereas the itch, syphilis, porrigo, elephantiasis, the yaws, the sibbins, and a disease called laander, and peculiar to Africa, always become chronic if not checked, and are not more active at first than afterwards. Some contagious diseases may be considered both acute and chronic. The hooping-cough (if it be a contagious disease) comes on usually as an active disease, with all the activity of most acute diseases, and yet it may run on for a length of time, so that it may be either acute or chronic; whereas scarlet fever, measles, small-pox, chicken-pox, hydrophobia, and typhus fever, cannot be chronic—they are always acute. Syphilis, which I mentioned as being a chronic affection, cannot indeed be so considered when it first begins, but it rarely has, comparatively, the activity which characterizes acute diseases. Hooping-cough, however, is generally active at the onset, and frequently it has the activity of small-pox or measles, but it may run on for many weeks—perhaps even months.

Febrile and Non-febrile.—Contagious diseases, both acute and chronic, are sometimes febrile and sometimes non-febrile; and therefore it is better to divide them into acute and chronic. Hydrophobia is an instance of an acute disease of contagious kind, but it is not in the least febrile.

Contagions and Infections.—Some of these diseases must be communicated by contact, either with the patient or something that he has touched, or some palpable matter that has proceeded from him. Of this kind are itch, syphilis, cow-pock, hydrophobia, elephantiasis, yaws, sibbens, laander—the three latter of which I have never seen—and the glanders of horses, together, in all probability, with the plague, and porrigo or scald-head.

Some, again, may be communicated both by contact and by merely the atmosphere surrounding the patient. In the latter case there must be the contact of something which has emanated from the patient, but then that something has not emanated in a palpable form. It is not the contact of any solid or any liquid that is to be seen, but the contact of something invisible; still that something must have proceeded from the patient. Now the diseases which may be communicated both by contact with the patient, or something that he has touched, or something palpable which has proceeded from him, or may be communicated by merely being exposed to his atmosphere are, the small-pox, the chicken-pox, scarlatina, and the measles. The latter, as I shall have occasion to state hereafter, have certainly been inoculated, just like small-pox. It is probably by means of something in the atmosphere which is not palpable, but which has emanated from the patient, that we may become the subjects of typhus—if it be a contagious disease—and hooping cough. Even if an individual touch another labouring under these diseases, still it is probably not only the contact, but an impalpable emanation from the patient that gives the disease.

Distinction between Contagion and Infection.—Now the adjective *contagious* embraces all these diseases, but it is also employed to distinguish those which are communicated solely by contact with the patient, or something that he has touched, or something which has proceeded from him palpably; whereas, the word *infectious* is given to those diseases which may be communicated by merely being exposed to the patient's atmosphere. The word contagious is used in the same way as the term horse. Horse is used to include both a horse and a mare, but it is frequently applied to the male only, and so these diseases are all continually spoken of as contagious, but the word contagious is *also* employed in a limited sense, to signify those diseases which are communicated by actual contact, or by touching something which the patient has touched, or something which has palpably proceeded from him. You will hear quibbles about this, but it appears to me that the word contagious is used as I have now stated—that you may separately have a contagious and an infectious disease, but you may express them both, according to established custom, by the word contagious.

Contagion more speedy in its operation than Infection.—Some diseases are both contagious and infectious—may be communicated by contact, as I have already said, or by merely being in the neighbourhood of the patient. Now it is observed, that when a disease may be communicated in both ways, it is communicable more quickly by conta-

gion than by infection. If two persons be exposed to an atmosphere infected with small-pox, and you inoculate one with the virus, the disease will appear in him much sooner than in the other. It is a well-ascertained fact, I think, that contagion, in the limited sense of the word, excites the disease sooner than infection, when the disease may be produced in both ways. Hence it is the practice, in regard to small-pox, when a person is exposed to the infection of the disease, to inoculate him as quickly as possible, in order to bring the disease on in the artificial manner before it can arise by infection.

Diseases occasionally Contagious.—Again, some diseases are thought to be only occasionally contagious—I use the word generically. Catarrh is supposed by many to be sometimes infectious, but I do not know whether it is. Ophthalmia appears certainly sometimes to be contagious; in the greater number of cases it is not, but it would appear, undoubtedly, in certain cases to be contagious. Erysipelas is not considered to be a contagious disease; but Dr. Wells, in a paper published in the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, furnishes very strong reasons for suspecting that certain instances which he details were instances of erysipelas by contagion. Continued fever may be an instance of the same kind: certainly it arises continually without contagion, but in other cases I am very much disposed to think that it is contagious.

Diseases Contagious for a limited period.—Some diseases, again, are contagious for a time only. This is the case with gonorrhœa and with hooping-cough. There is no doubt that, after a certain time, the discharge from the urethra, call it what you please, gonorrhœa or gleet, is perfectly inocuous—can communicate no disease. It is the same with hooping-cough: children will continue to hoop long after any chance exists of communicating the disease to others.

Partially Contagious.—It is said that some diseases are only partially contagious. The instance of this which is usually alleged, is syphilis. The matter from the primary sores of syphilis—a primary bubo—will communicate the disease; it is contagious enough, but it is said that the matter of secondary sores is not contagious. It is impossible to make experiments on this subject, and therefore I cannot tell you whether those who assert this are correct or not.

Contagious Diseases generated de novo.—Some contagions, again, appear to be occasionally generated afresh. Some of those diseases which will give rise to a something capable of producing the disease in others, do appear to be produced *de novo*. The itch, at any rate, will sometimes occur in patients after continued fever, where it is not possible to trace it to any other persons. Hydrophobia, there is every reason to believe, will sometimes spring up in the same way. Dogs become the subjects of the disease without it being in our power to trace it to any other animal; where there is no probability of their having been exposed to contagion. The same is true of the glanders in horses. This is also said to be the case with regard to typhus—that a patient has become the subject of it from a mere common exciting cause, and that he has been known to give it to others. Of this I cannot speak, because I never saw typhus fever contagious. With regard to all these diseases—itch, hydrophobia, and typhus—granting the latter to be contagious, whether you choose to believe that they will ever occur *de novo* or not, still they must have had that origin at some time or other; and it is not more wonderful for them to occur *de novo* now than that they were originally produced. There must have been a person or brute who first had measles, small-pox, scarlet fever, and so on, and who could not have derived it from any other being. If we even go to brutes, still the difficulty is the same; some one of them must have had the disease first of all—it must have occurred *de novo* in some one instance or other. Indeed it is not certain that small-pox, chicken-pox, hooping-cough, scarlet-fever, and measles, do not occur from time to time without contagion. We frequently see children labouring under these diseases who have been shut up in the country, without having had communication, directly or indirectly, with any others who could have had them, or having been near persons who had visited others suffering the affection; at least so far as we can trace it. It is possible, however, that an error may exist on this point—that there has been communication; but certainly the absence of communication, direct or indirect, occurs so often that I do not think we have a right to assert that they never occur *de novo*. I give no opinion on the subject, but I would not be positive on the negative side.

Contagious diseases occurring but once.—Some contagious diseases occur rarely more than once; among which we may mention cow-pock, chicken-pox, small-pox, measles, hooping-cough—by which I mean the real disease, not a spasmodic hoop—and scarlet fever. We must all have seen instances of the second occurrence of these affections, and sometimes perhaps the third, but the general rule is for them to take place but once; while the rest may occur more than once, and, indeed, some of them an indefinite number of times. It is unfortunately too true that some persons suffer syphilis and gonorrhœa over and over again; not, certainly, to

all eternity, but as long as they expose themselves; for it would not answer the moral end of these complaints were they, like small-pox and measles, to be had but once. With regard to hydrophobia, it is not known whether it may be had twice or not. So far as I am acquainted with the subject, both human beings and dogs have always died of the disease when seized with it, and therefore we cannot tell whether it may be had a second time.

These classes have no relation to another.—Now, although you may put each of these diseases into one class or another of those I have been enumerating, yet it does not follow that a disease which is placed with another in one class, will be side by side with it in another class. These classes are all distinct—the characteristics of each have no relation to those of another; the diseases which meet in one class will not meet in another, and the affections which are of different classes in one respect will meet in another class with others. For example, hydrophobia is of the class of acute contagious diseases, together with small-pox—both are acute diseases; but small-pox is in the class of those which may be communicated either by the atmosphere or by contact, whereas hydrophobia is in the class of those that can be communicated only by contact—by the application of palpable matter. Itch and syphilis meet in the class of those that can be communicated by contact only, yet there is no reason to believe that the latter occurs *de novo*, while I think we have some reason to believe that the former does. The cow-pock is in the class of those which can be communicated by contact only—and so is itch; yet cow-pock is in the class also of those which occur but once, whereas the itch is in the class of those which recur indefinitely. Hence you see that the classes have no relation whatever to each other.

One Contagious Disease preventing another.—Another circumstance I may mention is, that we have an instance of one (or at least we generally have an instance of one) contagious disease preventing another. The cow-pox is supposed to, and for the most part it certainly does, prevent the small-pox; but it is to be considered that it is not by any means proved that cow-pock and small-pox are not the same diseases, the former being modified. If they be but one disease, the fact of cow-pock preventing small-pox merely resolves itself into the fact that some contagious diseases do not usually occur more than once; but if they be distinct diseases, then it is an instance of one contagious disease preventing another.

These are short general views in regard to contagious diseases.

Interval between the Application of the Poison and the Appearance of the Disease.—When a contagion has been applied, there is for the most part an interval before it operates—there is usually an interval between the application of the poison and the appearance of the disease. This interval is various in different diseases, and it is various in different cases of the same disease. The interval of small pox is very short compared with the interval of hydrophobia; and then again, the interval of the latter is by no means always the same; it will vary from a few weeks to a few months.

Contagion destroyed by Dilution.—Some of these contagious diseases which are infectious, using the word contagious in a generic sense to comprise both, are very easily prevented by dilution of the atmosphere. You will find a great difference in diseases in this respect. The contagiousness of diseases which are infectious, or, if you choose to say, the infection of some diseases, is very easily annihilated by dilution, whereas great dilution has comparatively little effect on others. The infection of small-pox, and of measles, will sometimes operate in a very free ventilation. It is thought by some that their infection will not extend in pure air to a very great distance. But at any rate, in the purest air, we see these diseases caught by children in the neighbourhood of those that labour under them, whereas other diseases cannot be caught, if any pains at all be taken to dilute the air. As a remarkable example of this, I may mention that, if continued fever be a contagious disease, the contagion may be dissipated with the greatest ease; a comparatively trifling dilution of the atmosphere around the patient will prevent it from being communicated to any one; whereas a great dilution will frequently not prevent the small pox or the measles. So easily is the contagion of continued fever—supposing it to be contagious—dissipated, that if there be a very free ventilation around the patient, there is little or no danger of any one catching the disease; and it is asserted that, in small pox, the contagion in the worst cases will not extend beyond half a yard in the open air. Dr. Haygarth, in his celebrated letter to Dr. Percival, says that the infection of small pox will not extend beyond the distance of half a yard, and of continued fever much less; and this is one reason why its contagion has been denied altogether. You will find many persons who deny that continued fever is ever contagious. Granting, however, that it is contagious, we may have another reason for explaining the circumstance of it frequently not spreading far;—it may not be always contagious, but only occasionally so, like perhaps erysipelas. Another reason is, that persons for the most part escape the influence of the contagion if they be in good health and spirits while exposed to it. Now I presume that no good health, no good

spirits, neither good nourishment, nor any thing however favourable to health, will render a person less disposed to catch the small pox, or to catch syphilis; but it is a fact, supposing continued fever to be contagious, that if a person be in excellent health and spirits—if he have every means of promoting health in his power—if all be healthful in and about him, he may be exposed to the emanations of a person labouring under the most virulent typhus, and yet for the most part escape. If, however, his mind become depressed, if his body become debilitated in any way, then you will see the same person become the victim of the complaint when exposed to the emanations of a typhous patient in so slight a degree, that you would hardly suppose it possible for the disease to be communicated. Many persons are exposed with impunity to the emanations from patients lalouring under typhus, till their mind desponds, or by some chance they are thrown out of health, and then the contagion—if it be contagious—acts as a predisposing cause; and then this circumstance of depression of mind, or the accidental depression of the powers of the body, acts as an exciting cause. Exactly as in the case of ague, a person may be exposed to malaria, and not suffer the disease till he gets cold and wet through; the common original cause of the disease, the malaria, being the predisposing, and catching cold the exciting cause; so in typhus, the peculiar contagion is often the predisposing, and depression the exciting cause. I mentioned that, in the instance of the plague, persons have been known not to experience the disease till they have lost some of their most intimate friends, and have thereby become depressed. Diembroek, I stated, mentions an instance of this kind, and there are hundreds similar.

Now these things are all to be considered when we question whether continued fever is contagious or not. It is allowed by those who contend for its contagiousness, that the contagion is one which is most easily dissipated by ventilation; then, in the next place, it is allowed by them, that if a person be in good health, with every means of health in full play, he will generally escape; and if it be a fact that the disease is sometimes contagious, and sometimes not, and sometimes arises *de novo*, then we see another reason why some people have denied the contagion of the disease altogether. However, I am quite sure that much of the difference of opinion on this subject must have arisen from this circumstance, that many diseases have been called typhus, which were no such thing. There can be no doubt that many cases of continued fever which we see, are really cases of remittent fever dependent upon malaria, having nothing at all to do with contagion. Persons may be exposed to individuals labouring under remittent fever, which we see every day, and which nice observers may easily distinguish from typhus—persons are exposed to this every day without suffering the disease, and therefore typhus is continually said not to be contagious, when it is really remittent fever under which the patient labours, and which is not a contagious complaint.

Diseases communicable from Brutes to Man.—Some contagious diseases are communicated from brutes to man. Hydrophobia, cow pock, and, it would appear, small-pox, are of this description. Every one knows that the cow-pock may be given to the human subject; but it is related, and therefore I suppose it is true, that some experiments have lately been made to give the small-pox from the human subject to the cow, and these have been adduced as proofs that the two diseases are the same, because the disease produced in the cow was the cow-pock. The itch is a disease communicable from brutes to man, at least it is said that a kind of itch is produced from mangy dogs. There is a peculiar disease in Germany mentioned under the name of *milzbrand*—inflammation of the melt, a peculiar disease of brutes analogous to what has been called *malignant pustule*, and which is communicable to the human subject. Many persons have had malignant pustules produced in them by merely touching the blood of animals labouring under a certain disease, or perhaps by merely putting a knife wettedwith the blood into their mouth. The glanders of the horse is another disease which unquestionably may be communicated to the human subject. It is possible that all those diseases which may be so communicated from brutes to man, may be given back. M. Magendie has made experiments with regard to hydrophobia. He inoculated dogs with the saliva of a man (I believe only one) labouring under hydrophobia, and, he says, that they became the subjects of the disease. The cow-pock, I believe, may be given from the human subject to brutes; and so may, unquestionably, the glanders. Whether any other contagious diseases than those I have enumerated may be given from brutes to the human subject, I do not know; but I should think all these may be given back. It is said that there are some other diseases which may be communicated from man to brutes. The plague, it is said, has affected brutes; sheep have been seen to catch the measles; and the disease called *trichoma* may also be communicated to brutes.

Bad Health.—I mentioned that it appears some contagious diseases are very much disposed to, by being out of health, and I instanced typhus fever; whereas, other contagions act as well upon persons in health as out of it. But it would appear that some-

times bad health will prevent a contagious disease. It is well known, I believe, among vaccinators, that they frequently cannot give a child the cow-pock when it is labouring under any cutaneous affection. This may simply arise from the body being indisposed to take on two diseases at once; not that the body is out of health altogether, because a cutaneous disease is frequently a slight affection, but it is found by many who vaccinate to a great extent, that they cannot give a child the cow-pock when it is labouring under any disease such as scald-head, or any chronic eruption.

Habit.—Habit lessons the susceptibility of the operation of some contagions; at any rate, persons who are continually exposed to the infection of typhus fever, in its most concentrated form, generally escape, and so with respect to the plague. A new comer into some situations will sometimes suffer immediately, while those who have been accustomed to it will go on with impunity, and never suffer. Habit appears to have a great influence upon contagion, just as it has with regard to the aguish effect of marsh miasmata.

Indisposition.—Some persons have an unaccountable indisposition to certain contagions. Some children will not take the small-pox, though they sleep in the same room, and even in the same bed, with others labouring under it; the same has been observed with regard to the measles, and, what is very singular, after a lapse of time, sometimes a year or two, and sometimes longer, the least exposure will produce the disease in the same individual. It is well known that some persons run into all sorts of risks with respect to syphilis and gonorrhœa, and invariably escape, without using any precaution, though as bad as their neighbours; in fact, I have known persons of that description, and yet after a time they began to catch the disease whenever they run the chance of so doing. It is the same with respect to small-pox and measles in young persons, as it is with regard to syphilis and gonorrhœa in adults. The extremes of age are very insusceptible of many infections.

Again: some persons, without any indisposition, as it would appear, escape, without our knowing why. A person shall be exposed, for example, to the poison of syphilis to-day —the fact of the existence of morbid secretion being proved, because others, yesterday or to-morrow, suffer from the same source—and catch nothing, and yet a fortnight afterwards the same source may give the disease to him. Persons sometimes do escape without any indisposition, without our knowing why.

Denial of Contagion.—Now you would hardly suppose it, but such is the case, that some persons deny there is such a thing as contagion at all. Some deny the contagiousness of one disease and some of another; but there are some who say that contagion was unknown in ancient times, and that it is altogether a modern invention. There is, or was, one person so convinced of this, in the case of hydrophobia—contending that it is a mere imaginary disease, (that the symptoms which have occurred, have never taken place, at any rate, through hydrophobia)—that he has written a pamphlet to prove it. He says that he has inoculated himself with the saliva of a rabid animal, again and again, and has escaped. His escape, however, is no proof of the non-existence of contagion, because many persons escape all sorts of contagions every day. Two medical students, at Paris, went so far as to deny that syphilis was contagious; and in the year 1823 (only eight years ago,) they inoculated themselves with some syphilitic matter. They laughed at contagion, and gloried in having some syphilitic matter under their skin. However, the consequence of this experiment was a very bad suppuration in, and a partial destruction of, the axilla of one young man, he having inoculated himself on the hand; and the other had a bad ulcer somewhere else, and became so distracted at what had happened, that he actually committed suicide by opening the crural artery. Others have done exactly the same in the case of the plague, and they have suffered exactly in the same way. A Dr. Whyte was convinced that the plague was not a contagious disease, and he therefore rubbed some pus, taken from the pestilential bubo of one of his patients, inside his own thigh; and not contented with this, he inoculated his wrist from another pestilential bubo, in the pest house of the Indian army at El Hammed. Four days had hardly passed before rigors began, and all the symptoms of violent fever, and he died of plague before the end of the third day. You will find it mentioned in the Journal de Medecine for May 1811, that an Italian physician, or an Italian doctor of some kind, called Dr. Valli, was so convinced of the non-contagiousness of the plague, that he mixed the matter from some pestilential buboes with a number of other things, and formed it into an ointment, designating it his *pomade*. He rubbed this mixture on the eyelids of people who came to him with sore eyes, on the abdomen of others, &c., and thereby communicated the plague to thirty individuals. He was quite sure that he was doing no harm whatever. The Turks, however, thought differently; and disapproving of his practice, cut off his head.

ABSTRACT
OF

LECTURES ON THE SUPPOSED CONSEQUENCES OF INFLAMMATION,

Delivered in the Medical Class of the University of Glasgow,

By Dr. Badham.

Suppuration not an effect of Inflammation.

The object of my last lecture was to shew that neither the suppuration, ulceration, or gangrene of any tissue, need in any instance be assigned to previous inflammation of that tissue; these, as well as certain other familiar morbid appearances, being clearly results of some added and peculiar cause which may be either associated with inflammation, or operate independently of it.

1. As to suppuration, to which our inquiries were first directed, I endeavoured to ascertain how far the current opinion which ascribes that process to inflammation, was grounded upon correct reasoning and legitimate induction. In order to do this, having collected such evidence as the most approved writers on this subject afford, I proceeded to consider its real amount in the question to be determined. When Hunter tells us that inflammation leads to the formation of pus, "by being carried so high as to destroy that state of parts on which its own existence depends, the consequence of which is that they lose the inflammatory action and acquire that which fits them for forming pus," it is very reasonable to inquire how, consistently with the terms of this proposition, the suppurative process should fail to occur in several intense cases of highly inflammatory diseases; how it occasionally happens that the most confirmed inflammations, seated even in the cellular membrane (that tissue which, of all others, the most readily assumes the suppurative action), should fail to terminate in this way, as the experience of many surgeons besides Dupuytren sufficiently establishes. I see but one reply; which is, that simple inflammatory action cannot, whatever be its intensity or duration, "put the inflamed parts," to use the words of Hunter, "in a way to form pus."

Inflammation appears to me, though this is far from the current opinion, to be always the same action, in whatever constitution, on whatever tissue, and by whatever cause excited. The different appearances which are wont to attend or terminate its progress, seem rather to depend upon some other morbid actions yet unexplained, with which it is conjoined and by which it is modified. Wherever suppuration occurs, I apprehend that there are two distinct and independent actions assumed by the part, and the phenomena which manifest themselves in consequence, I conceive to be of a compound character. All the phlegmasiæ, without exception and of whatever tissues, consist probably, at their commencement, in the same identical action of the parts concerned. This identity is, however, frequently lost in their progress, and towards what is called their terminations in the various degenerations of tissue, and in suppuration, ulceration, and gangrene. Now, in order to deduce phenomena which have not a shadow of affinity to each other, from the one efficient cause, inflammation, it is usual to have recourse to supposed peculiarities in the texture of the part itself, or of the constitution of the individual affected. The appearances, however, here alluded to, and commonly called terminations of inflammation, may well appear, if it were for no further reason than their extreme dissimilitude, not to be so much results of inflammation as of a distinct and separate, though totally unknown, agency. To this heavy objection, the dissimilitude of supposed results which so intentively presents itself, many other reasons for distrusting the commonly received doctrine will occur on reflection; none, perhaps, greater, than the total want of proportion so often witnessed between the amount of the inflammatory action and the extent of its supposed consequences, added to the impossibility of foreseeing which of these results, or whether any of them, will happen till it has actually taken place.

To me it is more satisfactory, for many reasons, to presume that the remote cause which determines a given inflammation, may also determine what have been considered loosely as its ulterior consequences, than to charge so many anomalous appearances upon inflammation as their efficient; just as Dr. Elliotson has correctly remarked, that the diseased spleen attendant upon ague, is caused, not by the intermittent, but by the cause of the intermittent; since its appearance at all does not depend on the violence of the ague, or its duration, and since the amount of it, when it does happen, does not depend on the gravity of the disease with which it is so frequently connected. As to the verbal qualification of the term inflammation, by the epithet of "specific," as this form of expression scarcely conveys any distinct sense, and is not the representation of a fact, we might be well content to discard it; but supposing we retain it as an old familiar, with what propriety can we affix it to the suppuration of a bubo, for instance, and deny it to the suppuration of a phlegmon, since the action concerned must be the same, since the product is the same (Hunter), and there is no difference but in the remote cause? If inflammation be any thing, it is but one thing—proceeding by certain laws, and involving certain essentials.

Inflammation, strickly speaking, perhaps can be said to have no one natural termina-

tion except the return of the parts to a natural state; and suppuration, ulceration, and gangrene, have, I apprehend, as much right to be considered as results of a specific inflammation (for, seeing they are different, they cannot be results of a common inflammation) as the infinite variety of ulcers in the various tissues, or of exanthems on the skin. Each of all these appearances may be connected with a "travail inflammatoire," but each may be brought into existence independent of it.

But it may be said, if inflammation and suppuration depend upon different efficient causes, why does the former affection so constantly introduce the other?—and does not the very order of appearance indicate inflammation as the cause, and suppuration as the effect? But even suppuration, as we shall see, does not absolutely require inflammatory action; and as to the general fact of the former affection coming after the latter, when they are seen together, neither would this admission make their relation to one another necessarily that of cause and effect; for both may as readily be effects as one certainly is, of unknown causes. It is as easy to suppose the two tendencies, to inflame and to suppurate, to exist independently of each other, as to maintain that one induces the other; and as we cannot tell under what law of the economy inflammation itself is developed, we may be content to be ignorant of that disposition of parts, or that agency upon them, which determines suppuration. When, in conformity to some occult principle of the economy, inflammation is set up, it may, so far from producing, even oppose the formation of matter, which may come afterwards to manifest itself, perhaps only because the inflammation has gradually subsided, and when "the inaptitude of parts to display two diseased actions simultaneously" no longer prohibits. As the germs of two of the exanthemata, measles and scarlatina, may both be received into the system together, but shew themselves in the order of their respective incubation, the same may be true of inflammation and suppuration.

In further distrust of the logic which asserts the connexion of cause and consequence between inflammation and suppuration, I shall now attempt to shew,—

1. That suppuration may exist without having been preceded by inflammation; and

2. That if phlogosis did always precede the suppurative process, even this would not assure the alleged nature of their connexion.

I pass over the frequency of inflammation unfollowed by suppuration, though a very considerable objection in itself, and, in asserting the first proposition of suppuration without inflammation, have only to refer to authorities which go back at least as far as De Hâen. This proposition may not be currently admitted, yet the names in support of it are too respectable not to entitle it to consideration. Purulent collections are occasionally found in the parenchyma of parts which certainly had not previously been inflamed. Cases have not unfrequently been recorded by various authors, in which the brain, the liver, the spleen, and the kidneys, have become the seats of purulent collections, though not only the autopsy has discovered no trace of inflammation in the part containing the pus, but no evidence from symptoms could be pretended of such a process having existed; and where, the organ being in every other respect quite natural, one would be inclined to say, with Andral, "that the purulent collection was merely deposited on the surface, or in the tissue of the organ which secreted it." This distinguished pathologist declares that he has met with instances in which "almost every organ of the body has become the seat of deposited matter, and yet there has been no trace nor evidence of preceding phlogosis. In one such case, where numerous and vast abscesses had occurred in different external parts of the body, without inflammation, the patient voided with his urine a white, thick, and puriform discharge, after the evacuation of which he got completely well." The same author also assures us, that twice in one year, during his attendance at La Charité, he saw "purulent matter embedded in the middle of a clot of blood in one of the cavities of the heart, this organ not having afforded any trace whatever of disease." Dr. Latham, in his valuable lectures on diseases of the heart *, has noticed a similar appearance, though we do not learn whether the precursory symptoms were inflammatory or not. Andral states, that he has several times detected, with his friend M. Reynaud, "a substance similar to pus" disseminated throughout various coagula of blood in the *cellules* of the healthy spleen;" and Abernethy also states, that he has found an abscess, containing genuine pus, in the middle of an adipose, sarcomatous tumor; which tumor, he admits, to be wholly uninflammatory in its progress, during which period the purulent secretion of course took place. I shall not inquire whether the first of these cases, notwithstanding the rarity of their occurrence, do not go some length in support of the hypothesis of De Haën, that pus is formed already in the blood in which it is thus included; but at any rate they serve to shew, that a genuine suppurative process need not have its origin in inflammatory causes, since the efficient cause that can produce pus in one instance, can of course produce it in all, and since two causes are

* See Medical Gazette, Vol. III.

not to be invoked where one is sufficient*. "Que de fois," says Roux, to denote the frequency of the occurrence, "que de fois, à l'ouverture de cadavres, on découvre des abcès dont on avait à peine, ou dont on n'avait même pas soupçonné l'existence!" Hunter, even while he maintains that true suppuration is produced by inflammation, is forced to admit two kinds of pus arising from two kinds of action, each generating its own peculiar fluid. Now the fact seems to be, that the characters of these two purulent secretions, of this inflammatory and uninflammatory pus of Hunter, are seldom so great, and are sometimes so trivial, that it is difficult not to recognise them as various modifications in intensity merely of one and the same action. Dr. Thomson, in his work on Inflammation, admits that he has frequently found no difference between the products of inflammatory, and what the French call "cold" abscesses; and Mr. Abernethy has given some cases of lumbar abscess where inflammatory symptoms had been absent, and yet nevertheless where the pus drawn off was perfectly healthy in its appearance; now, if any one species of suppurative action be, as Hunter admits it is, uninflammatory, the inference appears correct that no purulent collection can be the simple result of phlogosis. Abscess may be divided, according to Roux, into "abcès aigus ou chauds," and "abcès chroniques ou froids," which run insensibly into one another; while a third division comprehends abscesses which make their appearance suddenly, and "are acute," if we look merely to the quickness with which they have been developed, but chronic *(froids)* as respects the want of antecedent inflammation: now as these various sorts of abscess are alleged to arise from a common cause, and to be insensibly lost in each other, it should seem that this common cause is either always of an inflammatory nature, or never. Dupuytren considers all abscess to be of the same nature; I agree with him as to that point, but not in the conclusions he has drawn from it. "L'ancienne division des abcès," says this writer, "en abcès chauds ou inflammatoires, et abcès froids, considerée comme s'appliquant à des tumeurs de nature différente, et provenant des vitales dissemblables, doit être rejetée. Les tumeurs purulentes ont toutes la même origine — l'inflammation; celle-çi ne manque dans aucun cas, mais elle peut affecter des tissus différents, presenter des nuances d'intensité variables à l'infini, et selon les circonstances, les collections purulentes se forment avec lenteur ou rapidité, acquièrent des dimensions considérables ou restreintes, et renferment des liquides dont les qualités diffèrent singulièrement." From this passage we learn that Dupuytren saw the necessity of confining all abscesses to one common law, always inflammation, but contends that the inflammation is not unfrequently latent: but what is latent inflammation? we cannot infer that any thing exists except by its signs; it seems more philosophical to hold, that abscess never results from inflammation than that it always does. The absence of symptoms will sustain the former position, but leaves us in hopeless confusion if we adhere to the latter.

2. I come now to the second point contended for—that inflammation always precedes or attends suppuration. Suppose, for argument's sake, that it did so, does this concession establish the relationship of cause and effect between the two? Cartilage is not esteemed the producing agent of bone, because, at least in some bones, cartilaginous structure precedes osseous deposition; for other bones are formed without passing through the intermediate stage of cartilage at all, and some cartilages continue such through the whole of life: indeed, there are various procedures in the animal economy in which two functions are seen closely united, and yet most certainly it is not as cause and consequence. It was stated, for instance, in my lecture on muscle, that, where animal temperature was highest, the energy of muscular contractility was also greatest; but it cannot be maintained, on that account, that contractile action depends upon animal heat. Any apparent relationship of cause and effect is, in this latter instance, destroyed by the consideration, that, after death, and before the animal temperature is dissipated, contractility has ceased; thus, as in this very instance, an eminent physiologist, Bostock, considers contractility as the "unknown cause of known effects," I would consider suppuration as the effect of a still unknown cause, which may be often, but is not always, conjoined with the cause of inflammation. Again: the exhalation of carbonic acid from the blood, and the change of colour which that fluid undergoes as it passes into the pulmonary capillaries, are phenomena so united in the process of respiration as to appear synchronous; but is one the cause of the other? Certainly not; for, as Contanceau ingeniously remarks, all blood, that has formed a secretion of any kind, becomes dark in consequence, and as the carbonic acid and watery halitus thrown

* If it be said that this line of argument is founded, not on what is usual, but the reverse, and that exceptions confirm rules, I answer, that exceptions confirm rules in grammar, or any work of man's ingenuity, but not so in physics. Physiology and pathology are subject to no real anomalies. We must not, then, throw any facts, under the false name of exceptions, out of our account in medical investigation; but should attempt rather to reconcile them with acknowledged principles than oppose them to these principles, applying the remark of the learned Decandolle on monstrosities, "qu'ils sont pour ainsi dire des experiences que la nature fait au profit de l'observateur."

off from the lungs are to be regarded in the light of secretions, these respirations cannot be the producing agents of the light colour of the blood. "La formation de l'acide carbonique et de la vapeur aqueuse d'une part, et de l'autre la coloration du sang veineux, n'ont point une cause commune, et doivent être considerées comme deux phenomènes indépendans, quoique survenues dans la même acte functionnel." The external material world might be made to furnish abundant instances of coincidence without connexion,—light without heat, heat without light, &c. &c. but these are less available to my argument*.

On the whole, if we adopt Marjolin's theory of inflammation, which I believe to be a true one, namely, that it is a mere "exaltation des forces vitales,"—an excess of action in a part, but still of natural action, we are plainly at a loss to conceive that it can furnish any new products, either as regards change of structure or morbid secretion. One argument, however, remains to be noticed, before I conclude. It is said, that as all pus is known to change its qualities and consistence according to the variations in intensity of the antecedent inflammation, this is a fact which plainly points to one as the cause, the other the effect. But it is not "peculiar to inflammation" to alter the character of pus; it has been ascertained that considerable influence is exercised over the qualities of purulent secretion by the processes of digestion and assimilation, and even by mental agitation,—so that digestion and mental influences would be causes of purulent secretion!

In conclusion, I should be disposed to say of the connexion of inflammation and suppuration what Guersert has said of inflammation as connected with tubercle: "Des que deux états peuvent se rencontrer isolément et independamment l'un de l'autre, rien ne s'oppose à ce qu'ils puissent se trouver réunissans que l'un soit precisément ni la cause ni l'effet de l'autre."

* Mr. Abernethy justly censures a prevailing error among medical men; namely, that we have "no knowledge of cause and effect, save what results from the continued observation of the priority of the one, and the consequence of the other." He shews, by several apt illustrations, that we do, by inquiry, attain "a rational assurance of the nature of cause and effect." "If," says he, "I am able, in the absence of the object, to represent upon paper a spherical or many-sided figure, do I not manifest a knowledge of the causes of light and shade beyond that which my perceptions alone would have produced? If, also, I can at will present the angle of a prism to a luminous body, so as to produce the regular exhibition of the rainbow colours, do I not exhibit a knowledge of the causes of such effects?" Now, if we could, by producing inflammation, secure the appearance of suppuration, we should, indeed, exhibit a knowledge of the cause of this last phenomenon: but, as we cannot succeed in doing this, the cause of the suppurative process is yet, I think, to seek. To admit inflammation as its efficient, is to admit "that we have no knowledge of cause and effect beyond that which results from mere observation of the precedency of the one and succession of the other," which is a libel on the human understanding, a severe satire on ourselves, and a complete prohibition to all rational inquiry.

ON VALVULAR DISEASES OF THE HEART.

To the Editor of the London Medical Gazette.

SIR,

HAVING observed in the Medico-Chirurgical Review of July last, p. 220, a case abridged by the Editor from one published by Dr. Nagle, which both of these gentlemen believe subversive of Dr. Corrigan's theory of the heart, I will, with your permission, lay before your readers the case itself, together with such remarks as may be necessary.

CASE.—"Cath. Langan, aged 32, and married fourteen years, was affected with cough, dyspnœa, and palpitation of the heart, and occasionally with copious hæmoptysis. The symptoms commenced before her marriage, and recurred frequently, with more or less severity, until the period of her coming under my observation. At this time, three months previous to her death, the pain in the region of her heart was extremely acute, the palpitation very violent, and the other symptoms much aggravated. Percussion elicited a dull sound over a large extent in the region of the heart, the actions of which could be heard all over the anterior part of the chest, from the clavicles to the margins of the false ribs, especially on the left side. They were so rapid, tumultuous, and disorderly, as to preclude the possibility of any analysis, until the remedies usual for the alleviation of such symptoms were employed. *The heart's impulse was remarkably strong on the right and left side of the sternum.* The pulse, however, of the radial and other arteries, was scarcely perceptible. No regular rhythm was at any time discoverable in the heart's movements. A number—six or eight, perhaps—of comparative weak and confused impulses, followed each other in rapid succession; after which came three—*strong*, rather regular, and admitting of an appreciable interval between them.

The hand felt not then an alternation of soft and rigid impulses, as it did immediately before. The arterial pulse became evidently more regular, and a little increased in strength these few beats, *but was by no means in proportion to the intensity of the impulse against the chest*, with which it was unquestionably synchronous, and invariably exhibited the regularities and irregularities of the latter phenomenon. It was hard, wiry, and rapid. When the heart relieved itself, as it were, by two or three comparatively slow and tranquil motions, then, and only then, a loud bellows sound was audible between the ensiform cartilage and the margins of the eighth and ninth false ribs of the left side. This soufflet did not extend to the right side, but was traceable on the left, to the extent of a few inches laterally from the sternum and up to about the third rib, *approaching which it exhibited an increased intensity*. Repeated observations left no doubt of its being perfectly synchronous with the impulse against the chest, and the pulsations in the different arteries examined; and what is important to observe, that it was *substituted* for the first of the two cardiac sounds. The contraction of the ventricles was attended by a loud dull sound on the right, with the bellows-sound on the left of the sternum."

Diagnosis.—"Hypertrophy, with dilatation of both ventricles, extending in all probability to the auricles. Certainly an obstruction at the aortic orifice, occasioned either by disease of the valves or contraction of the aperture." . . . . . "Being asked by Dr. Clinton, who frequently visited with me this patient, if I considered the auriculo-ventricular orifices obstructed? I replied, there was no phenomenon to warrant even the suspicion of their being so; and that the result of a *post-mortem* examination would strongly confirm the reasonableness of my objections to the new doctrine respecting the heart's impulse against the chest."

"*Autopsy.*—The heart was enormously enlarged, the auricles and ventricles being exceedingly dilated and hypertrophied, but the hypertrophy did not appear proportioned to the dilatation. The hypertrophy and dilatation of both auricles were in proportion to those of the ventricles. The increased thickness seemed to extend to the valves, which closed perfectly their respective apertures. All the orifices, except the aortic one, were not only quite free, but appeared to participate in the dilatation of the cavities. The aortic aperture very much contracted, and its valves considerably more thickened than any of the others, which, indeed, were thought by some to be in their natural state, but adapted to their respective openings."

"Dr. Nagle," continues the Editor, "properly observes, that had he formed his diagnosis according to the principles of Dr. Corrigan's theory, he must decidedly have fallen into error. *He would have attributed the bellows-sound to an obstruction at the auriculo-ventricular opening, as it occurred synchronously with the impulse against the chest*; but the dissection proved that the obstruction existed in the aortic valves. The case is strongly subversive of Dr. Corrigan's theory."

Now, sir, I am prepared to shew, not only that the principles of Dr. Corrigan's theory would not have led into this error, but that they would, in some points, have enabled us to deliver the true diagnosis with greater certainty and confidence than was actually done.

In the first place, the author has endeavoured to prove too much, when he states that the bellows-sound was synchronous both with the pulse and the impulse at the chest. This cannot be strictly true, for we have the authority of Dr. Elliotson and many others for believing, as our theory maintains, that the pulse occurs *after* the impulse at the chest. If, then, the bellows-sound was synchronous with the pulse, it could not have been so with the impulse. We, therefore, in denying that the bellows-sound in this case was synchronous with the impulse at the chest, would not ascribe it to the diastole of the ventricle; and therefore we should not have ascribed it to an obstructed auriculo-ventricular opening.

It is true that the soufflet in question was a first sound, but I have in a former paper shewn that a soufflet or bellows-sound of the heart differs in its times from the healthy one (Med. Gaz. No. 190. p. 8.); that it arises from different causes, and that the same rules do not apply to both. That a soufflet happens DURING the rush of the vital fluid through a narrowed passage; that the natural sounds occur at the *determina-*

tion of the rush;—consequently, that a narrowing of the auriculo-ventricular passage would cause a bruissement, which would happen *before* the first natural sound and impulse, and which in short would, from its rhythmical order, be called a second sound. In the same manner, a bellows-sound occurring during the ventricular systole, would, from its *immediately* following the impulse (as in the foregoing case), be called a first sound. It is entirely from want of making this necessary distinction between the healthy and morbid sounds of the heart, that the old theory has stood its ground so long. The following rule will enable the reader to apply practically what I have said—namely, that a bellows-sound, when the first sound belongs to the systole of the ventricle, and when a second sound to its diastole; thus far agreeing with the old theory.

I will now shew that the new theory would have enabled the observer to have prognosticated most certainly the non-existence of any auriculo-auricular contraction, which, after all, was the only point of doubt in the above case; for it appears that, in reply to Dr. Clinton, Dr. Nagle only ventured to say, "that there was no phenomenon to warrant even the suspicion, &c.;" but, according to the principles of the new theory, we should most decidedly have said that there *could be no such contraction*, because it is stated "that the heart's impulse was remarkably strong on the right and left of the sternum."

To those who have attentively considered our theory, one of its first and *peculiar* principles will be brought to mind—namely, *that the impulse against the chest is occasioned by the rush of blood from the auricles into the ventricles.* Now, whatever would impede that rush of blood would lessen the impulse; therefore a strong impulse, as in Dr. Nagle's case, is quite incompatible with an obstructed auriculo-ventricular opening.

Again, from the same consideration of *increased impulse*, the diagnosis of hypertrophy and dilatation of the auricles would have been confidently given; whereas Dr. Nagle had given it only as a "probability." How could he have done otherwise, without adopting our *peculiar* principle—that the contraction of the auricle is one cause of the aforementioned rush of blood and impulse at the chest?

I take no notice of the rest of the diagnosis, which agreed well with the post-mortem appearances, as they have little to do with either theory. The author has shewn considerable skill in giving it so correctly without the aid which our principles would have given him.

What, then, becomes of the assertion that Dr. Corrigan "would have attributed the bellows-sound to an obstruction of the auriculo-ventricular opening, &c?"

In support of the general rule peculiar to our theory, that a natural or increased impulse is incompatible with structural narrowing of the auriculo-ventricular passages, I will just refer to the following cases, which are the first that come to hand. Dr. Hewit's case of contraction of aortic orifice, (Med. Gaz. vol. vii. p. 326), with "*impulse increased;*" also Dr. Hope, case of Mary Andrews, same disease, where the "*impulse was increased in force and extent,*" (Med. Gaz. idem.) Again, in Dr. Elliotson's case of rupture of one of the aortic valves, (Med. Gaz. March 19, 1831), there was "*a strong impulse of the heart on the left side.* Also Delta's case of the same disease, (Med. Chir. Rev. April 1, 1831, p. 521), with "*impulse of left ventricle very strong.*" See also Dr. Hodgkin's very interesting cases of retroversion of aortic valves, (Med. Gaz. No. 66, p. 3.) In case No. 1, no symptoms are noted; in No. 2, "*the impulse communicated to the ear was reported to be remarkably strong.*" In the 3d case, "the action of the heart was irregular, *the impulse tumultuous and strong;*" and in the last case, "*the impulse of the heart was strong.*" Now in all these cases there was found no narrowing of the auriculo-ventricular passage; and in every case but the last there was a bellows-sound. Our theory, *and our theory alone*, could have determined that the auriculo-ventricular passages were free, and that therefore, knowing the seat of the bellows-sound, the disease was in the aortic aperture.

I could have cited many more such cases, but these are sufficient to shew the importance of attending to the impulse in the diagnosis of valvular diseases of the heart. I have formerly cited cases of *narrowed auriculo-ventricular passages*, to which I need not again

refer, in which *the impulse was diminished or lost*; and I have in vain hunted for cases of natural or increased impulse with such a diminished passage.

Since writing the above remarks, I have observed another very interesting case of Dr. Elliotson's, of rupture of one of the aortic valves, in which there was double bellows-sound, with "*a strong impulse of the heart.*" The diagnosis which was given was completely verified, but how was it that the principal feature in the case, namely, the rupture of one of the aortic valves, did not enter into the diagnosis? How was it that the old theory, even in the hands of Dr. Elliotson, could not determine the existence of this rupture? How was it that Dr. Hodgkin did only suspect the retroversion in one of the four cases cited? How can we account for it that neither Corvisart, Laennec, Bertin, Rostan, Andral, nor any one else, has told us how we may distinguish rupture or retroversion of one of the aortic valves? The following simple rule, derived from our theory, will enable any tyro to distinguish it without difficulty, namely, *a natural or strong impulse* will shew that there is no contraction of the auriculo-ventricular valves; and a bellows-sound occurring between the second sound of the heart and the impulse, or after the pulse is completely over, *being a second sound*, will indicate that the bellows-sound occurs during the diastole of the ventricle, and therefore must arise from regurgitation from the aorta*, (or pulmonary artery.) Dr. Hodgkin calls the brouissement in his cases of *retroversion of aortic valves*, a *bruit de scie*, and the bruissement of cases of *laceration* is usually described as a *bellows-sound*. Note also, that if *two* of the aortic valves are retroverted, *no* morbid bruissement is heard in the heart. Thus, in one of Dr. Hodgkin's cases, a "*bruit de scie*" was *sometimes* heard: the same absence of bellows-sound obtains also when *two* of the aortic valves are considerably lacerated.

But in Dr. Elliotson's case just referred to, there were *two bellows-sounds*; then, as "*the strong impulse at the chest*" proved there to be no narrowing of the auriculo-ventricular passage; *both* of the bellows-sounds must belong to the aortic aperture; one from a narrowing of the aperture, the other from a defect permitting regurgitation. Accordingly, dissection shewed the valves to be *much* thickened, and *one* of them lacerated.

I would propose the following rules for the diagnosis of valvular affections of the heart, wishing them to be considered both as touch-stones for our theory, and as easy guides for the pathologist. They are all, with the exception of No. 3, taken from the cases I have cited at different times. The symptoms of No. 3 are somewhat hypothetical, arising from the incompleteness of their description given by Corvisart, Bertin, and Laennec. These rules, however, will not exclude the aid derivable from the cylinder concerning the apparent seat of the sounds, &c. &c.

No. 1. Bellows (first) sound of the left side, beginning from the impulse ending abruptly in the pulse, *if with a natural or increased impulse*. Contraction of aortic aperture, no obstruction in auriculo-ventricular opening.

No. 2. Bellows (first) sound, beginning with *the* pulse, and ending with the second natural sound of the heart, *with diminished impulse*. Narrowing of the auriculo-ventricular passage, its valve permitting regurgitation.

No. 3. The same symptoms, with the exception that the *impulse is strong*. Imperfect closure of auriculo-ventricular valve from rupture of carnæ calumnæ or tendons; perforation of one of the valves; polypi, or other causes permitting regurgitation into the auricle; auriculo-ventricular opening free.

No. 4. Bellows (second) sound of the left side, beginning from the second natural sound (*i. e.* of the healthy side of the heart) prolonged, *with natural or increased impulse*. Retroversion, or laceration of *one* of the aortic valves, permitting regurgitation.

No. 5. Symptoms of No. 4, excepting that the bruissement is a "*cooing sound.*" Of this case I have only met with one example, referred to in the last note. On dissection there were found polypi adhering to the mitral valves. Auriculo-ventricular passage free.

No. 6. Symptoms the same as No. 4,

* The only exception I have met with to this rule, is in Dr. Elliotson's curious case of obstructed mitral valve from polypi, which I have cited, (Med Gaz. July 23, 1831.) In this case the brouissement was a "*cooing sound:*" *impulse very great.*"

with the exception that there is diminished impulse. Contraction of auriculo-ventricular opening.

Synopsis of some of principal arguments in favour of Dr. Corrigan's theory:—

1. The sounds and impulse are not occasioned by the striking of the apex of the heart against the ribs, because we have shewn by experiment, that when the living heart is placed out of contact with the ribs, the sounds and impulse are still produced.

2. The first sound is not produced by the contraction of the ventricles, because it happens considerably before the pulse, and because we have no instance of an *incipient* motion of any kind producing sound.

3. The second sound is not produced by the action of the auricles, because we have given an instance (a case of Dr. Elliotson's) in which the second sound was increased on one side of the heart, while the corresponding auricle "was bound down and lost," in consequence of adhesive inflammation, and therefore incapable of action; and because the observations of Harvey, Haller, and Turner, shew that the contraction of the auricle happens *immediately* before that of the ventricle.

4. Neither is the second sound produced by the action of the auricular sinuses, because the experiments of Dr. Barry and Laennec prove that the motion of the sinuses accords chiefly with the acts of respiration—that they are, in fact, passive reservoirs.

5. The impulse and sounds of the heart are occasioned by the motions of the fluid within the heart, because, as we have shewn, an exsanguineous heart, although acting with more than ordinary vigour, gives no impulse or sound (which fact also proves that the muscular action of the heart *merely* does not produce the sounds); and because we have shewn by experiments, that a fluid propelled from one cavity into another *always* produces a sound and impulse the same as those of the heart.

6. The first sound and impulse are occasioned by the rush of blood from the auricle into the ventricle, because as, according to 5, this rush will produce sound and impulse (which fact is also proved by Dr. Hope, who calls this impulse the "back stroke"), so the rush of blood being from the base of the heart to its apex, the shock of the moving fluid *must* be towards the ribs, from within outward; and the impulse of the chest is evidently from within outward; and also because we have shewn from several cases, that a narrowed auriculo-ventricular passage impairs the first sound and impulse, and that a natural or increased first sound and impulse are *always* attended with a free auriculo-ventricular opening.

7. The auricles are, at least, a concurrent cause of the rush of blood into the ventricle, and therefore of the impulse and first sound, because in numerous cases of increased impulse, the auricles have been found hypertrophied and enlarged, and because (3) it appears that the auricles contract immediately before the ventricles.

8. The second sound is produced at the termination of the ventricular systole, probably by the sudden arrest of the blood at that time, because we have shewn that such an arrest of a moving fluid always produces such a sound; or it may, according to Dr. Corrigan, arise from the coming into contact, though partially, of the internal parietes of the ventricles. One of these causes may be assigned, because (3 and 4) the other alleged causes, are inadmissible.

I remain, sir,
Your obedient servant,
W. T. Haycraft.

Greenwich, March 12, 1832.

CONTAGIOUS NATURE OF CHOLERA IN LONDON.

To the Editor of the London Medical Gazette.

Sir,

In the number of your journal for the 7th instant, in your comments on the propagation of the cholera, there appears the following passage:—"Whether cholera be contagious or not, is a point which may admit of reasonable doubt. For ourselves, we remain of our former opinion—that its history on the great scale would lead us to answer in the affirmative; but we confess, that in London the evidence of contagion has often been wanting. Probably the disease may exhibit the contagious principle more in some places than in others." Now, sir, as I do not altogether coincide

with you in this opinion, you will perhaps permit me to state the grounds upon which I continue to believe that cholera in London has been in every respect as contagious as cholera in any other part of the globe.

I do not purpose, on the present occasion, entering upon any minute disquisition on the subject of contagion, either generally or as regarded in connexion with the present disease. My object is solely to relate a few out of many facts that have fallen within my own not very extensive observation—facts from which, if I am to reason on the established principles of induction, I am able to deduce no other inference than that which I have stated.

Fact I.—The first case of cholera in Southwark, reported by the Central Board, took place on Sunday, 12th February, in the person of Mrs. Roberts, residing in Bear Gardens, Bankside, who died after an illness of about twelve hours. It has been stated publicly, and this has been frequently repeated, that Mrs. Roberts had been exposed to no contagion. It is, however, a singular fact, that her husband had been at work at Deptford Creek since the Thursday previous, going to his work early in the morning, and returning home every evening to sleep.

On reference to the Official Gazette, I find that the "Bradford" was at that lying in Deptford Creek, with cholera on board. It is right to notice that, on the 9th, three days before Mrs. Roberts' illness, there had died a man, (Florence Sullivan,) in the Mint-Street, whose case was suspected to be cholera; but as this was not recognized by the Board of Health, and as I have nothing to do with disputed cases, I have not taken the trouble to inquire into his history, the rather as his nearest surviving relatives have made up their minds against cholera, and will not submit to examination.

A similarly disputed case too occurred on the 7th, on board the "Felicity," at Hermitage Tier; and a case at Rotherhithe on the 9th, in the person of John James, concerning the real cause of whose disease there was, I believe, no reasonable doubt. This man had been working at a ship from Newcastle.

At Lambeth Butts, the first case was that of a man, Clark, on the 13th February. This individual had, for two or three days before, been down at Limehouse searching for work. I do not say that, because he had been at Limehouse, he had therefore necessarily been exposed to contagion; but at the same time I cannot help regarding it as rather *a curious coincidence*, that so many of the earliest cases should have had communication with places down the river, in localities then the seat of cholera. Clark's wife, however, was the second case at Lambeth; and from this spot the disease spread around. Of these last cases I have no personal knowledge.

Fact II.—Frank Burn, æt. 13, living in Winchester-Street, St. Saviour's, was attacked pretty severely on February 13, but eventually got well. Here, too, it has been stated, that nothing like contagion was traceable. This boy's father had for some days before been at work in the West India Docks, returning home to the borough nightly through Limehouse, and occasionally stopping on the way at public houses, &c. This man had diarrhœa at the time his son was taken ill; and two other children, in frequent attendance on their brother, became afterwards the subjects of severe bowel complaint. I do not assert that the elder Burn exposed himself to contagion at Limehouse, or that the looseness which he was just at that time seized with was the cholera; but I do say, notwithstanding, that here again is another odd coincidence of events.

Fact III.—Next in order come the cases that occurred in an Irish colony at the back of Dean-Street, Tooley-Street. The inhabitants of this spot are numerous, and most of them in a very wretched condition; many of them females, accustomed to live in gregarious intercourse with the seamen, &c. along the adjacent bank of the river. Under these circumstances there is, of course, great difficulty in tracing the source of the first case in this locality; and I learn from good authority, that several persons have been attacked and have died here, without medical assistance at all. The history of those, however, that are known, is sufficiently instructive. They are well given by Mr. Millard, in the Cholera Gazette, No. 4, p. 150-151; and I am happy that the result of my inquiries differs in no material respect from the statement there made by him, and to which I refer for particulars.

Case 1. *The first case* here was that of Margaret Dunn, a woman who had been out hawking tapes, &c. nobody knew where; she had been absent two or three days, and returned home on the afternoon of the ninth February, complaining of cramps, followed by the usual symptoms, of which she died on the 12th; her case was not known till after death, as no assistance had been sought for her.

Case 2. John Sullivan, living nearly opposite, attacked 13th; died 14th February; buried 19th, Sunday. Amongst the next cases were—

Case 3. Margaret Toomy, attacked on the 14th, in the same room in which Case 1 died, recovered.

Case 4. Johanna Connell, attacked on the 14th, in the same room as Case 3, recovered.

Case 5. Jerry Connell, æt. two years, child of Case 4, attacked 15th February, recovered.

Fact IV.—*Case* 1. Mrs. Fitzgerald, living in 3, Bird-cage Alley, High-Street, Borough, was seized with cholera on the afternoon of Sunday, Feb. 19th, and died in the course of the ensuing night. She was taken ill whilst she was out on her way to attend the funeral of one Sullivan, who had died at the back of Tooley-Street a few days before, and whom she had visited during his illness. (See Case 2, above.)

Case 2. Mrs. Doyle, who nursed Mrs. Fitzgerald, and was a robust fearless woman, sickened over the body of the latter, became worse, and died on the Thursday following, February 24th, in the same house. These two cases were attended by Dr. Whiting.

The day after Mrs. Doyle's death a family living in that house, taking alarm at the event of these two deaths, removed themselves and their goods to the house of a man named Daniel Corcoran, 9, Royal Tent Court, Kent-Street, in Newington parish.

Case 3. Corcoran was taken ill on Sunday afternoon following, with the most aggravated symptoms of cholera, and died that night, towards morning. In addition to this, the widow of a man who had died elsewhere of cholera on the preceding Tuesday, had since then taken refuge at Corcoran's. This was the first case in Royal Tent Court.

Case 4. On Saturday, 3d March, Owen Maly was admitted into Guy's Hospital for an illness which, after admission, was pronounced to be cholera. He was thereupon removed to the cholera ward, where he died on the night of the 4th. This man, *after close inquiry*, was found to have resided in Royal Tent Court, Kent-Street, and to have been acquainted with and visited Corcoran (Case 3). He had a few days premonitory diarrhœa.

In illustration of the impediment that so frequently exists to the complete elucidation of the circumstances of a case, I would refer to the official report of Dr. O'Shaughnessy, the highly-talented and industrious inspector of the Camberwell and Newington districts. In No. 4, of the Cholera Gazette, p. 157, there is the brief notice of one Daniel Coghlan, living in 9, Royal Tent Court, Kent-Street, who was seized with cholera on the 26th February, and died that night; the case having been seen by Mr. Callaway and several other medical gentlemen. The report concludes in these words:—"No communication could be traced with persons or things calculated to propagate this disease." At the time that we were in attendance upon the case, as well as on the following morning, we remained satisfied that such indeed was the fact, and that here was a case in which "the evidence of contagion was entirely wanting." But on the following day, being at Guy's Hospital, I recognized amongst the workmen there an individual who had been present in Coghlan or Corcoran's room during his illness, a relative of his, who knew him well. From this man, with some difficulty, I learnt the foregoing truths, which were afterwards literally verified by the inquiries of my friend, Mr. Charles Gaselee, made in a totally different quarter.

Fact V.—*Case*. This occurs in the person of Edward Bryan, a fine child, æt. four, who was just recovering from measles, living at 7, Ewer-Street, near Rowland Hill's chapel. He was attacked on Friday morning, March 2d, and died after an illness of about sixteen hours, under the care of Mr. Odling, the parish surgeon. We had completed our investigation into the history of this case, without having traced contagion. Shortly before the child's decease, however, an old woman in the room, a relative, living there and being much with the child, inadvertently associated the sufferings of the little boy with those of other cases that she had

lately been witnessing; and by a little cautious inquiry the following was elicited. This old woman had, on the preceding Sunday, been present at the death of Mrs. Sloane, whose husband had similarly died on the Tuesday before. She had been with them during their illness, and had since their death returned home. Sloane was a stout hearty man, and was earning 3l. per week; he lived at White's Ground, Bermondsey. Of course here is not *positive* evidence that this old lady brought the cholera up from Bermondsey with her, and gave it to the child; but it is worthy of notice, that this was the first case of the disease in Ewer-Street, and, I believe, the first in that immediate neighbourhood.

Fact VI.—This comprises a series of cases, so important in their bearing upon the propagation of cholera, that I shall need no apology for narrating them with greater minuteness than I have deemed it necessary to do in other instances. They are as follows:—

Case 1. Julia Gollagher, æt. about forty, a basket-woman, living in Peter-Street, Sun-Street, Bishopsgate, was seized with symptoms of cholera on the afternoon of Thursday, April 5th, whilst selling fruit in the street, and was compelled to betake herself home.

She had a brother who resided at 4, Half-moon-Street, near Bishopsgate church. When he heard of his sister's illness, he sent his wife to see her, who, finding her very ill, and in want of common comforts and attendance, had compassion upon her, and removed her to their own room in Half-moon-Street. Gollagher, with his wife and four children, lived in this room. It was, comparatively speaking, large, and was kept in very neat and clean order, and hitherto there had been no cholera in the vicinity.

Having brought Julia Gollagher here, Mrs. G. made up her little boy's bed for her in a corner of the room, and placed her in it; here she lay a short time, vomiting and purging in the sheets, till finding the bed was too small for her, and that she was not at ease in it, she placed her in her own large bed; and it being now night, her little boy immediately took possession of his own bed, on which were the very sheets and blankets from which the sick woman had risen.

In her new bed Julia G. lay till a very late hour of the night, her brother and his wife being in constant attendance upon her, pursuing the directions of Mr. Porter, of Bishopsgate-Street, who had been called to her. At a late hour of the night a temporary bed was made up for her before the fire, and her brother went to sleep in the sheets and on the bed whence she had just been moved, his wife remaining up to attend her.

On the following morning, Friday 6th April, Julia Gollagher was conveyed to the local hospital, under the management of the city board, in Skinner-Street, Bishopsgate, where she is now recovering; and her brother's family that night returned to their wonted habits, and again slept in the sheets which she had lately occupied.

Case 2. Saturday April 7th.—John Gollagher, æt. eleven, a fine healthy boy, in whose bed his aunt, Case 1, had first been placed, was this evening about half-past ten o'clock brought to the hospital in Skinner-Street, with very severe symptoms of cholera, and notwithstanding the active exertions of Dr. Cobb and Mr. Porter, died at seven the following morning. He had been indisposed since he slept in that bed, but his more severe symptoms had only existed thirteen hours before death.

Case 3. Saturday April 7th.—Daniel Gollagher, æt. forty-four, father of the above, was admitted into the City Hospital, in Abchurch Lane, at eleven P.M., where he died at eight the following morning. He had dined at home on beef-steak and potatoes, and had returned to his work after dinner in good health. He had had no dejection on that day; but between three and four P.M. he became relaxed; and when he came home about nine o'clock he had had but five or six stools, without pain, cramps and vomiting commencing only at this time. He worked for a master carpenter in Bishopsgate-Street, and in proof of the good character he bore, it will suffice to state, that his master employed him, moreover, in the house, to clean boots, knives, &c.; and his habits were so sober and steady, that, in the quaint but characteristic language of his widow, "no one would have suspected that he was an Irishman."

Case 4. Sunday April 8th. — Gollagher, æt. eight, daughter to Case 3, taken ill with symptoms of cholera. Under the judicious management of Mr. Porter, this child is now convalescent. The room in which all these things

occurred was on this day closed by the proper authorities; but by some oversight the widow was permitted to take with her to her new lodgings (in the same house, by the way) the bed furniture that had already been used by the former individuals; they slept in them that night and the following, and,

Case 5. At eight A.M., Tuesday April 9th, Betsy Gollagher, the eldest girl, æt. thirteen, was brought to the hospital in Abchurch Lane, where she died at half-past six the same evening. This little girl had been severely attacked since three in the morning; she had been indisposed for the last two or three days, and I had prescribed some medicine, which I afterwards found had not been administered.

Case 6. Mrs. Gollagher applied to me on April 9th, in consequence of severe purging that she was then enduring—so severe that it passed from her in the street. Warned by the fate of Case 5, she took what was prescribed, and is now well.

After this, of course the sheets and blankets in question have been destroyed, and there has been no more illness in that spot. It is worthy of note, that the room in which these distressing events occurred is situated on the second floor; so that if the cholera be a terrestrial miasm, it is odd that the people below stairs should have escaped; if it be a descent from the heavens above, it is singular that the dwellers upstairs should not have been attacked; if it be in a stratum of air wafted by the east winds, it is curious that, in its course, it did not visit any of the houses around that look upon this.

It must not be omitted to be stated, that Gollagher had not himself been to his sister's in Peter-Street; and that the only members of his family who did go there were, his wife and the infant at her breast, which even became more irritable as to its bowels than customary.

I have been trespassing on your space, and the patience of your readers, at greater length than I had originally contemplated; I shall not, therefore, take up any more of your time, although I had intended to have added some remarks on the question of the immunity or non-immunity of medical men and attendants on the sick. The evidence that I have at present to offer is before you; and I trust it may appear that it is of such a nature as (connected with the facts already before the profession), if not to prove, at least to tend to the almost positive probability, that the cholera which we have lately been visited by is essentially a contagious disease.

It is not to be denied, that in very many instances no history of contagion has been found; but this is no proof that such history has not existed. The feeling that has prevailed among the lower orders, so hostile to acknowledgment of the cholera, and to their medical attendants generally, as well as the disinclination that has notoriously been manifested on the part of medical men themselves to publish accounts of their cases, lest they might alarm or disgust the public mind, afford, I should suggest, sufficient and plausible explanation of the fact; and I hope it may not be deemed unreasonable to believe (since I have adduced cases in illustration), that in many of those instances in which it has been denied, there might be found, upon stricter investigation, clear and satisfactory testimony in favour of contagion.

I am, sir,
Your obedient servant,
ALEX. TWEEDIE.

3, Abchurch Lane,
April 13, 1832.

P.S.—FACT VII.—I may state, in addition to the facts already mentioned, that this very evening we have admitted into the hospital in Abchurch Lane, Louisa Cousins, æt. fifty-one, the nurse of the cholera hospital in Skinner-Street, Bishopsgate, where she had only been employed one week, attending to but two patients during that time.

Case 2. Saturday April 14th.—Ann Forecast, æt. forty-three, also a nurse at the cholera hospital in Skinner-Street, under the same circumstances as Case 1, has this day been admitted, with rather a severe seizure of the disease, into the hospital in Abchurch Lane.

Case 3. Jane Stanley, æt. fifty, admitted into the Abchurch Lane Hospital on the afternoon of April 19th. She resides in the same house as the Gollaghers (Fact VI.), and, since the death of the elder Gollagher, she had most compassionately given shelter in her own room to his widow and surviving children. She had previously been in good health, and was a fat corpulent

woman; she had the symptoms of cholera very severely, and died on the evening following that of her admission, a martyr to the doctrine of *non contagion*.

April 25, 1832.

EFFECTS OF

ARSENIC UPON POULTRY, WITH THE MORBID APPEARANCES IN THE VISCERA,

And on the relative Value of the various Modes of Analysis devised and investigated by Dr. Christison.

By Robert Venables, M.B. Oxford, &c.

The subject which has given rise to the present paper will, perhaps, by many be looked upon as trifling and devoid of interest. The poisoning of chickens may possibly be regarded as an incident of no importance in either a medico-legal or criminal point of view. However, the wilful and malicious administration of poison to domesticated animals is a crime, not only of great moral responsibility, but also one recognized and punishable by the laws of this country. Therefore the judicious and successful investigation of such cases becomes matter of moment, inasmuch as it may, when properly applied, conduce to the conviction of guilt, and to the acquittal of innocence.

The materials for the present paper had been arranged for some time, and were about to be sent for publication, when I learned that Dr. Christison had favoured the profession with a new and improved edition of his invaluable work on Poisons. I also heard that this gentleman had done me the honour to notice my contributions to this department in the Medical Gazette, and therefore I determined to defer the publication of these remarks till an opportunity occurred of consulting the new edition of Dr. Christison's book. This opportunity having at length presented itself, I shall proceed to detail the facts, and comment upon the principal modes of analysis in due order.

An individual who kept half a dozen fowls observed one of them to look rather dull and "mopy," as it is termed, about two o'clock one afternoon. This excited no particular attention at the time; but on the following morning, four of the six fowls were found dead and stiff. Where they had roosted was discovered to be unusually soiled by the excrements, owing to the violence and severity of the purging. On being consulted, I hinted the probability of poisoning, but the difficulty which presented in regard to such a conclusion was, that two should have wholly escaped, while the other four should have suffered. The two which escaped did not appear to suffer from illness of any description or degree, and therefore it was difficult to conceive how four out of six poultry which were almost always together should have taken, and the other two have avoided the poison which was laid for them. I therefore decided on an anatomical and medico-legal examination. The following details will present the reader with the results of this inquiry.

On opening the abdomen, the intestines presented patches of morbid vascularity of greater or less extent. The colour of these patches varied much in different situations, without, however, observing any regular order. The colour in some partook of the scarlet or crimson, while in others it was of a deep purple. The intestines, too, were in many places stiff, rigid, and hardened in their structure. The crop was full of barley and other food, and in some of them perforated. The gizzards looked unnaturally red and vascular throughout their substance, the colour varying from a darkish crimson to purple. But the most extraordinary appearance was a kind of gelatinous bag in the gizzard. In some the contents were perfectly fluid, of a yellow colour, faintly tinged with green. In others it was a complete gelatinous mass, of the same colour and general appearance as the fluid, with the exception of consistence. This appearance was observed in the gizzards of every one of the four fowls. I consulted a number of persons in the habit of drawing fowls, and consequently experienced in the usual appearances presented by the healthy gizzard, and they all declared it an appearance quite unnatural and perfectly new to them.

The lungs presented a dark grumous looking appearance, and were evidently turgid and congested in their paren-

chyma, while the vascularity of the pleural membrane, both costal and pulmonary, presented more of the inflammatory character.

The brain more vascular than I believe is usual; but I can hardly speak positively to this fact, not being intimately conversant with the healthy degree of vascularity in this organ. The trachea was unnaturally vascular, and the œsophagus, both externally and internally, presented patches similar in character to those observed in the intestines. The pleura pulmonalis, as already observed, was of a bright red or crimson colour, and the appearance of genuine inflammation much more strongly marked than in any other part, not excepting even the tissues to which the poison was directly applied. The anus much excoriated, as in cases of acrid discharges by purging.

Here, then, were strong presumptive indications of the ingestion of a poison of the *irritant* class. Now there could be very little doubt that the poison, whatever its nature, had been mixed up with the food and left for the fowls, and that they took it spontaneously—that is, there was no reason to believe that it had been violently forced into the stomach. This being admitted, or assumed, reduced the medico-legal inquiry within, comparatively speaking, very narrow limits. The analysis was thus reduced to that for arsenic, for it is the only poison of the irritant class, the injection of which was reconcileable with all the circumstances. This presumption was both feasible and consistent, because the other individuals of the above class, from either their nauseous or metallic taste, or from some other obviously sensible and disagreeable properties, precluded the presumption of a voluntary introduction. Hence, then, my chemical researches were confined to the methods of evolving arsenic: for the attainment of this object I proceeded as follows:—

The whole of the viscera of all four fowls being removed, were put, with their contents, into a basin, and were then cut into small pieces and shreds; the gizzards were also cut into small pieces or lumps, and scored, and the whole put into an untinned iron or "metal" saucepan. The basin was then well washed with distilled water, and the washings thrown upon the divided viscera in the saucepan. A sufficiency of distilled water being added to conduct the boiling, the mixture was rendered feebly alkaline by the addition of liquor potassæ *. The whole was now well boiled, and distilled water added, as the fluid or solution became too concentrated from evaporation and supersaturation with animal matter. The boiling being finished, the fluid was first passed through a fine gauze, to separate the coarser particles. The coarsely filtered portion was thick, and of a muddy colour, wholly unfit in its then state for any analysis. The addition of acetic acid, however, in excess, threw down a considerable quantity of coagulated animal matter, which on subsiding left the supernatant fluid clear and transparent, but of a deep brown or amber colour. A portion was next passed through a fine filter, but it was impossible in this way to deprive it of its colour. A current of sulphuretted hydrogen gas precipitated sulphuret of arsenic, in tolerable quantity, of a yellow colour, with a slight tint of green, which, however, when dried, became of a very deep brown, thus proving a considerable contamination with animal matter.

Another portion was treated according to the process recommended by Dr. Christison in the first edition of his work, with hydrochloric acid, nitrate of silver, and muriate of soda, &c. to precipitate the animal matter; but although this was effected to a certain extent, yet the filtered fluid, after being thus treated, still retained a deep brown colour, and the resulting sulphuret, instead of its natural yellow, presented a deep-brown colour. Here, therefore, neither of these processes could wholly remove the animal matter†.

A portion of the sulphuret obtained by the first process—that is, after coagulation by means of acetic acid—was projected by small pieces at a time into nitre, in a state of fusion; and the deflagration being perfected, a solution was effected by means of hot distilled water. After treating the solution in

* I think the rendering the fluid feebly alkaline preferable to trusting to the feebler solvent powers of simple distilled water. Add to which, that, as Christison has shewn, arsenic is sometimes converted into sulphuret in the stomach; the sulphuret which would otherwise escape is dissolved.

† On reduction, the empyreuma from the first process was very embarrassing; and although greatly diminished by the second process, yet it was troublesome.

the way described in my former paper, and applying nitrate of silver, a brick-red precipitate fell down. The precipitate being well washed with distilled water, was dried by the side of a stove, and was then reduced in a tube by being heated with a mixture of freshly ignited charcoal and boracic acid. A perfectly pure and uncontaminated crust of metallic arsenic, possessing all the characteristic properties, in the most sensible degree, sublimed.

The sulphuret obtained by the second process, that is, after coagulating and precipitating the animal matter by means of nitrate of silver, being well washed, dried, and put into a tube with black flux, prepared on the moment for the occasion, was reduced, according to the directions of Dr. Christison. Although a perfectly characteristic crust sublimed, yet rather a dense cloud of smoke passed up the tube, owing, no doubt, to the charring of the animal matter which adhered in such obstinate and intimate intermixture with the sulphuret. I was now determined to decide unequivocally the relative value of these two processes as to the quantity of arsenic obtained.

For this purpose, the bulbs containing the fixed residues, after the sublimation, were separated from the sublimed portions by holding the necks in the flame of the spirit lamp, and drawing them off. The bulbs, with their contents, were first crushed in a steel mortar, and then reduced to an impalpable powder in one of agate*. They were now boiled separately in distinct portions of nitric acid, and warm distilled water added till every thing soluble was taken up. They were next filtered, and the solution from the arseniuret of potassium was treated with caustic potass till a feebly alkaline reaction was produced. Acetic acid being added in slight excess, and a current of sulphuretted hydrogen gas passed through the fluid, a yellow-looking precipitate subsided after boiling. The precipitate being washed, dried, and deflagrated with nitre, was reprecipitated from the solution in distilled water by means of nitrate of silver. A brick-red precipitate subsided, which, on reduction with charcoal and boracic acid, as above described, yielded a small, but very characteristic sublimate of metallic arsenic.

The nitrous solution of the silver residue was next rendered neutral by the addition of liquor potassæ, as indicated by the immersion of two small slips of litmus and turmeric paper. A drop or two of hydrochloric acid was next added, and then a solution of muriate of soda in excess, till the entire of the oxide of silver was removed. The solution being filtered, was now rendered feebly alkaline by caustic potass, and then slightly acidulated by means of acetic acid, and a current of sulphuretted hydrogen passed through for a considerable time. After boiling, and adding a small quantity of solution of muriate of ammonia, it was set aside, but no sulphuret of arsenic, or, indeed, precipitation of any kind, took place after an interval of even six or seven days. Therefore the conclusion is inevitable, that no sensible quantity of arsenic, either combined or uncombined, had resisted reduction, or remained in the flux*.

Dr. Christison objects to this process on the grounds of its complexity. After stating the advantage anticipated by the arseniate of silver yielding all its arsenic, he observes, "I question much, however, if this advantage is not more than counterbalanced by the great addition to the complexity of the process—a complexity so great as to render it extremely difficult for any one but an expert chemist to carry through the analysis with success." This is an objection which really should not be allowed to weigh for one moment. The evidence of none but an expert analyst should be allowed to weigh upon questions involving life or death in a court of justice. Too much simplicity is calculated to induce incompetent persons to undertake investigations for which neither practice nor experience has fitted them. The great objects should be delicacy and accuracy, for these, after

* The little button of silver, being malleable, was not pulverised, as it could not have been effected without a file, which, for obvious reasons, was objectionable.

* It may possibly be necessary here to explain the rationale of these manipulations. The nitric acid dissolves any arseniuret which may have resisted reduction. The precipitation of the arsenic is effected by means of sulphuretted hydrogen. But in the case of the arseniate of silver, after dissolving the silver, it was necessary to separate it, otherwise the sulphuretted hydrogen would throw down sulphuret of silver, which would obscure the result. Hence the necessity for adding muriate of soda, by which the silver is precipitated.—Ed. ii. p. 255.

all, constitute the perfection of medico-legal analysis.

Before attempting to throw down the arsenical sulphuret, it is advisable to free the solution as much as possible from animal matter, because, as Dr. Christison has very properly observed, animal matter exerts a solvent or suspending power over the sulphuret. In cases of the *arsenious* acid, Dr. Christison's method of separating the animal matter by means of nitrate of silver, answers remarkably well*. But in the case of arsenic acid, or any of its soluble compounds, this process would fail. Uncombined arsenious acid will not decompose nitrate of silver, but *arsenic* acid effects it even by single decomposition, and therefore the addition of nitrate of silver to any solution of arsenic acid will precipitate arseniate of silver. Therefore the best process, as applicable to all the preparations of arsenic, is to boil the suspected matters with a feebly alkaline solution, filter coarsely, boil with a sufficient quantity of nitric acid to destroy the organic matters, filter, render feebly alkaline by means of caustic potass, add acetic acid in slight excess, filter, and pass through a current of sulphuretted hydrogen. Separate the precipitated sulphuret, deflagrate with nitre, dissolve in distilled water, and filter: the addition of nitrate of silver will now precipitate the arseniate of that metal in the form of a brick red powder, which on reduction with charcoal and boracic acid, as I have already shewn, parts with all its arsenic†, or at least all that is appreciable.

Dr. Christison certainly simplifies his process in the last edition of his work. He merely coagulates some of the organic principles by means of acetic acid, and then evaporates the filtered fluid at a moderate heat to dryness. The arsenic is re-dissolved by boiling successive portions of distilled water upon the residue. But in applying this process to several portions of the arsenical solution which has been the object of the present detail, a great quantity of animal matter was taken up by the water. The process, however, will answer very well either for deflagrating the residue after evaporation, or after dissolving out the arsenic by boiling successive portions of distilled water on the evaporated mass, precipitating by sulphuretted hydrogen, and subsequently deflagrating the sulphuret, and precipitating by nitrate of silver, as already explained.

In conclusion, I am decidedly opposed to too great simplicity; not that simplicity is of itself objectionable, but it is an evil, from the encouragement it holds out to persons to undertake inquiries, which, as they advance, they find themselves unfitted satisfactorily to pursue. As they go on they find embarrassments multiply; it is now too late to retrace their steps; their evidence is vague and confused; and they have often cause for self-congratulation, if they come off without the sacrifice of a hard-earned reputation.

7, Suffolk Place, Hackney Road.

P. S.—It has been privately objected to me that the process here instituted for evolving the arsenic from arseniuret of silver, should any remain in the flux, is doubtful. It is suggested that the nitric acid may peroxidate the arsenic and form arsenic acid, and which uniting with the oxide of silver would form arseniate of silver. This objection, however, is more theoretical than experimental. Arsenic is not readily peroxidated by nitric acid, and in this I am borne out by the authority of Rose, who asserts that to peroxidate arsenic, nitric acid is not sufficient, but "aqua regia." However, to do away with every possibility of doubt, I proceeded as follows:—

The solution in nitric acid was treated with potass in excess, so as to precipitate oxide of silver. The silver was removed after every addition, so that there was left no oxide to be redissolved by any

* Dr. Christison states that "some persons, not deficient in chemical address, have occasionally failed in applying that method to complex animal fluids, from being unable to prevent the arsenic from falling down in the form of an arsenite of silver."—Ed. ii. p. 252. I believe that this accident can never happen if care be taken, and that the reagents be pure. For instance, if the liquor potassæ be carbonated at all, and that the carbonic acid be not expelled by boiling, arsenite of silver may precipitate. But if the chemicals be pure, and that sufficient time be allowed for the interchange of principles to be effected by the different reagencies, I believe there is no danger of throwing down arsenite of silver, at least such an accident has never occurred to me, and Dr. Christison makes the same avowal.

† Dr. Christison states that he has found the arseniate of silver very refractory. I admit that the arsenic acid is more fixed than the arsenious, and therefore requires to be urged with the blowpipe. But from arseniate of silver, when urged with the blowpipe, even without any flux, arsenious acid sublimes, as I have already explained in my former paper, in the London Medical Gazette, viii. 806. The ignited charcoal decomposes the arsenious acid as it rises, and the pure metal alone sublimes.

slight excess of the alkali. By this means the whole of the silver was removed, and the slight excess of alkali combined with the oxidated arsenic. To the solution when neutralized by acetic acid, muriate of soda in solution being added, proved the complete removal of the silver. A slight excess of acetic acid being now added, sulphuretted hydrogen was passed through the fluid, to precipitate arsenic, if present, but there was not the slightest deposition of arsenical sulphuret. I may still farther add, that no arsenic could be separated, even by bringing the silver button into a state of fusion in contact with pure charcoal.

MIDWIFERY—TWIN CASES.

To the Editor of the London Medical Gazette.

SIR,

I HAVE forwarded you the particulars of two cases, which if you think of sufficient importance, are entirely at your service for insertion in your valuable periodical publication.—I am, sir,

Your obedient servant,

THOMAS F. RANCE,

Surgeon and Accoucheur to the City of London Lying-in Hospital, &c.

4, City Road, Finsbury-Square, February 22, 1832.

In a late number of the Medical Gazette, an interesting case is related of twins where some difficulty of delivery was occasioned by the head of one child interrupting the passage of the other, but which was eventually overcome without any instrumental assistance. Although, in the course of my practice, I have met with a considerable number of twin cases, no circumstances of peculiar importance connected with them have occurred except in two instances.

The first case took place about twelve years since. Mrs. C——, aged about 40 years, residing at Hoxton, had been delivered (at the full period of utero-gestation) four successive times of children who died about three or four days previous to the commencement of labour. The mouth of the womb was invariably dilated to about the diameter of an inch, and then remained stationary until the membranes were ruptured, when the action returned, and went on rapidly till the fœtus was expelled. In her last labour (for she was never again pregnant) the same circumstance occurred as in former labours; the membranes were ruptured; and in the course of half an hour she was delivered of a large male fœtus, in a state of decomposition: immediately the membranes of another ovum protruded; and in about fifteen minutes she gave birth to a small living female child. The uterus gradually contracted, and the two placentæ were brought away, which were quite distinct, the one appearing putrid, the other fresh and healthy. In about a quarter of an hour after their expulsion, a profuse hæmorrhage came on, which produced deliquium. On examining the uterus, I found that it had become relaxed. I immediately bound a firm bandage round the abdomen; introduced some linen rags, moistened with cold vinegar, into the vagina, and administered the infus. Rosæ, with an additional quantity of sulphuric acid. The hæmorrhage was apparently suppressed, but returned in about half an hour; the recurrence took place about every thirty minutes for about four hours, each time to complete fainting, and the uterus in its action corresponded with the occurrence, and cessation of the flooding, by alternately relaxing and contracting. I remained with her about five hours, until permanent contraction was established, and the hæmorrhage had subsided. She had upon the whole a good recovery, and was able to suckle her child, although the secretion of milk was small in quantity. For some time she had a chlorotic appearance, but in the course of three months the health was so fully re-established as to enable her to attend to her usual avocations.

The other case, to which I wish more particularly to allude, is that of Mrs. K——, of W—— Place, aged 34 years, pregnant the first time. The abdomen was excedingly bulky, and the uterus appeared to be stretched to its utmost extent. About a week previous to her confinement, she complained of swellings of both the upper and lower extremities, and remarked that she had scarcely felt any motion of the child for some days. On placing my hand on the right hypochondrium, close to the mar-

gin of the ribs, I distinctly felt a hard round substance, which conveyed the impression to my mind that it was the head of a child. At the lower part of the left iliac region, I found a similar body, of a globular form, which I concluded was the head of another child. From what occurred at the time of delivery, my conjectures proved to be perfectly correct.

On Sunday, February 13th ult., at one o'clock P.M. my attendance was requested by her husband, who informed me that she had some symptoms of labour. On my arrival she complained of frequent pains in her back, which were of short duration. The liquor amnii had escaped rather suddenly about two hours, without any previous pain. On examination, I ascertained that the os uteri was dilated to about an inch in diameter, and felt a spinous process of the vertebra of a child. Very gradual dilatation went on for about two hours, when the breech descended into the pelvis. The expulsory pains were feeble, rendering it necessary to afford some manual assistance to aid the delivery, which was effected with some difficulty about four o'clock P.M. The child was dead, as I expected it would be, from the long continued pressure upon the funis, which must have existed for nearly an hour before its birth. I found that the obstacle which impeded delivery was the head of another child, situated at the lower part of the uterus. Immediately after its extraction, a small quantity of fluid blood escaped, which was succeeded by a slight shivering, and the os uteri immediately contracted. The abdominal bulk was but little diminished; and from the size of the uterine tumor, independent of any other evidence, I should have concluded that it still contained a fœtus.

Every thing now remained stationary; there was no further uterine contractions, and not the slightest pain indicative of labour. I occasionally endeavoured, by external friction, and by irritating the os tincæ, to excite the uterus to action, but without the desired success. In this state she continued, perfectly tranquil, for eight hours, there being no hæmorrhage, nor unfavourable symptom to require any interference. Having waited till near twelve o'clock, I determined on the exhibition of a full dose of secale cornutum, with the intention of repeating it if the first should not have the desired effect, but in less than a quarter of an hour, pains succeeded its administration; the os uteri dilated, and the membranes protruded. Immediately upon their rupturing, the left arm passed down into the pelvis, violent uterine action instantly followed, and the abdomen of the child was pressed across the pelvis. I readily found the feet, which I drew down, and soon completed the delivery of a living male child. From the commencement of the effect of the secale cornutum to the birth of the child, could not have exceeded five minutes. The placentæ, which were slightly attached to each other, were extracted in about half an hour. A tight bandage was placed round the abdomen. The sanguineous discharge was not more in quantity than usual, and the uterus gradually contracted in a natural manner. The pulse was good, and every symptom favourable. The present period, (being the eleventh day) she is doing well. She suckles her child, and the lochial discharge is healthy.

I consider that the difficulties in this case arose from the circumstance of the child situated nearest the fundus of the womb coming first into the world, which was probably occasioned by its membranes accidentally rupturing. Had the children been small, it is possible this might not have been of any consequence, as there would not have been the same hindrance to its progress. In all the twin cases I have previously attended, I have supposed that the fœtus situated nearest to the os uteri was first propelled into the world, and have almost invariably found the other immediately to follow, no instance having ever occurred to me in which more than an hour has elapsed between the birth of each child, with the above exception.

ON THE FORM AND STRUCTURE OF THE MEMBRANA TYMPANI.

By Henry Jones Shrapnell,

Surgeon to the Royal South Gloucester Regiment of Light Infantry Militia, Member of the Royal College of Surgeons, and Fellow of the Geological Society of London.

(With a Woodcut.)

The form of the membrana tympani has been variously described by different

authors—namely, as being round, oval, roundish, irregular, &c.—and its attachment to the os malleus has been said to be by means of the mucous membrane of the cavity of the tympanum; and, again, that the manubrium of that bone forms one of the radii of the round membrane, and terminates in its centre. These, and other assertions equally incorrect, prove that the minute anatomy of the organ of hearing is yet a desideratum in anatomical science. A correct knowledge of the anatomy of the ear, is, however, not only of the utmost importance to the surgeon in a pathological point of view, but it forms a most interesting field of research in its connexion with the science of acoustics. It does not even appear that the beautiful forms with which the organization of the ear is so replete, have ever been examined, in order to trace the analogy they must undoubtedly bear to the vibration and transmission of sound. Indeed it is not unlikely that a careful and attentive examination of the wonderful, yet playful variety of structure, exhibited in the different orders of the creation, adapted to the habits of particular individuals, as well as to the various media in which they live, may throw some light on the unknown nature of sound itself.

In examining the ear with such a view, too much attention cannot be paid to form, as an essential part of structure. Partly with this view, but chiefly on account of its importance to the surgeon, I am induced to give the following description of the membrana tympani, and of a structure connected with it which does not appear to have been hitherto noticed.

In order to demonstrate the true shape of this membrane, the groove of bone into which it is inserted should be carefully cut away, as it conceals and tends to give its outline a more irregular figure than it really possesses. When thus cleared, the membrana tympani resembles the figure of a horse-shoe.

Three-fourths of its circumference forms three-fourths of a correct oval, the remaining fourth being, as it were, cut off; leaving two angular portions, with a connecting space between them, which has a process projecting inwards towards the centre of the oval.

Situation.—In this outline we therefore distinguish an anterior superior angle, which is situated nearly on a level with the base of the zygomatic process of the temporal bone; a posterior angle below the level of the anterior angle, and inclined a little more outwards, which is the nearest point of the membrane to the mastoid process; a process between the angles, but nearest to the anterior, which gives origin to the suspensory ligament of the malleus; and a portion forming three-fourths of an oval, which descends from the two angles forwards and inwards, in a slanting diagonal direction across the inner termination of the meatus externus, so as to form an acute angle with the inferior surface or floor of that canal.

Structure.—The membrana tympani will be found to be made up of two structures, which differ from each other both in their functions and physical properties. The principal structure appears to be destined for the purposes of vibration, when acted upon by the percussive force of sounds entering the external meatus. It is composed of elastic radiating fibres, which have two points of attachment—one fixed to a groove of bone in the termination of the external meatus, the other, moveable and central, to the manubrium of the malleus.

The other structure possesses considerable elasticity, but does not appear to have that tension which is requisite for vibration. It is not given off from a groove of bone, neither is it composed of radiating fibres; but of fibres irregularly arranged, similar to those of cellular membrane. It is flaccid—so much so, that when air is blown into the cavity of the tympanum, through the eustachian tube, it bulges out, while the remaining portion of the membrane remains comparatively unaltered. In many animals, particularly in rabbits and sheep, similarly treated, the appearance of a tumor, of considerable elevation, presents itself. These portions of the membrana tympani may therefore be distinguished by their physical properties of tense and flaccid elasticity; for although both of these states answer to the definition of elasticity, yet, as far as they regard the propagation of sound, they are directly opposed to each other; the former being highly favourable to, while the latter totally annihilates the vibration of sound.

To facilitate a particular description of these membranes, it will be necessary

to distinguish them by appropriate names. I shall therefore name them membrana tensa and membrana flaccida, answering to their different states of tension and flaccidity. The membrana tensa occupies the whole of the termination of the external meatus, in which there is a groove. This groove commences rather abruptly, at a point about half a line below the anterior superior angle, external to, and about a line above the entrance of the sulcus, which receives the processus gracilis of the malleus; it extends around the oval portion to the posterior angle, and from thence to the apex of the process between the angles, where it terminates. It is remarkable that this groove is formed of a peculiarly dense, white, ossific structure; the earthy matter of which is deposited earlier than in the surrounding part of the temporal bone. It emanates from a distinct centre of ossification, having its fibres arranged precisely similar to those of a rib bone. It may readily be detached in a macerated fœtal head of eight months, forming an incomplete ring, which at this period exhibits a good view of the form of the membrana tensa. The commencement of the groove for the reception of the processus gracilis is included in this early ossified structure.

At birth, the two extremities of the incomplete ring are united to the squamous portion of the temporal bone; and as a further deposit of bony matter takes place, the oval circumference becomes united to the petrous portion: after which the external meatus commences its formation, and continues to the extent which it acquires in the adult state. An interesting question arises on considering this dense ossific structure: is it for the purpose of maintaining the membrana tensa at all times in a state of tension? I have reason to think it is; for if in a sheep recently killed the cavity of the tympanum is opened, and the head of the malleus is detached from the incus, it immediately springs up from the force of this tension, drawing the manubrium of the malleus downwards and outwards. Now, if this reasoning is correct, it sets at rest the controversy respecting the muscularity of the membrana tympani; it being contrary to the known laws of muscular action to be always accompanied by a state of tension. All the appearance of muscular fibres which I have seen connected in any way with the membrana tensa, has been in the ox; where the upper portion of the termination of the external meatus exhibits some pale fibres, which appear to pass over the membrana flaccida, and to be connected with the membrana tensa at its upper part, and are lost towards the sides of the membrane. If these are muscular, they must act by increasing tension only. The manner in which the central fibres of the membrana tensa are fixed to the manubrium of the malleus, equally merits attention. In order to give an accurate idea of this, it is necessary first to observe the particular form of the manubrium itself. This process of the os malleus is not straight, so as to form one of the radii of the fibres of the membraua tensa, but it presents an external surface which is flattened and curved in two directions, and on which there is a slightly elevated ridge extending from the base to the apex along its edges. To the whole extent of this ridge the central fibres of the membrana tensa are fixed, radiating from it, and not from the apex only. At the apex of the manubrium this ridge passes around it, forming a small oval curve corresponding exactly to the lower and internal part of the groove in the oval circumference. A space is left between this ridge, extending from the apex nearly to the base of the external surface of the manubrium, which is covered only by periosteum and cuticular integument.

In this space a main artery passes, to distribute its minor ramifications between the radiating fibres of the membrana tensa. At the base of the manubrium the malleus has a turbercular projection, directed towards the process between the angles, to which is attached the terminating fibres of the membrana tensa; being here firmly fixed, somewhat in the nature of a ligament, passing from the tubercle on either side to the terminations of the oval groove. From this point of the malleus, thus hung in the centre of the ligament, the manubrium does not descend to the centre of the membrana tensa, but continues nearly across two-thirds of its extent, and nearer to the anterior side, next the glenoid fissure, than to the posterior portion, next to the mastoid process. Nevertheless, the apex is not so near to the circumference of the membrane as it at first appears, on ac-

count of its projection inwards to the cavity of the tympanum, which projection increases from the base on the plane of the oval circumference gradually to its apex, which is half a line or more within that plane; the apex itself being slightly incurvated outwards and downwards, from the cavity of the tympanum, and forwards towards the glenoid fissure, in an opposite direction to the tubercle at the base.

From this construction, it follows that the fibres of the membrana tensa present curvilinear forms in every direction, and which appear to be precisely those best calculated to produce a play of delicate movements, according with the vibratory force of sounds. It would be too voluminous a subject here to enter into the consideration of the particular action of the muscles of the ossicula auditus. I may, however, briefly state, that I consider that the muscle termed laxator tympani cannot possibly draw the malleus outwards, but it contributes to increase tension, and should be named musculus tensor tympani externus.

The arteries of the membrana tympani are derived from the stylo-mastoid branch of the facial, and take their course around its circumference, and along the manubrium of the malleus, as already mentioned. From the circumference they are distributed between the tense fibres, and consequently in a converging direction towards the malleus.

The membrana flaccida occupies the anterior superior angle, and so much of the membrana tympani as is above the tubercle of the malleus, and the ligament in which it is hung.

Its figure is somewhat triangular, being circumscribed by the above-named ligament inferiorly; and anteriorly and posteriorly by thin edges of bone in the upper part of the termination of the external meatus. These thin edges are of very irregular formation, and in some instances inclose a space of double the dimensions which are met with in others. These dimensions vary in proportion to those of the membrana tensa, from about a ninth to a sixth part.

The membrana flaccida differs from the membrana tensa in several particulars:—

First, in its flaccid state; secondly, in not being attached to a groove of bone of peculiar formation; thirdly, in the irregular distribution of its fibres and blood-vessels, and in its variable form; fourthly, its inner surface is lubricated with mucus, similar to the rest of the walls of the tympanum, excepting the membrana tensa, and membrana fenestra rotunda, which present a polished shining surface; fifthly, the plane of the membrana flaccida is level, and differs from the plane of the membrana tensa, being inclined more outwards, in the direction of the roof of the external meatus.

Its form in different mammalia varies more particularly than in the human subject. In the ox, it is an elongated fissure; in the pig, in form of a truncated bent cone; in the rabbit, round; and in the hare, oval. It differs also in its relative proportion to the membrana tensa. It is very small in the cat; it is more developed in man; half the size of the membrana tensa in the hare; while in the sheep it equals, if not exceeds, the size of that membrane.

The great extensibility of the membrana flaccida suggests the idea that it is formed for the protection of the more tense fibres, during the effects of sudden and loud sounds, or the actions of coughing and sneezing, when, by its yielding, it saves the tense fibres from being ruptured. In the hare and the sheep (timid and defenceless animals, were it not for the extreme delicacy of their ears, warning them of approaching dangers), this structure is remarkably developed. Their membranæ tensæ are extremely thin, tense, and transparent as glass; not like the human subject, where it is more like wetted parchment.

In a surgical point of view, the membrana flaccida offers the most eligible situation for puncturing the drum of the ear, in cases of closed eustachian tubes; not only because it is that part which is most readily seen on looking into the meatus auditorius externus, but because an opening here would not interfere with the function of the membrana tensa, which appears to be that which is principally concerned in the sense of hearing.

It is not, however, the membrana flaccida which has given way in all those cases of accidental rupture, in which smoke can be expelled by the external part of the ear, but the lower part of

the membrana tensa, where the play of the malleus has its most extensive movement. The precise situation of the membrana flaccida being well learnt, the instrument should only just pierce the membrane, on account of the situation of the neck of the malleus, which passes upwards immediately behind, and would be liable to be injured by an unskilful hand.

19, Russell-Street, Brixton Road.

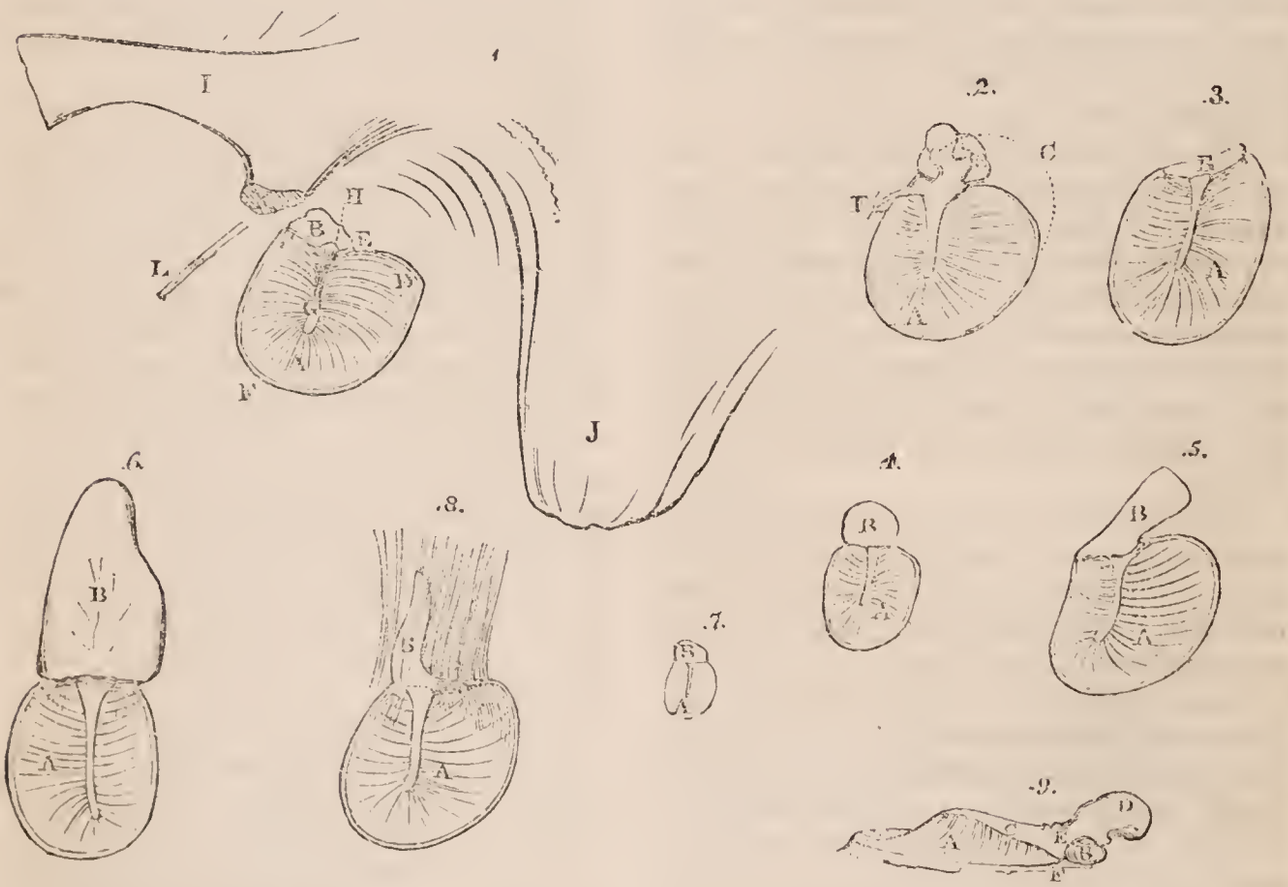
Fig. 1.—*External view of the membrana tympani, left ear.*

A, In this, and in each figure, the membrana tensa.
B, In this, and in each figure, the membrana flaccida.
C, Anterior superior angle.
D, Posterior angle.
E, Process between the angles.
F, Oval circumference, with a second line introduced, to shew the extent of the groove.
G, External surface of the malleus, to which the membrana tensa is not attached.
H, Tubercle at the base of the manubrium of the malleus.
I, Zygomatic process of the temporal bone.
J. Mastoid process.
K, Portion of the base of the zygomatic process sawn off.
L, Situation of the glenoid fissure.

Fig. 2.—*Internal view of the membrana tympani, right ear.*

B, Membrana flaccida much developed.
C, Dotted line, completing an oval figure.
E, Processus gracilis entering the glenoid fissure.

Fig. 3, Cat; Fig. 4, Rabbit; Fig. 5, Dog; Fig. 6, Sheep; Fig. 7, Rat; Fig. 8, Ox; C, Appearance of muscular fibres in the meatus externus; Fig. 9, Profile of the internal surface of the membrana tympani; C, Manubrium; D, Head; E, Neck; F, Tubercle of the os malleus.



CASES OF SCROTAL HERNIA.

To the Editor of the London Medical Gazette.

SIR,

SHOULD the two cases enclosed appear to you of sufficient interest for publication, I should be obliged by their insertion in your journal. They will serve to point out some of the difficulties and obscurities occasionally met with in the treatment of old ruptures.

I am, sir,
Your obedient servant,
GEORGE BUSK,
Assistant Surgeon.

Seamen's Hospital, Dreadnought,
February 24, 1832.

CASE I.—E. G. ætat. 56, admitted January 24th, 1832; hale looking; complains of purging of scanty watery stools, and occasional vo-

miting; feeling of inflation; abdomen tense and tympanitic, especially across the upper part; slightly tender when suddenly pressed; skin cool; pulse 120, hard; tongue furred; has a large old scrotal hernia on the right side, which has been irreducible for five years, and is at present quite free from pain or uneasiness; its contents appear to be hardened omentum posteriorly, and intestine anteriorly. Has been ill for two or three days.

Calomel, gr. v.—H. purgans.—Enema purgans.

In the evening the belly became considerably more tense, and very painful. He has had, since admission, two stools; the first of which was thin, but feculent, the second watery. The hernia is not all painful, and by continued taxis some part of its anterior contents can be returned into the abdomen, but are protruded again immediately when the pressure is removed.

V. Sect. ad Syncopen.
Hirudines, xx. abdomin.
Calomel, gr. iij. 2dis horis
Magn. Sulph. ℥j. alternis vicibus.

25th. — General appearance much the same. No stool, and the enemata return as injected. It was now resolved to cut down and divide the external ring, which was done in the usual manner, but there appearing to be little or no change induced by that measure, the sac, which was very thin, was opened to a small extent at its upper part, and the passage into the abdomen found to be quite free. The contents of the sac, of perfectly healthy appearance, consisted of part of the colon and some convolutions of small intestine. Those situated anteriorly were the colon, which could be returned to some extent, but the posterior were the small intestine, and were firmly adherent to the sac. Great difficulty was experienced, on closing the wound, in keeping the intestine confined, and it was requisite to insert five stitches for that purpose.

26th.—Much the same; has very little pain or tenderness; vomits every thing; no stool.

Pergat.

27th.—Does not appear any worse. Has had no stool; very little tenderness in the abdomen, and none in the sac. Vomits after taking any thing, but not otherwise. Countenance and voice good; skin natural; pulse 90, soft.

Pergat.
Ol. Croton, gtt. j. Ext. Col. Co. gr. x.

In the evening he seemed no worse The dressings were removed, and the wound appears in a favourable condition. A few small scybala came away with the last enema.

Ol. Ricini, ℥vj.

28th.—About twelve last night, soon after taking the castor oil, he was attacked by severe pain in the left side and upper part of the abdomen, below the situation of the stomach, which has been very severe ever since. No tenderness in the tumor, or its neighbourhood.

T. Opii, ℥ss. statim.
Hirudines, xxx. abdomin.
Fotus Calid.
Enema ex Ol. Terebinth.

In the evening complained of less pain, but was evidently sinking fast. The vomiting had been stercoraceous for two hours. Died at 12 P.M.

Autopsy.—The whole of the small intestines much distended with air; peritoneum universally in a state of inflammation, but which seemed to have been most violent in the right side; effusion of lymph and seropurulent fluid. The contents of the hernia were the lower end of the ilium and the cæcum, of course including the ilio-colic valve. The ilium was posterior, and firmly adherent to the sac, which in that part was enormously thickened and indurated, and was the occasion of the feeling as if omentum were included in the hernia. The colon occupied the anterior and lower part of the tumor, and was partly reducible. There was no appearance of stricture about the hernia, but that part of the ilium contained in the sac was much contracted in its diameter and indurated, and immediately above the ilio-colic valve presented several small ulcerations, some of which had completely perforated the coats of the intestine, allowing the escape of its contents, by which apparently a high degree of inflammation had been excited in the posterior part of the neck of the sac, which had proceeded to suppuration and incipient gangrene. The part of the ilium above where it entered into the hernia, was intensely inflamed, and distended with fluid fæces. The cæcum contained in the tumor was perfectly natural in appearance and empty, as was the remainder of the colon and rectum. A portion of the omentum on the right side appeared as if it had been at one time included in the protrusion, but was not so when the examination was made, and probably had been liberated by the taxis in the first instance. There was a small hydrocele of the tunica vaginalis.

Case II.—W. H. ætat. 35, admitted March 6th, 1831, of thin spare habit; complains of some pain in the abdomen, which is tympanitic, and of constipation of the bowels. Has been long the subject of large scrotal hernia on the right side, which is easily and complete-

ly returned into the abdomen. Pulse, tongue, and skin, natural; has been ill for some days.

Ol. Ricini, ℥j. A truss.

7th.—The castor-oil not having operated, he had last evening a purgative enema, from which he had two scanty feculent motions. The castor-oil and enema were repeated this morning without effect, and he afterwards took

Ol. Croton, gtt. ij.; Ext. Col. Co. gr. x.

The abdomen is more tense; pressure firmly applied is borne without pain; no vomiting; pulse natural.

Leeches and fomentations.

In the evening had another purgative enema, and was bled to sixteen ounces, when the pulse failed (blood presented no inflammatory appearance). Has had no sleep for several nights, and has taken scarcely any food.

Calomel, ℈j. Opii, gr. ij. h. s. s.

8th.—No stool; passed an easy night, and slept for a short time. Abdomen more tympanitic, and he now complains of some, but very slight, pain on pressure; is troubled with severe occasional pains, recurring frequently, and lasting but a short time, causing him to cry out.

Haust. Purgans. Enema ex Ol. Terebinth. ℥ij. et Aq. Cal.

and a second enema, with mist. assafœtidæ, produced no appearance of feculent discharge; and another dose of castor-oil excited vomiting for the first time, which, however, has not since returned. The hernia now, as before, returns with the utmost ease when down, and the abdominal ring can be clearly felt, and the spermatic cord quite unconnected with any other structure. In the evening purgatives were repeated, with the warm-bath and venesection to syncope, but without the desired effect.

9th.—No evacuation; the spasmodic pains recur at shorter intervals, and the abdomen is more tympanitic and much more sensitive to pressure; voice hollow. After the administration of several purgative enemata, a drachm of tobacco, infused in a pint of water, was exhibited at twice, but without perceptible effect. Afterwards, an enema of a large quantity of cold water excited much uneasiness, but no fæcal discharge. He has vomited two or three times at long intervals, and is reported by the nurse to have thrown up last night a fluid resembling fæcal matter; that vomited to-day consists of brownish coagula, but without fæcal smell.

To take Croton Oil, gtt. ij. every hour through the night.

10th.—Has taken twelve doses of the oil in pills, but without effect. No vomiting; some hiccup; speaks in a whisper, and appears much sunk. Abdomen very tympanitic; thinks and says that he is about to die. The hernial tumor is more tense, and now requires slight manipulation to be returned; he has not latterly been able to endure the pressure of the truss on the abdomen, to keep it up.

Died in the afternoon, somewhat suddenly.

Autopsy. — On examination, there was found a long, slender, membranous band, extending from the lowest part of the hernial sac to the transverse arch of the colon, which was evidently formed by the omentum, and was looped round the colon in such a manner that when the lower extremity of the band was drawn downwards, that part of it which surrounded the colon was tightened so much as to completely close the canal; but when left slack, the stricture was not so great as to prevent the passage of fæces. The intestine above the stricture was enormously distended with flatus and fluid matter, and highly inflamed; that below it, natural. There was no appearance of any portion of intestine having been in the slightest degree strangulated in the hernia, nor were there any adhesions between the sac and intestine. The band above described was by no means of recent formation, and it appeared evident that, when the rupture was down, the band must have been put so much on the stretch as to produce a considerable degree of stricture around the arch of the colon.

It is worthy of remark, that he had been at many previous times subject to attacks of constipation and other derangement of the bowels.

FEVER RECENTLY EPIDEMIC.

To the Editor of the London Medical Gazette.

Sir,

I have been anxiously waiting in the hope that some one better able than myself, and whose advantages have been more extended, would give the profession an account of the peculiar kind of fever which, for the last few months, has been raging among the poor in London.

Having had much opportunity of observing, and attending upon some very severe cases of fever, lately occurring in the Middlesex Hospital, I am induced to offer you a few remarks.

Fevers, so called, vary in character,

not only in different seasons of the year, but in the same seasons in different years. Those of the present have been characterized by a peculiar state of debility, accompanied by all its concomitant dangers, as petechiæ, intermitting pulse, subsultus, and low delirium.

There would appear, for the last few months, to have been a particular condition of the atmosphere, which, like a poison, affects the system in a very remarkable manner;—patients of all ages, whether strong hearty men, or weak delicate females, having been equally affected, the nervous system in all being first attacked, and petechiæ, or an eruption resembling rubeola, occurring almost invariably, even in cases where the fever itself was not of a very severe character. In public institutions, patients are admitted in every stage of disease, some having been ill only a few days, others having been under medical treatment for a longer time. In many instances under my own observation it was found necessary to send the patient immediately to bed, and to endeavour, by warmth and stimulants, if possible, to rouse the system, apparently in the last stage of existence; and if these methods were not diligently employed, the patient would rapidly sink, never again to be rallied. If, however, this stage was got over, the other symptoms of fever presented themselves. The pulse, from having been scarcely perceptible, would become frequent, feeble, and perhaps intermitting; low delirium, with subsultus, would ensue, the petechiæ enlarge, the abdomen resent pressure, the tongue become dry, brown, and tremulous, and the patient remain in this state for some days. As to the organs chiefly involved, the brain appears to have been early the seat of disease, and the abdomen, in most instances, subsequently affected. In many patients who died, shortly after admission, as it were in a state of collapse, the only morbid appearances detected were congestion of the vessels of the membranes, and also of the substance of the brain, bloody points following each incision. When recovery from this stage was effected, although the prognosis was for some time unfavourable, no patients died; therefore, nothing certain can be stated as to the extent of mischief the disease occasioned, but probably increased congestion would have been found in the brain, and inflammation and ulceration of the intestines.

Treatment.—It was generally found, the more simple the treatment, the earlier the approach to convalescence. The chief indications seemed to be to support the system by nutritious food frequently administered, and often by wine in small quantities; at the same time to guard against increased congestion of the brain, by cold applications to the head; and to arrest the ravages of inflammation and ulceration of the intestines by mild mercurials. If depletion was resorted to, whether locally or generally, the low stage of the fever was more severe; and if the patient was not carried off at this period, the urgent symptoms were protracted for many days, and convalescence retarded for some weeks,—shewing that general depletion was decidedly improper, and that local bleeding, when absolutely necessary, was to be employed with the greatest caution.

In the most severe cases, calomel and antimony seemed quite inadmissible, but the hydr. c. cretâ proved to be of the greatest benefit, and when the bowels were relaxed, the same combined with Dover's powder.

But there is one point of practice which has been found highly beneficial in this peculiar kind of fever, which is deserving of notice. In that state of the nervous and vascular systems where the patient is affected with low muttering delirium, and the pulse scarcely above 100, with no disposition to sound sleep, patients seem literally to have been saved by small doses of the acetate of morphia. I more particularly mention the exhibition of this salt as it seems to be deprived of all the injurious and stupifying properties of opium, the patients awaking in the morning, after a night of good sleep, with the tongue moist, subsultus diminished, skin cool, delirium lessened, and, as it were, a crisis taking place, from its exhibition.

I subjoin a few of the most interesting cases, by way of illustration, if you think them worthy of insertion.

In the first four, convalescence was rapid, no depletion having been employed; and, in the succeeding two, recovery was retarded, in consequence, apparently, of depletion having been adopted.

Case I.—Daniel Thornton, under the care of Dr. Watson, Feb. 14, æt. 27: upon admission appeared to be in a dying state; said to have been ill about a fortnight, and to have kept his bed four days. He was extremely feeble, and unable to give any account of himself: the pulse scarcely perceptible; the tongue covered with a brown fur; mouth and teeth covered with sordes; extremities cold; and a rubeolous eruption, mixed with petechiæ, all over the body. After he was put into a warm bed, and a small portion of brandy had been given, the vital functions were less depressed. The pulse became perceptible; was 110, feeble, but regular. The countenance flushed; eyes suffused; constant rambling; eruption more general. Abdomen full, but not resenting pressure. Subsultus and tremors.

Habeat Sp. Æther. Sulph. Co. ℥ss. ex Mist. Camph. 6tis horis.

Cold lotions were kept constantly applied to the shaved head.

15th.—The night was passed without sleep, with incessant low delirium. Countenance flushed; pulse 100, feeble: answers rationally, but in a hurried and indistinct manner; one scanty, dark evacuation.

Pt. c. Mist. Ætheris. Pulv. Magnes. Co. ℥ss. ex Aq. Menth. Pip. stat. et nocte Liq. Morphiæ Acet. ♏v.* è Mist. Camph. To have beef-tea, arrow-root, or sago, in small quantities, every hour.

16th.—Rambled much in the early part of the night, but had some quiet sleep towards morning. Pulse very weak; tongue moister at edges; other symptoms continue the same. One evacuation since last report.

Pt. et habeat Hydr. Submur. gr. ij. statim; et Ol. Ricini, ℥ss. post horas tertias. Rep. Liq. Morphiæ nocte.

17th.—Has had some very quiet and refreshing sleep. Countenance improved; pulse 86, with rather more power; tongue continues to improve.

Contr. Mist. et Liq. Morphiæ, et hab. Vini Rubri ℥iv. quotidie.

From this time he rapidly became convalescent, and was up and about on the 1st of March.

Case II.—Martha Ridge, æt. 42, also under Dr. Watson, Feb. 17th.—Countenance pale; cannot be roused; deep sighing; petechiæ general; abdomen full, resents pressure in the iliac regions; tongue covered with a brownish black fur; pulse 100, very feeble; ailing five weeks with pain in the head, stomach, and sickness; wanders at night.

Brandy ℥j. Hydr. c. Creta, gr. v. his. Beef-tea, arrow-root, or sago, omni horæ. Enema avenæ. Radatur cap. et Lot. frigid. app.

19th.—A quiet night; constantly drowsy, but easily roused. Had a copious, solid, dark evacuation after the enema; pulse 90, and soft; tongue cleaner.

20th.—From this time the coma diminished; tongue cleaned; petechiæ began to disappear; she became more alive in her manner; and gradually became convalescent by the 4th March.

Case III.—William Harrison, admitted January 11th, under Dr. Watson, æt. 65. Countenance much emaciated; skin hot and dry; abdomen supple, and he does not wince when it is pressed; bowels much purged; pulse small and feeble, and intermitting, subsultus; tongue dry and brown; much delirium. Ill five days, with pains in the limbs, and general malaise.

Hydr. c. Creta. Pulv. Ipec. Co. aa grs. v. nocte maneque. Ammon. Carb. grs. v. e. Mist. Camphor, 6tis.

The next day, pressure made over the cœcal region seemed to give him pain.

12th.—Improving; less pain in abdomen, and better in every respect; much delirium at night.

16th.—Great inclination to sleep; tongue rather more moist; pulse very small; skin less dry.

Wine, ℥iv. quotidie; arrow root and milk sæpe.

Under this treatment he continued to improve, although very slowly, until the 18th, when he suffered a relapse; all the former symptoms returned, the abdomen became very painful on pressure, petechiæ made their appearance, and there seemed no chance of recovery.

Ordered to omit the æther; to take wine, milk, strong beef tea, and arrow root, by turns every half hour, and a blister to be applied to the neck.

He again rallied and improved rapidly, and was up and about the ward Feb. 1st.

In this case ulceration probably existed in the intestines, which repaired themselves in spite of the advanced age and enfeebled constitution of the individual; his recovery being attributable alone to attention and care, and keeping up the sinking powers by food and nourishment every half hour.

Case IV.—John Hodge, admitted under Dr. Hawkins February 14, æt. 38. Countenance pale; pain in head; abdomen soft, with slight pain on pressure; bowels purged; tongue covered with a moist fur,

* This is equivalent to one-third of a grain of the acetate.

pulse 120, with power; has a cough. Ill eight days, first with rigors, pains in the head and limbs. Perceived an eruption this morning, for the first time, of petechiæ, interspersed with papular spots resembling rubeola.

Hydr. c. Cretâ gr. x. o. n.

15th.—Symptoms continue the same; has vomited bilious matter twice this morning.

16th.—Tongue very dry; bowels still relaxed; pulse very weak; cough continues troublesome.

Haust. Ammoniæ Tartratis c. Vin. Ipecac. ♏x. et Sp. Æther. Nitric. ʒss. ter die.

20th.—Countenance flushed; tongue still dry in the centre, and brown all over; no sleep at night; answers incoherently; eruption continues; slight dyspnœa.

Emp. Lyttæ pect. Omit. Hydr. c. Cretâ. ℞ Camphor. Calomel. aa. gr. ij. Opii gr. ¼ 6tis. Rep. haust.

22d.—Countenance more sunk; tongue more dry, and scarcely able to be protruded; pulse very weak.

Vini Rubri ℥iij. quotidie. Beef-tea and arrow-root sæpe.

From this time he went on slowly improving, the bowels becoming regular; eruption disappearing; nights more tranquil; and was up and by about the 7th of March.

This case tends to illustrate the excessive debility which arose at the accession of the fever, the eruptions appearing before the other severe symptoms showed themselves.

Case V.—Augustus A. admitted Feb. 25, æt. 20. Countenance heavy; severe pain in head; pain in the iliac regions upon pressure; skin covered with a faint but well-marked eruption, resembling rubeola; tongue chapped and dry; pulse 100, and full; head very hot; ill six days with rigors, heats, and pains in head; the latter was relieved last night by V. S. ad Oiss.

Hydr. c. Cretâ gr. v. ter. Radatur cap. et app. Lot. frigid.

This patient became worse, and, on the night of the 1st, the following was his state:—Nights restless; more wandering; tongue more dry, and can scarcely be protruded; skin hot; pulse feeble, and 100; muscæ volitantes; tinnitus aurium; slight subsultus; abdomen painful; bowels rather relaxed; a copious crop of petechiæ, very livid, have appeared upon the arms; the eruption fading.

Habeat. haust. Aromat. statim c. Liquor. Morphiæ Acet. ♏x.

March 2d.—Slept very well; no wandering; tongue less dry, but thickly furred; skin cooler; petechiæ rather on the increase; is stupid and heavy.

Pt. haust. Anod. vespere.

From this time no more morphia was required; he passed quiet nights; all the formidable symptoms, with the assistance of wine and nourishment, slowly receded, and he gradually improved, but was not able to get up until the 25th.

Case VI.—J. L. æt. 32, admitted Jan. 30. Countenance flushed; pain in head; conjunctivæ suffused; skin hot, and covered with a faint eruption; bowels purged; pulse 100, very feeble; tongue with a brown fur, rather moist; pain in abdomen; ill six days with general malaise; has only "laid up" two days.

Hydr. c. Cretâ, Pulv. Ipecac. Co. aa. gr. v. omne nocte. Abrad. Cap. et app. Lot. frigid. Hirud. xij. temporibus.

31st.—Leeches bled profusely, but the pulse became so reduced as to require the exhibition of ammonia; eruptions more general, and more like petechiæ. *Vespere*—wandering, talking, and singing; subsultus; pain on pressure in the right iliac region.

Feb. 2d.—A restless night; petechiæ more distinct; skin rather less hot; abdomen tense and painful; tongue dry, brown, and tremulous; subsultus increasing. Has beef-tea, arrow-root, or wine, every hour.

Vespere, 11, P.M.—Has begun to mutter and talk; subsultus increased; no disposition to sleep, and has had no sound sleep since admission; can scarcely protrude the tongue.

Liq. Morphiæ gtt. x. statim.

3d.—Fell asleep soon after the morphia, and was awoke at 9 A.M. to take some nourishment, when he appeared refreshed and less wandering. He soon relapsed into his former state, but with no appearance of coma, and was easily roused.

Hydr. c. Cretâ gr. v. nocte maneque, sine Pulv. Doveri.

He required two more opiates, after which he gradually recovered, the symptoms becoming milder, but was not up until the 20th.

I fear I have already intruded upon your valuable pages, therefore am unwilling to add any more cases.

I remain, sir,
Your obedient servant,
J. F. Elwin.

Middlesex Hospital,
March 26, 1832.

CASES OF EYE DISEASES, WITH REMARKS.

By Mr. William Mackenzie,
Lecturer on the Eye in the University of Glasgow.

No. III.

Dacryocystitis Scrofulosa.

"The best mode of obtaining and extending medical and surgical knowledge is, in my opinion," says Mr. Abernethy, "to pay that strict attention to diseases which qualifies us to note even the slighter shades of difference that distinguish them from each other." Going upon this principle, it might be proper to distinguish inflammation of the excreting lachrymal passages, as it occurs in strumous children, from the same disease, whether acute or chronic, in adults, more or less of a healthy constitution. In strumous children, this complaint is more frequently connected with caries of the bones, forming the osseous canal for the transmission of the nasal duct; is often attended with extensive destruction of the integuments round the fistulous opening, communicating with the sac; and occasionally presents, as a complication, ulcers on the tonsils, which might readily be mistaken for syphilitic ulcers. In such subjects the style is difficult to manage, is not productive of the same amount of benefit as in the adult, and not unfrequently must even be laid aside, on account of the great irritation it occasions On the other hand, constitutional remedies are of much greater utility in cases of strumous inflammation of the lachrymal sac than in the acute or chronic dacryocystitis of adults.

In the following instance, the cure was obtained without any manual interference with the parts affected, and a considerable share of the benefit appeared to arise from the use of iodine.

Margaret Philips, aged 16, was admitted at the Glasgow Eye Infirmary, 19th April, 1831, with a circumscribed swelling at the inner extremity of the left lower eyelid, the centre of the swelling ulcerated, and the ulcer probably communicating with the lachrymal sac, although the opening was too small to admit an ordinary probe. These symptoms had been preceded by a watery eye. The nostrils had the fœtor of carious bone. She was ordered to apply a poultice over the swelling, and to take a Plummer's pill morning and evening.

She returned on the 23d, with the right lachrymal sac in a state of inflammation. Two leeches were applied, and she was ordered two drachms of sulphate of magnesia every morning.

On the 27th, the symptoms on the left side were abated.

On the 29th, the mouth being affected, the pills were omitted.

On the 4th May, blisters were applied behind the ears, and she was ordered to use the diluted citrine salve.

On the 9th, the inflammation over the lachrymal sacs was very much diminished.

On the 12th, the blisters were reapplied, and afterwards dressed with infusion of cantharides ointment. Ten drops of a solution of one drachm of iodine in one ounce of alcohol, were ordered to be taken thrice a day.

On the 1st June, the dose was augmented to fifteen drops.

By the 30th, a decided improvement had taken place.

On the 1st July, the iodine was increased to twenty-five drops thrice a day.

No further report of the case is entered in the journal till the 5th Feb. 1832, when it is stated that the ulcerated opening into left lachrymal sac has been healed for some months, and that the only symptom complained of is stillicidium. The iodine was continued, along with the diluted citrine salve. Warm water was ordered as a collyrium.

On the 2d April, the stillicidium was not so troublesome, the general health greatly better, the lachrymal sacs and neighbouring parts apparently sound, and no fœtor from the nostrils.

Tumors in the Eyelids.

Case I.—Andrew Duncan, aged 2, 3d Feb. 1832, presents a granulated whitish tumor, about the size of a large pea, growing from the right lower lid, and covered merely by the skin. It first appeared six months ago.

A simple incision through the integuments and through the tumor, followed by pressure, forced the growth easily from its seat. It was found to be solid, of considerable consistence, and formed of a congeries of roundish grains, without any cyst.

On the 10th, the incision was perfectly healed, and the case dismissed.

Case II.—Janet Adie, aged 23, 29th Feb. 1832, applied at the Infirmary on account of an oblong tumor seated in the right lower eyelid, of six months' standing. The integuments covering the tumor were red, and tender to the touch; the conjunctiva on the corresponding part of the inside of the lid was inflamed, and the cartilage apparently thinned.

The tumor was opened with the lancet on the inside of the lid, and the gelatinous-like contents removed by pressure.

On the 7th March the patient complained of headache, and was ordered a blue pill every night for six nights, and a compound rhubarb pill every morning.

On the 24th the report states that she had felt her head better since taking the pills. The inside of lid being still vascular, was touched with the four-grain solution of nitrate of silver.

On the 30th some hardness still remained in the site of the tumor.

These cases afford an illustration of the two most frequent tumors met with in the eyelids.

The first occurs much more frequently in children than in adults. It is seated between the skin and the orbicularis palpebrarum. The integuments covering the tumor are of a white colour, or rather, are so thinned as to allow the white colour of the tumor to shine through. It presents a granulated appearance even before extirpation; and on being removed, is still more distinctly seen to be formed of numerous grains or smaller tumors, which are probably enlarged sebaceous glands. These are white and opaque, and the whole tumor is firm, and not apt to go into suppuration. Neglected, this sort of tumor sometimes reaches the size of a filbert. The integuments covering it are now apt to become ulcerated, and the mass is discharged entire, or in portions from time to time.

The second sort is known by the name *chalazion*. It occurs only in adults, and is almost uniformly connected with dyspepsia. It is seated between the orbicularis palpebrarum and the cartilage of the eyelid. The integuments over the tumor are elevated, and generally of a dark-red colour. The cartilage is thinned by the pressure apparently of the tumor; the conjunctiva is inflamed; and at length these textures give way, and a fungous growth appears on the inside of the lid. Extirpated, by a simple incision, either on the outside or on the inside of the lid, the incision being followed by pressure, which makes the morbid substance start from its seat, we find on examining the tumor, that it is of a light reddish colour, semi-transparent, soft, its surface smooth and uniform, and its consistence gelatinous. If it is not extirpated, it runs into suppuration, and while it does not fail to destroy the cartilage and the conjunctiva, as has already been mentioned, it points externally, and bursts like any other abscess.

Neither of these tumors is encysted.

Their external characters being so very distinctive, I was anxious to ascertain whether they differed much in chemical properties. Dr. Thomson was so kind as to examine them, and informed me that No. 1 possessed the characters of coagulated albumen; while No. 2, dissolving with great ease in acetic acid, and being thrown by prussiate of potash, seemed to consist of an imperfect fibrinous matter.

As to the extirpation of these tumors, a simple incision is all that is necessary, followed by pressure, till the mass is fairly dislodged. In the first variety, the incision is to be made through the integuments; in the second, in general, through the conjunctiva and cartilage.

Conjunctivitis Erysipelatosa.

Mary M'Donald, aged 20, was admitted at the Eye Infirmary, on the 1st March, 1832, eight days before which she had rigors, followed by headache and erysipelatous inflammation of the conjunctivæ, without any affection of the integuments. The conjunctivæ were of a pale red colour, and on one side the membrane hung in soft masses from between the eyelids. The tongue was white, and the patient complained of thirst. She had applied leeches to the temples, and taken a dose of sulphate of magnesia. She was inclined to attribute the affection of her eyes to exposure to the emanation from a solution of chloride of lime. She was ordered one grain of muriate of mercury in eight ounces of water, as a collyrium.

On the 2d the swelling of the conjunctivæ was much abated, and the eyes easier. The four-grain solution of nitrate of silver was applied to the inflamed surface.

On the 6th the swelling was gone, and the redness much less. The solution and collyrium were continued.

On the 10th she was ordered ℈j. of precipitated carbonate of iron thrice a-day; and on the 18th was dismissed cured.

Although erysipelatous conjunctivitis is minutely described by Beer, it is one of the rarest of the ophthalmiæ. We see, indeed, the conjunctiva reddened in erysipelas of the lids; but in the disease of which the above is an instance, the symptoms are confined to the conjunctiva, and the integuments are unaffected. It is easily discriminated from any other form of conjunctival inflammation.

Foreign Body adhering to the Cornea.

Daniel Newton, aged 14, from Paisley, applied at the Glasgow Eye Infirmary, on the 1st March, 1832, on account of considerable pain and inflammation of the left eye, which had continued for two months, notwithstanding the application of a salve, a sugar of lead poultice over the lids, leeches, and repeated blistering. There was a small semi-opaque elevation, running nearly in perpendicular direction, and occupying the centre of the cornea. Its figure being different from that of any ordinary pustule or speck, I touched it with the convex side of a small hook, when it separated from the cornea, and proved to be what, in Scotland, is known by the name of a meal-seed—that is, a fragment of the husk of a grain of oats, about the sixth of an inch in length, and the fortieth of an inch in breadth. The cornea, where the foreign substance had adhered, appeared slightly nebulous. He was ordered to bathe the eye frequently with warm water.

On the 8th the inflammation was completely gone, the cornea natural, and vision perfect.

Cases of this sort occur not unfrequently. Some time ago, a child of three months was brought to me by its mother, who said that one of the eyes had been inflamed for six weeks, and that a speck had grown on it. Different remedies had been tried, and a blister had been recommended. On examining the eye, I found one-half of the husk of a canary-seed adhering to the cornea, a little below the level of the pupil, simulating, to an inexperienced or careless observer, a speck or pustule. It was easily removed with the point of a tooth-pick.

A child of 4½ was brought to me from the country, with a black substance firmly adherent to the conjunctiva covering the sclerotica. It was supposed to be a piece of coal, and several attempts had been ineffectually made to remove it. Laying the child on its back, fixing its head between my knees, and steadily elevating the upper eyelid, I laid hold of the foreign substance with a pair of forceps. It came away, leaving the portion of conjunctiva which it had covered in an inflamed and fungous state. On examination, it proved to be half the husk of a hemp-seed, which had stuck to the eye by its concave surface, and had become blackened by imbibing moisture from the conjunctiva. It had remained for seven days in the situation in which I found it.

A child was brought to me with severe inflammation of one of its eyes, and puriform secretion from the conjunctiva. From under the edge of the upper lid there projected a black roundish body, which at first view I was afraid might be a protrusion of part of the iris through an ulcer of the cornea. The parents were of opinion that the eye was gone, and evidently laboured under some notion of the same kind as that which occurred to myself when I laid down the child to examine the exact state of disease. How great was my surprise when, on raising cautiously the upper eyelid, I found that this was a case, not of figurative myocephalon, but real. A common house-fly was fairly lodged, and had been so for eight days, between the eyeball and upper eyelid, its head only projecting in the manner described, and producing an appearance as if the eye were disorganized.

Ophthalmia Scrofulosa.—Adhesive Inflammation of the Cornea.

Isabella Fitzsimmons, aged 9, was admitted at the Glasgow Eye Infirmary, 3d August, 1831, with the following symptoms. Numerous pustules round the upper edge of the right cornea; considerable reticular inflammation of the conjunctiva; tongue white: thirst; is feverish, and tosses during the night. A solution of tartrate of antimony, in divided doses, was ordered as an emetic, and the four-grain solution of nitrate of silver was applied to the eye.

On the 5th the pustules were observed to be diminishing in size, and the redness was less.

On the 9th the symptoms still abated.

On the 12th the pustules were all but gone.

On the 17th a new pustule was observed at the lower edge of the cornea. She was ordered an ounce of sulphate of magnesia.

On the 19th there was general vascularity of the conjunctiva.

On the 21st a small ulcer was present on the centre of the cornea. She was ordered twelve grains of sulphate of quina in a twelve-ounce solution; a table-spoonful to be taken thrice a-day.

On the 2d September the inflammation was found to be increased, with an onyx at the lower edge of the cornea. Six leeches were applied to the right lids; a dose of calomel and jalap administered, and the quina augmented to ℥j. in twelve ounces.

Next day, a blister was applied behind the ear.

On the 8th the onyx was less.

On the 9th the quina was increased to ʒj.

On the 13th a considerable effusion of lymph was observed on the internal surface of the cornea, below the level of the ulcer. The eye was evidently in imminent danger. Extract of belladonna was smeared on the brow and upper lid. The quina was continued, but as this is a remedy of little, if any, power over adhesive inflammation, two grains of calomel, with the third of a grain of opium, were ordered at bed-time.

On the 15th the pupil was somewhat dilated, and the lymph on the internal surface of the cornea was less.

On the 16th the lymph was much diminished.

On the 18th it was all but gone. The ulcer was still deep, but smooth. Numerous red vessels were seen creeping over the lower edge of the cornea. The pupil still kept dilated. The remedies were continued as ordered on the 13th.

On the 22d the pupil was widely dilated, the lymph completely gone, the ulcer contracting, and the cornea free of red vessels. The belladonna was omitted.

On the 27th the calomel and opium were stopped.

On the 1st November a very small leucoma was the only remaining symptom.

This case affords a good example of the spread of inflammation from the investing membrane of the cornea to its proper substance, and from this to its lining membrane. It shews the danger of trusting to sulphate of quina in every circumstance of strumous ophthalmia, and the favourable influence of calomel and opium in adhesive inflammation of the cornea.

The term *corneitis* is generally limited to chronic or strumous inflammation of the external covering, or conjunctiva of the cornea. There are, however, many other varieties of inflammation of the cornea.

The external membrane is subject to puro-mucous and to phyctenular inflammation. It is also frequently the seat of ulcer, the ulceration sometimes spreading along the surface of the cornea without attacking the proper fibro-cartilaginous substance of this part of the eye. Vascular albugo is a peculiar variety of inflammation of the conjunctiva corneæ.

The proper substance of the cornea is often the seat of inflammatien, when neither the mucous membrane nor the lining membrane is at all affected. Inflammation in the cellular tissue, or (as some consider it) lamellar tissue of the cornea, frequently ends in onyx, which appears to be an effusion of pus or puro-lymph.

Inflammation of the lining membrane (considered by many as a serous texture, and by others as a layer of cartilage) is much more rare. It is met with, however, under several different forms. Sometimes, as in the above case, it assumes the adhesive form, and lymph is secreted in considerable quantity. In other cases the membrane presents those very peculiar opacities described by Mr. Wardrop, no other part of the eye being affected. In a third set of cases, we find signs of inflammation in the investing membrane of the iris, and in the anterior hemisphere of the crystalline capsule, with very numerous brownish spots on the internal surface of the cornea.

SUCCESSFUL TREATMENT OF CHOLERA.

[In further illustration of the treatment which has been adopted in the cases of cholera which have occurred at Cold-bath-Fields, we insert the following communication from Mr. Wakefield, the intelligent and highly-respectable practitioner who has the medical charge of the prisoners.]

Lansdowne-Place, Brunswick-Square,
April 25th, 1832.

Sir,—So much has already been written on the subject of cholera, that I should not now appear before the public but from a conviction that the facts which I am about to state, if generally known and properly authenticated, (which they can easily be) must be useful to those of the profession who, in future, may be called upon to treat this new, but most malignant disease.

The first case which I saw occurred on the 5th of this month, in the Prison at Cold-Bath-Fields. Three others quickly followed, and were immediately put under the common treatment: these four patients died, after a short illness, with all the symptoms of cholera distinctly marked.

Soon after the commencement of the disease, a number of the prisoners were attacked with marked symptoms of derangement in the gastric organs; and as all of these cases occurred in the infected part of the prison, it is more than probable from this, as well as the general appearance of the patients, that the diarrhœa, with which they were attacked, was the effect of the poison which produces cholera. From having seen similar cases in the commencement, transformed rapidly into a state of collapse, my conviction is, that every one of those patients were more or less in serious danger; and I believe also, that had they either been left to themselves, or improperly treated, the majority of these cases would have run into a state of collapse, perhaps in a few hours; indeed I have little doubt that the

one-half of them would have been lost under the practice which is generally adopted in the treatment of this disease.

Independently of the numerous cases where the individuals were labouring under the premonitory symptoms, I have now had twenty-five cases of decided cholera where the patients were in a state of collapse; and in justice to Dr. Stevens, who suggested the use of the saline remedies, as well as from a sense of what I owe to the public, I conceive it my duty to state, that after having seen both the old and the new treatment fairly put to the test, I am fully convinced that the saline practice is not only the most scientific, but decidedly the most successful that has yet been adopted for the cure of cholera; and from what I have seen, my conviction is, that if this treatment be fairly and extensively tried, the mortality from cholera will be greatly diminished. When used at an early period, it either prevents or arrests the progress of the fatal symptoms; and even where this treatment is not used until a later period of the disease, its effects are distinctly marked; and I may safely say that I have seen several most malignant cases recover from the state of collapse under the saline treatment, where the patients, I doubt not, would have died under any other practice.

We have now upwards of twelve hundred persons in this prison; and from the commencement of the disease up to this date, there have been nearly one hundred cases where individuals have been more or less evidently labouring under the influence of the cholera poison. Twenty-five of these assumed the malignant character of the disease, having the majority of the symptoms described in the printed document issued by Dr. Macann. Four of the first cases, as before observed, were treated in the common way, and every one of them died. All the others, however, were immediately put under the use of the saline practice, as recommended by Dr. Stevens, and out of the whole number who have been thus treated, we have only had three deaths from cholera, and two of these were cases of relapse. I may state, also, that within the last few days I have had one most malignant case in the New Prison at Clerkenwell, where the patient was in a state of complete collapse before I saw him. His extremities were cold; his pulse at the wrist was entirely gone; he had the cholera-voice, and his tongue was icy cold. This man, like those in the other prison, was immediately put under the saline treatment with the happiest effects, and I consider him now in a state of convalescence.—I am, sir,

Your humble servant,
H. Wakefield.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

A Treatise on the Diseases of the Heart and Great Vessels; comprising a new view of the Physiology of the Heart's Action, according to which the Physical Signs are explained. By J. Hope, M.D. Senior Physician to the St. Marylebone Infirmary. W. Kidd, 228, Regent-Street. 1832.

[Concluded from preceding volume, p. 814.]

In a preceding number we presented our readers with an analysis of the first portion of Dr. Hope's work. The second part, at which we have now arrived, treats of the inflammatory affections of the heart and great vessels, and is divided into three chapters, under the heads of Pericarditis, Carditis, and Arteritis, the last including affections of the inner membrane of the heart.

The means that we have hitherto possessed to aid us in distinguishing these inflammatory states, have been so vague and indefinite, that even such distinguished pathologists as Laennec and Corvisart have declared that they were ignorant of any signs by which their existence could be ascertained. Dr. Hope has taken great pains to investigate this subject, and has, as we conceive, succeeded in remedying the defect up to a certain point; so that in many, if not in most cases, an acute and cautious examiner may now be enabled to effect a diagnosis with considerable precision. The anatomical characters, the general as well as physical signs, and the medical treatment of these inflammatory affections, are discussed in these chapters in all their bearings, and with

a degree of clearness and sound judgment highly creditable to the author's industry and diligence. Our space compels us thus generally to pass over this part of the volume; but we hope we have said enough to shew, that it is not less worthy of being studied than any other portion to which we may more particularly direct our analytical inquiries. We cannot leave this chapter, however, without recommending to the more especial notice of the reader that part of it which treats of adhesions of the pericardium as a consequence of rheumatic or other inflammation, &c. a subject which he will find illustrated by new and original views, with a reference to cases, at the end of the volume, by which these views are supported and substantiated.

The third division of Dr. Hope's work, that treating of the organic affections of the heart and great vessels, next comes under our notice.

Having detailed the anatomical characters of hypertrophy of the heart, and explained the various forms and complications of this state of disease, as well as the manner and cause of its production, our author proceeds to illustrate its various pathological relations to and effects upon the general system. He commences his remarks on this head by exposing a fundamental error of Bertin, who supposed that hypertrophy never causes symptoms of a retarded circulation. Dr. Hope believes, with the majority of authors, that the dyspnœa, engorgement of the lips, and venous capillaries generally, the passive hemorrhagies, and the serous infiltrations that so constantly attend hypertrophy, exist as a direct consequence of the natural tonic power of the capillaries being overcome by the increased force and activity of the circulation under such circumstances. He shews, more prominently than we have hitherto been accustomed to regard it, the close connexion that exists between hypertrophy of the left ventricle and apoplexy, as cause and effect, on which he remarks, that he believes, with MM. Richerand and Bertin, "that hypertrophy forms a stronger predisposing cause to apoplexy than the apoplectic constitution itself; and that, in most instances, those persons who present the apoplectic constitution, in conjunction with symptoms of increased determination of the head, are at the same time affected with hypertrophy."—P. 209. Having demonstrated the influence of hypertrophy of the left ventricle of the brain, by the same train of argument he connects hypertrophy of the right ventricle with pulmonary congestion and hemorrhagies from the lungs.

We are then supplied with a systematic arrangement of the symptoms of hypertrophy of the heart: these are of two kinds, general and physical; "the former consisting of its effects on the functions of the economy at large; the latter comprising the impulse and sounds of the heart, and the resonance of the præcordial region on percussion."—213. The first class is supported by authority and Dr. Hope's own experience; and the latter justified and explained by a reference to those principles respecting the phenomena of the heart's action, which are amply discussed in the commencement of this work. By aid of these symptoms, as thus detailed and explained by Dr. Hope, the diagnosis of hypertrophy of the heart, in its various complications, appears to us to be placed in a situation less of conjecture, and more within scientific reach, than heretofore. According to his own experience, these two classes of signs, the general and the physical, "taken conjointly, render the diagnosis so easy, that a material error can scarcely be committed," whilst "taken separately, neither is sufficient to indicate disease of the heart with perfect certainty."

Dr. Hope believes "hypertrophy to be more susceptible of cure than any other organic affection of the heart," inasmuch "as it is easier to diminish the nutrition of the heart than to increase it, or to remove a valvular or other mechanical obstruction."—238. The extreme degree of antiphlogistic treatment recommended by Laennec, on the plan of Valsalva and Albertini, is objected to by Dr. Hope on the following grounds, expressed in his own words:—"My objections to the treatment are founded on the circumstance, that, though I have invariably found the greatest benefit to be derived from sparing abstraction of blood, at intervals of two or three weeks or more, I have constantly noticed that when, from the severity of the dyspnœa and palpitation in the advanced stages of the complaint, the practitioner was induced, or thought himself compelled, to resort to frequent bleedings at short intervals, the patient,

though perhaps temporarily relieved, progressively declined from that moment, the paroxysms recurring more frequently and with greater violence, until they eventually terminated in his destruction."—242. According to his own view, "the indications in the treatment of hypertrophy are, to diminish the quantity without materially deteriorating the quality of the blood, and to do this in such a manner as, without producing reaction, permanently to enfeeble the action of the heart and the energy of the circulation."—244. These indications seem to him to be best fulfilled as follows:—Bleeding to four, six, or eight ounces, every two, three, four, or six weeks, according to the patient's strength, so as to obviate the dyspnœa, and keep down the heart's impulse, &c. Cupping from the nape of the neck, if necessary. The diet to be spare and not stimulating, consisting chiefly of white meats and bland liquids in small quantity. Exercise should be very gentle, so as not to hurry the circulation. When depletion is not expedient, saline aperients, so as to procure three or four copious watery stools daily, for a week at a time; the bowels to be always kept lax by the same or analogous aperients; and not only when there was dropsy, but when there was none, Dr. Hope has seen decided advantage result from diuretics, such as cream of tartar, broom-tea, &c. When there is decided dropsy, more efficient diuretics are called for, and, in the event of their failure, the hydragogue cathartics are of great avail. Derangements of the stomach and biliary secretions should never be overlooked in hypertrophy; they must be treated on common principles: and nervous and other troublesome symptoms, as they may arise, must be met by the usual remedies.

"The above," says our author, "and indeed every other mode of treatment, is unavailing if not *steadily* pursued; and it must be pursued for one, two, three, or more years. Thus employed, I have found it effect cures in a considerable number of instances, some of which were advanced even to the second degree. In the first degree, especially before the period of puberty, this fortunate event is often obtained, although bleeding be resorted to only at long intervals, as from six weeks to three months."—247.

On the subject of Dilatation of the heart, we again find Dr. Hope differing in opinion from M. Bertin, by maintaining, contrary to the views of that eminent pathologist, that dilatation, in the great majority of cases, is as justly entitled to the rank of a primitive disease as hypertrophy, "and is not dependent as a consequence on the pressure of the circulation, but on the manner in which the heart resists that pressure."—260-1. We refer to the work itself for further elucidation of this affection, which the reader will find fully discussed, and amply deserving his most attentive perusal.

We pass over several less frequent affections of the heart, such as softening, induration, atrophy, &c. in order to afford space for a few observations on some of the remaining chapters of the work.

On the subject of Valvular disease, we are furnished with a practical application of those principles of the heart's action, which, as we have seen, Dr. Hope was the first to propose. According to the author's views, "the exciting causes of valvular disease are, 1st, over tension of the valves by the force of the circulation; and 2d, inflammation, generally of the chronic kind"—(365); and there being no positive signs by which either of these may be detected before they have terminated in organic mischief, the only indications of treatment in this disease that remain are,—"to prevent its further increase; to counteract its tendency to induce hypertrophy and dilatation; and to relieve the symptoms of an obstructed circulation."—366.

In fulfilment of these indications, Dr. Hope advises depletion to a moderate extent, (not after the fashion of Valsalva and Albertini); spare and temperate diet; tranquillity, as much as may be, of body and mind; and attention to the state of the digestive organs and alimentary canal. By these means, if we do not cure the existing disease, we yet prevent its increase; and as the symptoms of valvular disease are seldom severe till hypertrophy, or dilatation, or both, supervene, the life of an individual affected by it may be frequently prolonged to an advanced old age, with great comfort to himself, even though his symptoms may have commenced very early in life. But, "on the other hand, if precautionary measures be neglected, and hypertrophy or dilatation

superinduced, there is no organic disease of the heart, except adhesion of the pericardium, which tends more rapidly to a fatal termination. Hence the great importance of detecting and attending to disease of the valves in its earliest stage."—367.

In this chapter on valvular disease is included a section on Cardiac Asthma. Asthma, as dependent on diseased heart, has not hitherto attracted much notice, many authorities being entirely silent on this head. Dr. Hope, however, has here shewn the very important connexion that subsists between them; and his remarks, both in explanation of the phenomena of asthma and on the subject of its treatment, are in a great measure new, and well worthy of careful attention.

It was a remark of Laennec, that "of all the severe lesions of the thoracic organs, three alone remain without pathognomonic signs to a practitioner expert in auscultation and percussion; namely, aneurism of the aorta, pericarditis, and polypi in the heart previous to death." We have already alluded to the improvement that Dr. Hope's investigations have effected in the diagnosis of one of these three states—pericarditis; and it remains that we should say a few words on the chapter that he has devoted to another of them—aneurism of the aorta.

This chapter is the substance, with considerable additions, of a series of essays on the subject, that were published in this journal in August 1829, and is, in our estimation, one of the most minutely detailed and most practical in the whole book. It would require more space than we have at our disposal at present, to enter fully into the views which our author takes of this formidable affection; we must, therefore, be content to refer our readers to the volume of the Gazette in which they will find the original essays.

The fourth part of the work, that treating of the Nervous Affections of the heart, comprises within it a brief sketch of "Neuralgia of the heart, or angina pectoris"—"Palpitation, spasm, or convulsion of the organ" — and "Syncope;" whilst several affections, which are not reducible to any of the preceding heads, are placed together in Part V. under the title of "Miscellaneous Affections:" these consist of "Polypus of the heart"—"Displacement" — "Hydro-pericardium" — and "Pneumo-pericardium." The sixth and concluding part contains an account of about thirty cases; these, though comparatively "few in number, furnish well-characterized exemplifications of all the ordinary and many of the more rare diseases of the heart, also of the general history and signs given in the antecedent parts of the work."

But for the great press of matter at the present moment, we should have been well pleased to have made our analysis more complete, but we find that it is in vain to attempt much beyond an indication of the contents of the volume,—contents, as we have already said, which will amply repay their diligent perusal.

MEDICAL GAZETTE.

Saturday, April 28, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

PRESENT ASPECT OF THE ANATOMY BILL.

It will form, we doubt not, a curious subject for a future generation of inquirers, to find out the possible motives of the cautious and reluctant legislation with which one of the greatest necessities that bears on society at the present day is provided for; and to note how much crime—even of a hitherto unheard of description—must be perpetrated before people will be convinced of the propriety of doing that opportunely, which must be done eventually at any rate. We know of no parallel for the conduct of our legislators in this matter, except such as we find in their fatally lingering amendments of the old Game Laws. Some sixteen or eighteen years ago an active and able magistrate could thus write:—"The extent and progress of the evil cannot be conceived by those who are not con-

versant with the lower ranks of the country villages. From extensive observation and inquiry, I believe in my conscience, that three-fourths of the crimes which bring so many poor men to the gallows, have their origin in the habits necessarily introduced by the almost irresistible temptations held out in consequence of the prohibitions of the Game Laws, and a nightly breach of their enactments;" and he adds, that the wonder was not that so many were corrupted, but that so many escaped the temptation. We think that, with a very few changes, the language of Mr. Weyland might be applied to the laws affecting anatomy; for where is the exaggeration in saying, with respect to the latter, that "The extent and progress of the evil cannot be conceived by those who are not conversant with the lower ranks throughout the country, and especially in our great towns? From extensive inquiry (might we add) we are induced to believe, that no inconsiderable portion of the mortality which of late years has affected the lower classes, and much of the crimes which have brought many wretches to the gallows, have had their origin in the circumstances necessarily arising from the unjust, absurd, and contradictory state of the Statute-Book regarding anatomy; and the wonder is, not that so much mischief has been done, but that so much has been escaped." Sir Robert Peel, whose profound acquaintance with the state of the criminal laws of the country entitle him to every attention, has gone even farther than this, and has not hesitated, with as much candour as boldness, openly to declare, that those horrible practices of exhumation and burking, so disgraceful to Great Britain in the eyes of all foreign nations, have resulted *from the refusal of Parliament to legalize the sale of bodies after death.* But those horrible practices are not all that have resulted from the unwise refusal of Parliament to legislate, though their enormity has evidently occasioned some evils of a less prominent, but not less extensively injurious, character, to be rather too much overlooked. In laying an effectual injunction on the study of practical anatomy, or so confining it as to be accessible only to comparatively few, the lower classes have been deprived of that saving help in their distresses which would have rescued numbers of them from untimely graves. It was not merely science—the most practically useful of sciences—that was checked by this almost unaccountable error of conduct on the part of our legislators, but the gross injustice has been committed of denying to the poor, especially to those at a distance from the great towns, that efficient medical skill which has been available only to those in more competent circumstances; and it is perfectly frightful to think how much silent injury may have been inflicted in this way.

By the measure, however, which is now before parliament, and which is to have its third reading after the recess, these foul stains upon our national code will, we hope, for ever be expunged. We have spoken freely of this Bill on former occasions, and pointed out where we thought it defective, and in what respects it might be materially improved; and, upon examining the changes which it has recently undergone in its progress through committee, it is with no small gratification that we notice several of those alterations which we had suggested. In particular, a *licensing* clause has been introduced—a step, of the propriety of which we have never seen any good reason for holding a different opinion. The license, as we understand it, is to cost nothing, and is to be procurable from the Home Office, by an application backed by the signature of two local magistrates. What possible objection can be made to this ar-

rangement, we should be glad to know? We are aware that there are some absurd people who preach up strange conceits of "free trade," and the advantages of "competition;" but what respectable member of the profession will object to being recognized by the government which grants him the power—the unprecedented power in this country—of pursuing freely his anatomical researches? And surely if the government are willing to take cognizance of the practice of anatomy, as it is henceforth to be carried on, not clandestinely, but with the same freedom as any other branch of science—and at the same time to regulate the mode in which anatomists are to be supplied with subjects; and further, if they are to do their duty in securing the public against the future perpetration of those horrid crimes which have so naturally filled the community with alarm—we say that they have every right to satisfy themselves as to the persons in whom they would repose such an important trust. Without licenses, we confess we cannot see how the machinery of *legalized* anatomy can go on.

One of our correspondents (the distinguished Professor of Anatomy in the University of Dublin), while he allows due praise to the zeal and ability of Mr. Warburton, thinks that the measure now before parliament is a very imperfect one, and that by the time it receives the sanction of the Lords, it will deserve to be entitled "an act for rendering the practice of anatomy difficult in England and Ireland, and impossible in Scotland." The imperfection of the Bill to a certain extent is indisputable, and to be ascribed only to what Dr. Macartney justly assigns as the true cause—the deficient information of the age; the want, in short, of a certain modicum of common sense and good temper on the part of the public to allow of more perfect legislation on the subject: but we should hope that the end of the Bill will not be so unlucky as the Doctor supposes. There is possibly much truth in his observation respecting Scotland; for there can be no doubt but that, under the licensing and inspecting system, in addition to the strong prejudices of the people of that country against meddling with the dead for anatomical purposes, the difficulties of supplying the schools there, will be very great indeed, most probably insuperable, in consequence of the extension of the Bill to Ireland. This extension to the sister-country, by introducing there the same system of licensing and inspecting, will cut off the exportation supplies from North Britain; a contingency so extremely likely, that we are induced to predict that the first proofs of the imperfection of the present Bill will certainly come to us from Scotland.

But for England and Ireland we have no fears. There may be some repugnance on the part of executors at first, some qualms on the part of those having "lawful custody" of the body, and perhaps some scruples about the nature of the equivalent proposed; but that will soon be better understood—"*sunt verba et voces, &c.;*" and things will go on smoothly after a time, as they have long done in France and in other places. In Ireland we doubt the supply will not be considerably increased: at all events, it will be less precarious—less dependent on the good will and pleasure of the miscreants who haunt the Hospital-fields—and less impeded by foreign exportation. Why the same system should not apply to both countries, we have never been able to see, and we will venture to assert that *no* valid reason can be assigned: the Dublin teachers will not surely put forth a selfish one.

A word on the "*traffic*" ere we conclude. Whose affair is it?—that of the

buyers or the sellers? The sources of supply under the new Bill will be abundant, and with a regulated system, the prices cannot be extravagant. It would be a very desirable thing, to be sure, to get subjects as well as licenses for nothing: but if anatomists cannot procure bodies without paying a reasonable price to the sellers thereof, they have perhaps no great grievance of which to complain after all; and as to the immorality or heinousness of *trafficking* about the matter, it seems to us to be an affair that should be allowed to rest purely with the consciences of the sellers.

ANATOMY BILL—EXTENSION TO IRELAND.

To the Editor of the London Medical Gazette.

Sir,

In the report of Mr. Warburton's speech on the Anatomy bill, published in the last Mirror of Parliament, he has been made to say, that I "had come over to London, a few weeks ago, at the head of the deputation from the Irish College of Surgeons, for the purpose of recommending the extension of the Anatomy bill to Ireland, but that he had heard, since my return to Dublin, that I had been induced to alter my opinion." How or with whom such a curious mistake could have arisen, I cannot imagine. I was not in London. In place of having anything to do with the deputation from the Dublin College of Surgeons, the objects of that mission were carefully concealed from me. I have not changed my opinion of the Bill, although I believe most, if not all, of the anatomical teachers in Dublin now repent their having proposed to extend its operation to Ireland. I always thought it a very imperfect measure, and one which, with the addition of such clauses as it would probably receive in its progress through parliament, might become injurious to the interests of anatomy. These anticipations have been fully realized, and I expect by the time that the bill has become the law of the land, it will deserve to be entitled,—an Act for making the practice of Anatomy difficult in England and Ireland, and impossible in Scotland.

It would have been desirable that no legislation had been attempted on this subject until the public should have been taught to speak on the practice of anatomy and of the medical profession with something like common sense; to accomplish which, nothing more would be necessary than the strict enforcement of the regulations by the different medical corporations regarding education, and a law to abolish all quacks and unlicensed practitioners. The public would then be compelled to provide the means of medical education, or go without medical aid, and there is no doubt which they would adopt.

I regret that Mr. Warburton, whose zeal and patience deserve the highest commendation, should have failed in carrying a more efficient measure.

You will oblige me by correcting, in the Medical Gazette, the error in the published debate, in which my name has been introduced.

I remain yours faithfully,

J. Macartney.

April 21, 1832.
Trinity College, Dublin.

[In our abridged report of the proceedings, in our last number, we omitted that part of Mr. Warburton's speech in which Dr. Macartney's name was mentioned, presuming that it could scarcely be correct.—Ed. Gaz.]

ANATOMY BILL—LICENSING SYSTEM.

To the Editor of the London Medical Gazette.

Sir,

The question on which I have ventured to differ from your valued correspondent Mr. Travers, bears too interesting and important a character at the present moment to be hastily dispensed with, believing that the fairest and fullest examination of its bearings will go far to disarm the public of undue prejudices, and insure a more valuable legislative measure in behalf of anatomy.

I perceive, sir, that no change has been wrought in Mr. Travers's mind by any reasoning hitherto employed, and he declines all further discussion, by saying "I have now done with the subject."

Were I silent at this juncture, it might be inferred either that his last letter has converted me to his opinions, or that its matter was completely unanswerable: I therefore request your insertion of this

rejoinder, to shew that I am not brought over to his views, and that I do not think his logic so deep as to defy all refutation.

On reading Mr. Travers's remarks about protecting the public from the hand of the assassin, and of the uneducated surgeon—about unrestricted and unlimited anatomical pursuits being incompatible with public safety — one would really suppose that the system of *Burking* had been mainly promoted by country dissection, since no restraint or limit has hitherto been set upon its practice (save the unpleasantness of dissecting, as it were, by stealth, and difficulty of obtaining bodies.) But what, I would simply ask, is this:—Why the great schools alone have been detected as recipients for burked bodies, and hence have in a more especial manner excited suspicion, and demanded the eye of inspectors?

I impugn not individuals, neither do I seek to disparage schools of anatomy in any form; but I do contend, that to deprecate and write down private dissection, against which nothing has been proved to favour burking, and to extol and alone license public schools, which have indirectly fostered it, is, to say the least of it, a weak and futile proceeding.

It is not the mere dissection of a body, wherever performed, which has called into existence the nefarious system of burking, and the unhallowed practice of robbing graves (both of which naturally engender popular prejudice and irritation), but it is to the *traffic* in human flesh—to the high price of an article which ought never to be made marketable, and for which there exists no legitimate source of supply, that we are to refer such crimes. Do away altogether with this odious trade; legalize dissection under certain wholesome restrictions; appropriate all unclaimed bodies to meet the demand; and both exhumation and burking will henceforward be inevitably extinguished.

Mr. Travers has insinuated that his opponents advocate the pursuit of anatomy without restraint or limit. My letter does not warrant this inference; neither, as far as I remember, does Mr. Todd's; and I now disavow any such wild opinion. True, I would not limit the practice of dissection to schools only; by no means; for I perceive neither propriety, nor equity, nor necessity, in permitting teachers of anatomy and their pupils to dissect, to the exclusion of all others; yet I would not sanction it beyond the pale of regularly-educated medical men.

Again, as regards *restraint*, I am by no means inimical to salutary checks, such as (to use Mr. Travers's own expression) the "intervention of a formal legitimate certificate, and a special register of it by a responsible officer." This certificate, however, is not to be doled out solely to the teachers of schools, but to every authorized practitioner throughout the kingdom; and, in order to render the working of this machinery both easy and effective, let the magistrates, who are ever at hand in all parts, grant such certificates, and be fully empowered to inspect, either personally or by deputy, any place where dissection is carried on.

Under such an arrangement no medical person need entertain the least fear from popular intrusion and insult, because he would be acting under legal authority, and would receive legal protection, whilst the bodies for dissection being *in toto* unclaimed, those feelings of affection which arise from the ties of consanguinity would never be excited; and my firm conviction is, that in a very short time not the least molestation would be even *offered* to the practice of anatomy wherever performed, provided all proper decorum and respect for the dead were observed, and likewise that an interment of the remains, with all due funereal solemnities, were enforced.

Mr. Travers still cherishes the belief that "not one in five hundred would dissect at home, were every facility granted." This being the settled conviction of his mind, why has he troubled himself at all about the country practitioners?—why exerted his pen so strenuously against granting them facilities, when he firmly believes that they would not avail themselves of them, and when (supposing his opinion right) it must be quite a matter of indifference whether their means of dissection be facilitated or impeded by legal enactments? Why aim at uprooting all private dissection, and affixing licenses only to certain schools? Being, however, of a totally opposite opinion—thinking that practitioners would gladly embrace the opportunity of dissecting when required, if facilities and security from molestation were granted — I shall continue to advocate a measure to this effect. If Mr. Travers be sincere in

the expression, that he should "hail the day with pleasure, when every principal market town in the kingdom had its anatomical as well as its grammar school," he surely cannot see any impropriety, or danger to the public weal, in dissection being extended a little farther under similar sanitary restrictions.

He has accused his opponents of overlooking surgery altogether, "in their zeal for anatomy." This surely must be an oversight; and if he will but search again, he may easily discover many positive expressions relative to the cultivation of surgery in conjunction with anatomy, equally positive with the hard epithets (as he chooses to call them) monopolizing, &c. which he did not fail to discover. Who could ever dream of defending the practice of anatomy, without any reference to surgery and other branches of the healing art, seeing that without the existence of the latter the former would be quite unnecessary? And what surgeon taking in a body for improvement, would omit to make operative surgery a principal part of his occupation? And as to driving the practitioner to school again, whenever he finds his anatomical and surgical knowledge grow somewhat deficient, there never was broached a more preposterous design; for surely after he has been once grounded in his profession, he must be a very dunce not to be able to preserve that knowledge in his own private apartments, with book, body, and scalpel, without requiring a young demonstrator once more at his elbow, to say nothing of the injustice of compelling him, by fettering laws, to resort to some public school whilst engaged in practice.

It appears that professors of anatomy may be allowed to revel in an abundance of dead bodies, whilst the private practitioner dares not touch one, save and except in the shape of a few postmortem cases, which are now considered an abundant supply for *him*, and quite equal to all his wants; for Mr. Travers exclaims, that "if their anatomy be at fault, and they are anxious to refresh it, do not postmortem inspections afford excellent opportunities?" I feel surprised at a gentleman of Mr. Travers's professional information adducing such arguments. He surely does not require to be told that these postmortem cases are had recourse to purely for pathological information; that some relative of the deceased, or a deputy, is for the most part present, watching every motion, and therefore that time, on these occasions, does not suffice for minute anatomical investigations, and that operative surgery is here quite out of the question.

I maintain, therefore, that these occasions are wholly inadequate to the desired end, and that to introduce it as a succedaneum for dissection, is only diverting the public mind with trifles, and wandering from the main question, which is plainly and briefly this:—Whether, under proper restrictions, an act of parliament, which shall extend the power of dissection to all regularly educated men, would not be infinitely more advantageous to the community at large, than the confining that power to a number insignificantly small, compared with the whole body of the profession?

I have contended that it would, and shall ever be ready to support that opinion; and remain, sir,

Your obliged obedient servant,

JOHN WAYTE.

Lynn, March, 1832.

[Dr. Wayte's letter was received some weeks ago, but we delayed its insertion, as we intended to take the earliest opportunity which the progress of the Bill afforded to bring the subject prominently before our readers: the same consideration led to the postponement of Mr. Robbs' letter, which follows.—ED. GAZ].

ANATOMY BILL—EFFECT ON COUNTRY PRACTITIONERS.

To the Editor of the London Medical Gazette.

SIR,

I CANNOT refrain from again addressing you on the subject of the "Anatomy Bill," in consequence of Mr. Travers, in his last communication, attempting to give an explanation for his former interference on that subject. In the first place, Mr. Travers holds forth that two things are necessary for the legislator to guard against; "first, to protect the public from the hand of the assassin; secondly, from that of the uneducated practitioner of surgery." Now, sir, for the purpose of guarding against the first evil, we must have a bountiful supply of "subjects," and

for that end we must have recourse to the different hospitals, gaols, poor-houses, &c. for their unclaimed dead; and there ought to be a provision, compelling governors and masters of the above institutions to give up their unclaimed dead, three days after death, for dissection, unless previously removed by a relation; the claimant first producing his college diploma before a magistrate, and then a certificate signed by the same magistrate, and two legally qualified practitioners of surgery residing in the same district, certifying the cause of death, &c. Were this done, I have no hesitation in stating my belief that there would be no lack of "subjects," and at the same time the public would be guarded against "burkers," while the pursuits of anatomy would be without restraint or limit.

With reference to the second obstacle, which Mr. Travers appears to lay considerable stress on, I would reply by saying, I think our diplomas are the tests by which regularly-educated men may be distinguished from empirics. But as Mr. Travers appears so closely wedded to the licensing system, and employment of inspectors, which he contends are absolutely required for the public safety, and yet as he has the honesty to acknowledge a compromise necessary, I will make another proposition—namely, a provision making legal the transfer of dead bodies from the "licensed schools," to members of the Royal College of Surgeons, provided always such bodies, or parts of bodies, be accompanied with a certificate from one or more of the inspectors, the certificate being a license for legally qualified surgeons; so that, by such a provision, the country surgeon would be enabled to keep pace with his more fortunate brethren, who have the advantage of repeating surgical operations on the dead in licensed schools. With respect to the insults which medical men would subject themselves to, merely for keeping up a thorough knowledge of their profession, I look upon such proofs of public displeasure as perfectly imaginary; for during the past week I have myself instituted two post-mortem examinations, and instead of receiving insults from the relations of the deceased, I feel that I have gained additional confidence and respect. And as for the opinion which Mr. Travers so strenuously maintains, that not five out of five hundred would dissect, I am aware, from my general knowledge of country practitioners, that many possess but little or no knowledge of anatomy, and are so alarmed at the idea of an operation, that they immediately send off to the nearest market-town, for a surgeon who is known to be capable of the undertaking; wherefore it is evident that those operating surgeons residing in market towns, such as Grantham, Stamford, Sleaford, &c. ought to possess the means of refreshing their anatomy and surgery; for that operations are rare, and consequently that we overlook our surgery in our zeal for anatomy, are positions which I altogether deny; for not only is minute anatomy learned and maintained by repeated dissection, but also the best mode, and the most dextrous, of performing operations, are acquired by repeating them frequently on the dead. I will beg leave to add, that, during the twelve months I filled the office of "dresser" at Guy's Hospital, I had the opportunity of witnessing many and various operations, and can testify that, when I had previously performed them on the dead subject in the dissecting-room, I could with the greatest facility follow the operator through every stage even of the most complicated operation; therefore I do contend, that anatomy and surgery are inseparable, for no man can be a surgeon without being an anatomist; and I will maintain, that post-mortem examinations are totally insufficient for keeping up a thorough knowledge of anatomy and surgery. In conclusion, I would simply ask Mr. Travers whether, during the earlier part of his career, he would have been satisfied with deriving his information only from post-mortem examinations?

I remain, sir,
Your obedient servant,
WILLIAM ROBBS,
M.R.C.S.

Grantham, March 10, 1832.

HUFELAND ON CHOLERA*.

Academy of Sciences, Paris,
April 16, 1832.

M. MOREAU DE JONNES has addressed the following letter to the President of the Academy.

The learned Professor Hufeland, of Berlin, has just published a Dissertation upon Cholera, and concludes, from his observation of that disease during its late irruption in Prussia—

* See also an original paper by M. Hufeland, in our last volume (ix.) p. 860.

1st. That the oriental cholera is a new disease.

2dly. That it is a disease unknown in Europe, and the same with that which has been observed in India, where it originated.

3dly. That the cause of this disease is a peculiar principle, the same that has produced and still produces cholera in Bengal.

4thly. That the cholera is propagated by importation and by infection, in the most extended sense of these words.

5thly. That with regard to the transmissibility of cholera, it belongs to the class of contagious diseases.

Appreciating as I ought the labours of the celebrated Professor of Berlin, I must take the liberty of remarking that his conclusions are precisely the same as those of several memoirs which I have read to the Academy; the first of which dates as far back as ten years.

These memoirs have been published in several periodicals, and are analysed in the Annual Reports of the Academy. One of these memoirs was read by me, at the sitting of the 16th April, 1821; and another, which furnished matter for a report to the supreme Council of Health, was read on the 11th December, 1823. It is this last document which has been accused of having determined the Russian government to take precautions, with the view of endeavouring to prevent and stop the irruption of cholera in 1830.

Being aware, from this example, of the possibility that the researches of Professor Hufeland may bring upon him more censure than pra se, I think it my duty to claim priority as to his conclusions, and to attach myself to their evil fortune. I felicitate myself, however, upon being confirmed by the authority of so able and experienced a practitioner in the conclusions which I had the honour of being the first to announce to the Academy, the melancholy truth of which it is now unfortunately much easier to put to the test of proof than at the time alluded to.

(Signed) A. Moreau de Jonnes.

LATEST ACCOUNTS OF CHOLERA IN PARIS.

We have received letters from Paris to the 23d instant; in one of which, from a distinguished physician resident in that city, we are assured that, from the irruption of cholera up to the above date, *twenty thousand* persons are ascertained to have died of the disease in the French capital alone. Although the violence of the epidemic has somewhat abated, the numbers attacked still continue very large; and nothing like unanimity of opinion as to the most efficient method of treatment has yet been established. Numerous autopsies have shewn the ganglionic nerves in a perfectly healthy condition; and the reputation of M. Delpech has suffered some diminution in consequence. In addition to the former list of medical men who had been attacked, we may mention MM. d'Halancourt, Lefevre, and Laugier; Angouard, Deslandes, Boucher, Dugua, de Villers, and our countryman Sir J. Chermside. The three first have sunk under the disease, the others are doing well; the last, we are happy to say, has already recovered.

REPORT OF CHOLERA IN GREAT BRITAIN, UP TO FRIDAY, APRIL 27, 1832.

New cases in London since our last report	55
Deaths	33
Total number of cases in London since the commencement of the disease	2532
Deaths	1334
New cases in other parts of Great Britain since our last report	557
Deaths	217
Total number of cases throughout Great Britain since the commencement of the disease	11328
Deaths	4529

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

April 1832.	Thermometer.		Barometer.	
Thursday . 12	from 33	to 52	29 95	to 29·91
Friday. . . 13	36	58	29·82	29·93
Saturday . 14	32	59	29·98	30·09
Sunday . . 15	39	61	30·05	30·01
Monday . . 16	39	61	29 98	Stat.
Tuesday . 17	31	60	30·00	29·93
Wednesday 18	34	53	29 79	29 49

Prevailing wind N. E.

Except the 13th, 14th, and 17th, generally cloudy; rain at times.

Rain fallen ·225 of an inch.

Thursday . 19	from 41	to 54	29 56	to 29·81
Friday. . . 20	38	54	29·69	29·63
Saturday . 21	35	57	30·01	30·11
Sunday . . 22	31	57	30·09	30·00
Monday . . 23	36	63	29.81	29·68
Tuesday . 24	35	53	29·68	29.75
Wednesday 25	37	49	29·80	29·75

Prevailing winds N.W. and S.W.

Except the 21st, 22d, and 23d, generally cloudy; with frequent rain.

Rain fallen ·3 of an inch.

Charles Henry Adams.

BOOKS RECEIVED FOR REVIEW.

Stafford on the Injuries, the Diseases, and the Distortions of the Spine.

King on Lithotrity and Lithotomy.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 5, 1832.

LECTURES
ON
THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Part I.—Lecture XXX.

Continued Fever—the subject of Contagion continued.

I was mentioning, gentlemen, at the conclusion of the last lecture, some singular instances of the denial of what it would appear almost impossible that any one would think of denying. I mentioned some instances of the denial of the contagion of hydrophobia, of plague, and of syphilis; but the hour having elapsed, we were obliged to leave off in the midst of these illustrations.

Denial of the Existence of Contagion.—In regard to plague there is a gentleman, Dr. Maclean, one of the number of those who denied that plague was at all contagious; and he went so far as to explain why it or any other disease was ever supposed to be contagious. He says that the idea of contagion is merely a comparatively modern invention; that it was neither more nor less than a popish trick of 1547. He says, and would have us believe, that one of the popes, Pope Paul the Third, in 1547, after the death of our King Henry the Eighth, of blessed memory, when the holy fathers were assembled at Trent, finding he could not manage them—that he could not get them to vote his own way, broke them up by setting forth the idea of contagion:—that he stated there was a contagious disease in the town—a disease that might be caught from those affected with it, and thus excited so much fear among the holy fathers, that they broke up their council and all withdrew to Bologna. The celebrated council of Trent was dissolved; the holy men were so frightened at this invention of his Popeship, that away they all went scampering to Bologna.

If you look into Boccacio's Decamerone, (the first part of which is not improper for me, a grave professor, to beg you to read, as it gives only an account of the plague)—you will find it was published in 1348, and contains a description of the plague at Florence, and that the stories of the Decamerone were told by a party who retired from Florence some distance into the country during the plague—were told by some young gentlemen and ladies, who amused themselves by relating them. You will find Boccacio say, that the disease spread from the diseased to the healthy, as flames spread to an unctuous substance. Not only speaking and associating with each other produced it, but the clothes, or any thing else that belonged to persons who laboured under the plague, communicated it. He says, that even brutes got the disease by touching the clothes of those who died of the plague. He says, "I saw two pigs grubbing about the clothes of a man who died of the plague; they then staggered and fell down dead."—*Amendui sopra gli mal tirati stracci morti caddero in terra.* Now this was actually published in 1348, and yet a man would seriously have us believe that the idea of contagion was invented in 1547.

However, some are more hardy than this; they will deny almost all sorts of contagion. There is a M. Lassis, of Paris, who denies all infections and contagions together, excepting measles and lues venerea. But this is by no means a modern folly. I imagine that some of these persons have hoped to signalise themselves by advancing something original, when they denied this or the other contagion: and particularly those who deny many contagions, or nearly all, must have thirsted for fame indeed. But they are too late; they are merely dull imitators, for many persons among the older writers have denied sometimes one contagion, and sometimes another. You will find that Gadbury, the astrologer, says that the

plague is *not more infectious* than the *small pox*, scurvy, pleuritis, ague, and gout. He published in 1665, and his book is called London's Delivery Predicted.

Now there can be no doubt that great mistakes have been committed on the opposite side, as to the contagiousness of particular diseases. You have seen that Dr. Fordyce and Dr. Cleghorn, both able men, believed that ague was contagious. When speaking of scurvy, I mentioned that some contended formerly that that disease was contagious. We now know to a certainty, that neither ague nor scurvy is contagious. Again, on the other hand, some diseases were not supposed to be contagious, which we now know to a certainty to be so. It is by no means certain from the writings of Sydenham, that he was aware that small pox or scarlatina were contagious, or even perhaps measles. He may occasionally write as if they were, but he attributes them to other causes than contagion; and it is by no means clear that he was much impressed with the fact. But you will find other writers referring small pox, and these other diseases, to all sorts of causes, instead of contagion; such as the faults of parents before their children were born or procreated; any thing, in short, rather than contagion. Mistakes, it must be allowed, have been committed on both sides the question. Diseases not contagious were formerly supposed to be so, and diseases known now to be contagious beyond all doubt, were at different periods formerly not imagined to be of that nature; and therefore we ought to listen with some degree of patience to those who deny that this or that disease is contagious.

The Contagion of Typhus Fever considered.—Now typhus fever is a disease which some deny to be contagious, and I myself willingly confess, that I never saw an instance in which it shewed that character; but notwithstanding this, I cannot but believe that it is frequently a contagious disease. Such it has proved itself at the Fever Hospital. I do not recollect an instance of its spreading, notwithstanding I have been now so many years physician to St. Thomas's Hospital, where, of course, there is always a great deal of fever; but it is to be remembered, that the majority of cases of fever which we have there are generally by no means violent, compared with what I am told they are at such an institution as the Fever Hospital. We lose an exceedingly small number of cases of fever, because persons are brought in either in the early stage, when the disease is easily treated and cured, or because they are not desperate cases that people in general think of bringing to us. I hear, however, that to the Fever Hospital, which is the receptacle for this disease only, patients are taken as a matter of course when they have fever, whether moderately or desperately, and therefore many in whom there is no hope that the disease will be cured. Our greater success, therefore, is owing, in a great measure, to the comparative mildness of the disease we are called to treat. Now this may be one reason why the disease does not appear contagious with us; the emanations from the body are not of that extremely powerful kind in which they frequently are at the Fever Hospital; and it is not *all*, but a very small number, who labour under the disease. Again, in the next place, we have extreme ventilation, extreme cleanliness; and this is another reason why a mild disease at any rate will not spread. I have no doubt there is the same cleanliness and ventilation at the Fever Hospital; but then this must be infinitely less efficient, because *all* the patients have fever, whereas with us generally but one or two, and rarely more than three patients with fever are in the same ward, large as our wards are. There can be no doubt that if typhus fever be very bad indeed, now and then persons standing near the patient, even if there be good ventilation, may contract the disease.

But with regard to the arguments in favour of typhus fever not being contagious, it is to be remembered that it is a disease the contagion of which may be most easily rendered quite inert by the dilution of ventilation, unless in some very *very* bad instances, on the one hand, or where there is a great predisposition to it on the other. In the next place it is a contagion, which is allowed by those who stand up for its contagiousness, to be resisted exceedingly if the body be in good health, and the person be in good spirits. But it appears to me, that there are instances without end of a person catching typhus fever from others. It is true, I have never met with one of these cases, yet those who see far more of the disease than I do—though I see quite as much as I wish, perhaps about fifty cases in a year—mention instances innumerable; and you will find cases without end recorded in books, such as I cannot pretend to controvert. I understand, in regard to the Fever Hospital at Battle-Bridge, that every medical officer there, physicians, surgeons, and apothecary, have had the typhus fever, and some more than once; that every nurse, every matron, every laundress, every housemaid, and, in short, every body that ever had any thing to do with the place, have all had it, and many officers have died. Yet nothing of the kind has happened at the Small-Pox Hospital, which is on the very same spot. And this is very important, as clearing up the point whether all these persons have had it through the emanations of the patients there, or through the situation; because I myself see every day persons brought to St. Thomas's with typhus fever, from the midst of others who have the disease; but then I never consider that any proof of con-

tagion. The disease *may* have arisen in my cases from contagion, but then the fact is not *proved;* for it may be the situation which has given rise to it, some fault in the house, some drain, or other external cause, which may have caused the disease first in one, and then in another. I never pretend to say that is not the case. For infection to be proved, the individual who communicates the disease must go from the place where he resides to the spot where the healthy person is, and there give it to him. If the healthy person go to the sick person, and the sick person be in the place where he has been living, then no one can say that the disease has not been produced from the situation, and not from the patient. The disease *may* have arisen from contagion —from the emanations of the patient, but this is not *proved.* If the patient go to a healthy spot, especially from an unhealthy to a healthy spot, and the disease then occurs there in others—or if any thing which the patient have touched be taken from the place where the patient resides, to a healthy place, and there the disease takes place, then it is proof, if there be a sufficient number of these instances, that the disease is contagious.

Fomites.—Inanimate substances have become impregnated with the secretions or emanations from a person labouring under a disease, and communicate the disease to another—all such substances are called *fomites.* Among these, woollen substances are by far the most influential. All woollen substances imbibe secretions and emanations, and convey infectious and contagious diseases far more than any other description of substance. You will recollect that, even in the case of malaria, some assert that bushes will entangle it; and, on cutting them down, persons have been seized with intermittent and remittent fevers, arising in all probability from the disengagement of a considerable quantity of malaria which had been collected. Even cutting down woods will have the same effect; and therefore there is something analogous to fomites in the case of malaria; but it is in cases of contagion that to inanimate substances the term "fomites" is applied.

Now there are instances without end of the plague having been conveyed simply by old clothes. It is even said that the plague of London was conveyed to Derby by that means. There are scarcely more numerous instances of any thing in medicine, than of plague and typhus fever having been conveyed by left-off clothes. The exanthemata are frequently so communicated; and when I come to speak particularly of yellow fever, I shall be able to lay some instances before you in which this has unquestionably been the case; although it would appear from the testimony of one whom I have the pleasure to call my friend, that yellow fever is of different kinds—that there are two kinds, one which is not contagious, and one that undoubtedly is so.

I dare not say how long fomites will retain contagion. I know that a person who has been about another labouring under measles, will communicate the disease at the end of even a month; and that a room once infected with scarlatina will give the disease for a twelvemonth. The contagiousness of fomites is best destroyed by heating them well: articles may be exposed to a high temperature; and large fires, with plenty of smoke, best disinfect apartments.

Now whenever such a thing occurs as disease being produced in a healthy spot, by the approach of an unhealthy person to a healthy one, or the application of fomites to a healthy person, then it is a proof of contagion, provided there be enough instances, because one or two cases may be quite accidental.

However, you will find an argument against the contagion of typhus fever adduced from this circumstance—that it will sometimes disappear during an extreme temperature, and it will sometimes disappear altogether without any obvious reason; whereas, if it were contagious (it is argued) it would spread from one to another, till all, or the greater part, suffered it. It is said that it is only a disease which depends upon a particular state of the atmosphere, and not upon an emanation from a diseased person, and that *therefore* it is suspended by the extremes of temperature, or will suddenly cease without any obvious reason. It is indeed true that non-contagious yellow fever, and intermittent and remittent fevers, and other diseases decidedly not contagious, will be aggravated or repressed by extremes of temperature, and by causes not discoverable. But this is exactly the case not only with typhus fever, but with diseases which all people of common sense allow to be contagious. Epidemic small-pox is frequently checked by extreme cold. The plague, which I believe almost every body allows to be a contagious disease, in the strict sense of the word — not infectious, but contagious — is also stopped by extreme heat or cold. Small-pox is frequently so stopped, and also by the wind called *harmattan,* which also arrests the plague; and what is curious, this wind will prevent persons from taking the small-pox, even if they be inoculated. When that wind prevails, inoculation is commonly found to be fruitless. We therefore have the same fact with respect to diseases undoubtedly contagious, or infectious, or both, that we have with respect to such diseases as our remittent or intermittent fever, or others which depend upon something in the atmosphere not proceeding from diseased persons. This, therefore, can be no objection. Hooping-cough and measles, which most persons allow to be contagious,

are generally checked at the height of summer; and Sydenham says that scarlatina is most prevalent when the summer is over.

But there are still more remarkable facts with respect to the stopping of diseases known to be contagious—nay, of some diseases which are not infectious, but contagious, in the true sense of the word. Cow-pock, which can only be communicated by contact, will sometimes decline so much that at one period Dr. Jenner could not prosecute his inquiries into the disease for the want of matter. Although there was apparently no reason for the disease not to spread as usual, as it is said, "in a natural way," from the tit of a cow to the hand of a human subject, and this is very important, as clearing up the point, whether, notwithstanding that circumstance, it was sometimes so checked that Dr. Jenner could not obtain a case of it, and when he was publishing his work on the subject, the very same thing happened; and Dr. Woodville says, that the disease did not reappear till the following spring, at which time it chiefly prevails. Sometimes, without our knowing why, small-pox, an unquestionably contagious disease, will not affect a place quite contiguous to another, although it is prevailing in the one, and free intercourse is being carried on between them. Van Swieten mentions this, and Sir John Pringle says that he saw small-pox carried by recruits into the camp, and yet it did not spread. Dr. Odier, of Geneva, inoculated children when the disease was not epidemic, and though they were going about the streets every day during the eruption of the disease, and although there was the freest communication between the children who had been inoculated and others, yet not a single instance was seen of the disease spreading. There was something in the atmosphere, whatever it was, that prevented it. Sir James M'Grigor says, that the small-pox was raging in houses at Bombay contiguous to the barracks, and yet no one child or adult in the latter place imbibed the disease: there was something in the situation which prevented it. If you will read Burkhardt's Travels, you will find something equally curious on this point. This author says, that "it is a curious fact, but one which has been attested to me by many persons, that small-pox has never been known to visit the Wadykenous, which is a narrow shore from the Cataract up to Korosko." But, he adds, "this disease is well-known at Derr," (close by,) where it is much dreaded. It was never known to pass a certain point, notwithstanding there was free intercourse.

Now if such singularities will occur in the case of diseases known to be contagious, there is no reason at all to doubt that typhus fever is contagious merely because it will suddenly cease in a neighbourhood, or in a district, without our knowing why; or because it may be apparently arrested by an extreme of temperature. Not only will undoubtedly contagious diseases sometimes not spread in the natural way, to use common language, at certain seasons, but sometimes you cannot produce a disease notwithstanding you inoculate for it. It would appear to be the same in the case of hydrophobia. We sometimes hear of nothing but mad dogs; a mad dog is killed every day—sometimes for a month; and then again we never hear of the disease for a twelvemonth. Now one would suppose that the disease would be communicated from one to another easily enough, but it is impossible not to imagine that at one period there must be a far greater susceptibility to the disease than at another.

You see, therefore, that there is no *à priori* objection to the possibility of typhus-fever being contagious: the only question would be, is it a fact, or is it not? And I think there are sufficient instances—instances without end, of persons having communicated the disease to others. Provided there is not free ventilation—provided there is plenty of dirt, bad living, and unhealthy circumstances, I think there can be no question as to the disease frequently proving contagious.

Period of Incubation.—The interval which occurs between the application of the poison of typhus-fever and the appearance of the disease, is exceedingly various, exactly as occurs in other diseases. Dr. Haygarth says, that of 72 persons who were exposed to the contagion of typhus-fever, 5 were seized with the disease within 10 days after exposure; 13 were seized between the 10th and the 17th days; 41 were seized between the 17th and the 32d day; and 1 so late as the 72d day. This variation of interval is the same as is observed with respect to all diseases acknowledged to be contagious. Hydrophobia is a disease which sometimes begins in a few weeks; sometimes it will begin in a few days; generally, however, a few weeks elapses, and sometimes several months, before it makes its appearance. The same variation is observed in the case of small-pox; nay, there is often a difference, as I have mentioned already, as to the time at which this contagion takes effect, depending upon whether it is applied by means of the atmosphere, or in a palpable form by inoculation. The poison from inoculation produces the disease much sooner than infection—much sooner than if the poison be applied by means of the atmosphere.

How long the poison may lie dormant I cannot say, but to take a case from another poison—malaria, we must remember that many months frequently elapse before ague is produced, so that the malaria becomes the predisposing, instead of the exciting cause; and the same may be the case sometimes with respect to typhus-fever. It may exist dormant for a length of time, but what is the utmost period it will do so I cannot pre-

tend to say: it must have a limit, of course, but we know not what that limit is.

An objection to the contagiousness of typhus fever has been raised not only from the irregularity of the period at which it begins, but from the irregularity of the duration of the disease; yet scarlatina, although undoubtedly a contagious disease, is very uncertain as to its course. Scarlatina will in different instances shew the eruption at very different days; sometimes the first day—the very day on which the patient is taken ill, the eruption will appear, and sometimes it will appear before there is the least redness or soreness of the throat; and then, when the eruption does appear, it sometimes will last only a day, or a day and a half, or two days, and sometimes it will continue ten days. No valid objection can be raised to typhus fever being a contagious disease merely because it is very various, not only as to the period at which it takes place, but also as to its duration; for scarlet fever likewise exhibits these very same varieties. Nay, the same would also appear to be the case with regard to small-pox; for Sydenham speaks of an epidemic small-pox in which the eruption took place on the fourth day, which you know is not the usual period at which it occurs.

But although the contagion of typhus fever is rendered much more active by concentration, and both that and filth are injurious to a patient, and must render him more liable to be affected by the contagion, yet it frequently happens that the utmost filth and the greatest closeness, will not produce the disease. Some have ascribed the disease to confinement of air and to filth, and both these will unquestionably be injurious to the health at large, and contribute to render a person, exposed to the contagion of typhus, the victim of the disease; yet persons are continually exposed to mere filth and confinement who do not suffer. In Kamschatska, where the people live seven months in the year in yourts, which are cavities dug 7 or 8 feet under ground, and covered with a thatched roof, having only one small apartment perhaps for three families, with a stock of provisions consisting chiefly of dried putrid fish, and in which pits they eat, sleep, and do every thing promiscuously, so that there is the most intolerable stench, which, though not perceived by themselves, is but too evident to travellers, they have no fever; on the contrary, they are all healthy, with the exception of scurvy, which arises from the want of fresh provisions. The Greenlanders and Esquimaux, who crowd themselves together and exclude the air, having no chimneys in their huts, but an inner apartment, the fog and smell of which is quite suffocating to strangers, very rarely have fever; in general they only experience scurvy. The habitations of the Russian boors are equally intolerable to strangers, and yet it is said they never have putrid diseases. Dr. Lind says, that in the slave ships crossing the Atlantic, although the poor creatures are crowded below deck as much as possible, and at night are shut up under close hatches; and although they suffer from a change of climate, and some are suffocated; yet, in general, they have no infection; and if an accidental infectious disease enters among them, it is of a much milder character than when it occurs in felons who have been transported in an opposite direction, owing probably to the opposition of a high temperature to the contagion of typhus. It is said (but I do not know with what degree of truth) that contagious fever never occurs in these slave-ships. At the black-hole in Calcutta, where, in the month of June, 1756, one hundred and forty-six persons were confined from seven or eight o'clock in the evening till dawn the next day, in an apartment about a cube of eighteen feet, which only opened to the west by two windows strongly barred, one hundred and twenty-three individuals were suffocated, and the rest were of course made ill. It is said, however, that all which the survivers afterwards felt was heat and extreme exhaustion, and that a great many had boils, but no fever was produced. Howard, the philanthropist, says that there was no fever at Venice in the prisons, though these were the closest possible. He also says that the prisons at Naples were close and offensive; that the people were ill, but no fever was produced. He adds, that we must look for an additional cause of fever than mere filth and confinement. Dr. Mounsey says exactly the same of the prisons at Moscow and Petersburgh, though the prisoners were crowded together and had but little ventilation. But it is to be remembered, that when fever is introduced, then it rages with the most dreadful violence; and it is also found that new-comers into such filthy situations, when contagion is present, suffer much more than those who have been habituated to them; that contagion acts the more, on account of the bad state into which this filthy confinement throws the constitution. Those that were habituated to these situations before the contagion was introduced, became gradually accustomed to it. People every day live in confinement and stench which would half poison us, as you may well imagine; but when strangers go into this filth, after having been in pure air, they are much more liable to experience the disease, and if they suffer it is worse than others. Still, although, till certain contagions are introduced among people who live close, and dirty, and ill, they consider themselves as well as others differently situated, yet when those contagions are introduced, these at once act as a test of the tendency of the previous mode of life; and these people differ in an infinitely higher degree than others, provided there is not present any

accidental counteracting cause, as great heat in the case of typhous contagion. A mere surgical accident will test the previous mode of life in two people who might have appeared equally healthy.

As mere confinement will not in general produce fever, without some predisposing cause, so dead putrid animal matter is also said to be inocuous, while unaided. An instance of the latter is adduced with respect to Paris: there were 600,000 bodies buried in Saint Innocent's churchyard, in that city, in the course of six centuries, and they were spread under ground over two acres. The soil, by this vast deposition, was raised above the streets, and of course there was an offensive smell, but no fever arose from it. They were removed partially, in the heat of summer, till the ground was levelled, and the workmen were frequently asphyxiated, frequently fell down senseless, but none of them were attacked by fever, notwithstanding no precaution was employed. It is said, that in Seville there is a fetid odour from the ground where 10,000 bodies were deposited in an epidemic, that the soil cracked, and great stench was produced, but no fever. Howard, the philanthropist, says, that at Smyrna there was a most horrid smell from the burial-ground after plague, and many corpses lay uncovered, but no bad consequences at all ensued to the family of the governor, whose house was exposed to the exhalations from the spot. Dissecting-rooms will not produce fever, unless an individual be either exposed to contagion or is very much out of health through anxiety of mind, hard study, or some accidental circumstance. Persons attending a dissecting-room will become out of health and be ill, and perhaps die, from the very severe effects of slight wounds; but I am not aware that contagious fever which will spread to others has ever been produced in this way. Various diseases which have been confounded with fever, have been produced, but I am not aware that fever itself has so occurred, when there was no depressing passion, no anxiety from over study, want of proper rest, nor excess of any kind. Still, if mere stench, unaided by other things, and through delicacy of constitution, not counteracted by the presence of the ordinary causes of health, should impair the health, I can conceive that fever may be produced at last. Nightmen, as far as I know, are as healthy as other persons, and yet their grounds are certainly offensive enough. A spermaceti manufactory existed near Bristol for two years, the stench of which was intolerable, but no fever was produced. It is said, that at a bone manufactory, situated at Oldland, in Gloucestershire, where bones were employed for the purpose of procuring muriate of ammonia, an intolerable strench was produced, and yet no fever resulted from it. It is said, that the superintendant removed to a more convenient house, situated upon a hill, and there he and his family lost their health, and he had some idea of returning back to the stinking place, that he might recover his health. At sugar refineries, where blood is kept till it is putrid, persons do not suffer. Leather-dressers are exposed to offensive smells, and yet they escape.

It is, therefore, I think certain that mere confinement, mere emanations from persons crowded together amidst the greatest filth, will not in itself produce fever. I think it is quite certain that animal matter, in the greatest state of putrefaction, does not of itself afford any thing which will produce a disease called contagious. It may be that the persons so exposed are fed well, and are in good spirits, and have all other means of contributing to health at their command. Still, however, if any matter which is in a state of putrefaction, as is, or was formerly, the case in dissecting rooms, as occurs in bone manufactories, and places where putrid blood is used, and from which there are horrid emanations, could alone produce contagious diseases, we should have fever every day where we have nothing but the most perfect health. There can be no doubt that if these things throw a person out of health, then any contagion, or any other cause of fever, will act intensely in producing this disease. This is allowed with regard to cholera. Nobody believes that any putrid emanations, or any thing that proceeds from persons crowded together, or the putrefaction of animal matter, will produce that disease, but all know that these things have a tendency to throw people out of health, and by the body being thus brought into an unnatural state, it is rendered an easy prey to any other causes that are applied.

I have here, gentlemen, a very fine specimen of ulceration of the intestines after fever, with which I have been favoured by Dr. Tweedie.

CLINICAL LECTURE
ON
NON-MALIGNANT AFFECTIONS OF THE FEMALE BREAST,

Delivered at St. Bartholomew's Hospital,

By Mr. Earle.

Susan Irwin, æt. nineteen, a young woman of healthy appearance, admitted into Sitwell's ward December 15, 1831, states that five years ago she received a blow upon her chest, and shortly afterwards a small and rather painful swelling appeared in her right breast, which continued in a very inactive state for two years. It then, however, became painful, and increased in size, which it has continued to do. At present the tumor is of considerable size, painful,

and tender to the touch. The pain she describes as shooting through the shoulder, extending into the axilla, and down the arm, but there is no swelling or hardness of the glands in the axilla. Leeches have been repeatedly applied, and afford some temporary relief. There is likewise a similar, but much smaller tumor in the left breast, accompanied with severe lancinating pain, which, however, she says, has existed but two years.

From the circumstance of there being a similar tumor in each breast, and from the character they present, it is very unlikely that the first arose, as she supposes, from the application of external violence.

These affections are not of uncommon occurrence, and of a perfectly innocent character, being the products of simple chronic inflammatory action. They usually occur in young persons, between the age of 15 and 30, and often in those who are otherwise in the enjoyment of good health.

The diagnostic characters distinguishing this from a carcinomatous affection (besides that of the age of the patient) are well marked. This tumor is much more superficially placed, being even prominent when viewed at some little distance, appearing rather on the surface than the interior of the mamma; it is very moveable in every direction, excepting towards the surface of the mammary gland, to the edge of which it is attached. At its commencement it feels like one of the lobes of the mammary gland, converted into this indurated structure; and as it proceeds, other lobes seem to become gradually involved, and combined in one swelling. It is generally confined by its attachment to one side of the mammary gland. In some cases it is wholly unconnected with this body, but not so in the majority. Sometimes there would seem to be a communicating band between the tumor and the gland; and if traced at this point, the tumor seems to be lost in the substance of the gland itself. When examined with a light hand it is found to present a peculiar irregular surface, not so indurated as that attendant upon carcinomatous disease, nor yet possessing that elasticity which characterises fungoid affections.

It is often dependant upon irregularities in the female constitution, and sometimes connected with a depravity or suspension of the menstrual secretion. At the menstrual periods there is always an aggravation of the pain felt, and even in those cases where generally there is no pain attendant upon the tumor, yet at these periods more or less pain is present. It is very slow and gradual in its progress, and rarely reaches any considerable magnitude. Upon examining a tumor of this kind which has been removed we find it to consist of many small lobes, connected by dense cellular tissue, and these again composed of smaller lobules, the whole presenting an appearance not unlike that of a sweetbread; and it is, indeed, to cases which may be considered as similar to these that Mr. Abernethy has applied the appellation of pancreatic-sarcoma. Mr. Abernethy describes this disease as made of irregularly-shaped masses, in colour and texture resembling the larger masses which compose the pancreas. These appear to be connected with each other, like the portions of that gland, by a fibrous cellular substance. This texture, he says, often forms in female breasts, amidst the mammary glands, commencing probably in the lymphatic glands. These two affections present so many points of resemblance that it is fair to consider them as one and the same affection.

Mr. Earle expressed himself ignorant of any means by which these tumors might be entirely dispersed, although this has been effected to a certain point by the use of iodine. They are a perpetual source of annoyance to the patient lest they should be of a cancerous nature; and, indeed, if left to themselves, although at their commencement simple unmalignant structures, yet after a long lapse of time, and after the critical change which takes place in the menstrual function of the female, most unquestionably they may take on a malignant action. Therefore we should on these accounts recommend their removal to the patient.

Although in this case both breasts be affected, that should not, as it would in a carcinomatous affection, afford any obstacle to the performance of the operation; for in these affections there is neither any disposition of the disease to return, or to manifest itself in any other part of the body afterwards.

In general the removal of these tumors may be accomplished without taking away any of the external skin, by a simple incision; but where, from the pressure of the stays, or any other cause, any portion of the skin may have become adherent over the tumor, it must be removed. In performing the operation, also, we need remove only that portion of the mammary gland to which the tumor may be attached, a circumstance often inducing a female to submit to an operation which she would not have done if the whole gland were to be sacrificed, a matter of much importance at the time of life when these affections generally occur. The structure of the tumor, however, in some instances so much resembles that of the healthy mammary gland that it is not always easy to say whether or not the whole has been removed. The best criterion is the degree of hardness which exceeds that of a healthy mammary gland. The glands which have become enlarged in the axilla soon subside after the removal of the disease from the breast. If, after the tumor is removed, any very consi-

derable cavity be left, it will be advisable not to endeavour to procure union by the first intention, unless the internal parts can be brought into complete apposition. It is useless, and often positively injurious, to endeavour to effect this speedy union of the integument. It has several times occurred that very few vessels have bled during the operation, and the edges of the wound have been brought closely together, leaving a deep cavity, into which, from the hæmorrhagic action of the vessels being excited, most troublesome secondary hæmorrhage has taken place.

Mr. Earle took this opportunity of making some observations on some other affections of the breast, likewise not of a malignant character.

A common fatty tumor may form in this part, and be treated just as if it occurred elsewhere.

Another affection described by Sir A. Cooper is the hydatid tumor of the breast, which will sometimes reach a very large size, without being productive of much pain: occasionally the size it reaches is immense. Thus Sir A. Cooper mentions a case where the tumor weighed thirteen pounds. This affection may occur at any age under twenty, and frequently does so at an advanced period of life. An excellent example of this disease occurred in this hospital in the course of last year, which was successfully removed. On dissection the whole breast appears much consolidated, containing cysts filled with serum, or a muco-purulent fluid. These cysts are sometimes vascular, at others being simple bags of serum; sometimes they much resemble the hydatids which form in the liver. Another species is composed of numerous membranes, which may be peeled off from each other like the concentric layers of the crystalline.

In some cases, hard cartilaginous bands extend across the cysts, so as to keep them expanded after they have been cut across.

In these cases there is but little disturbance of the general health, nor is the tumor tender to the touch. Though elastic, as in fungoid disease, it is infinitely more so, so as to give a decided and easily perceptible sense of fluctuation. In order to detect this property you should not use any forcible pressure; but, supporting the tumor with one hand, you should produce the slightest possible impulse with the other, and then the fluctuation is directly perceived.

This disease is not at all likely to return, provided the whole of the cysts be removed; but if any one of them be left, it would seem to have the power of generating others. When there is a single cyst of moderate size, it may be opened, and the cyst obliterated by adhesive inflammation.

There is another affection in which we have no distinct tumor or lump in the breast, but the whole organ becomes enlarged in bulk, sometimes to an enormous degree; it is, in fact, a state of hypertrophy of the breast, and has been well described by the late Mr. Hey.

In all cases it is connected with a suppressed or disordered state of the menstrual function. Mr. Hey mentions the case of a girl who menstruated when only twelve years old, but shortly afterwards the menses disappeared, in consequence of damp clothes being worn during the period. The breasts increased in size, and her left breast reached the enormous weight of eleven pounds four ounces; upon its removal the menses returned, and the right breast diminished.

Mr. E. has not met with one of these cases, but he suggested, in preference to the removal of the breast, that the effects of iodine might be tried; and, if not successful, he conceived that benefit would arise from cutting off the supply of blood to the part, by securing some of the largest vessels leading to the gland.

In reference to this subject of hypertrophy, Mr. E. mentioned the particulars of a singular case which was at present under his care. It was that of a very large tumor of the cheek, in a child two years old, producing great deformity, and said to be, by the parents, congenital. The whole structure of the cheek and superior maxillary bone was increased, or in what might be said to be in a state of hypertrophy rather than of any new development; the superior maxillary bone, from its great increase, projecting very much beyond the other. At the age of six months the child cut its cuspidatus upon that side, which is now deformed, and of a gigantic size. There are likewise two bicuspid teeth, as large as those found in an adult. The parents state, that this swelling of the cheek, although now very large and prominent, has always increased in proportion to the increased growth of the child only, who is strong and healthy. The carotid artery upon the affected side beats with a much more forcible pulsation than upon the opposite side.

It occurred to Mr. Earle, whether cutting off the supply of blood to that part might not be of service; for although it could not have any effect in lessening the growth already formed, yet it would perhaps prevent its future progress, and thus, as it were, give the system time to overtake it. Previously, however, to doing any thing of this kind, it was his intention to make some experiments, in order to try what effect tying the carotid artery upon one side had on the growth of the teeth and jaw upon that side in young animals.

Mr. Earle next alluded to a case of mammary abscess (Mary Porter) now in Sitwel also. He observed that mammary abscess

was not of unfrequent occurrence, especially after the first delivery; arising either from exposure to cold, too long detention of the milk, or the irritation produced by sore nipples.

This last cause—sore nipple—is of itself frequently a source of the greatest annoyance and suffering to the woman. It is not at all an unfrequent occurrence for the nipples, after the first delivery, to become inflamed, and, by subsequent irritation, ulcerated—often producing the most horrible suffering and torture every time the mouth of the child is applied; yet, so great is the maternal solicitude for the welfare of the child, that women will often undergo the most horrible torments rather than relinquish suckling.

Many applications have been recommended for this affection, but there are some which are especially beneficial. In the simple cracked nipple, and where ulceration has not yet taken place, great benefit is attendant upon the employment, every time the child quits the breast, of small portions of the unguentum hydr. nitr. oxyd. mixed with a small portion of opium. If the mouth of the infant were not so repeatedly applied to the sore nipple, it would soon become well; but every time it sucks, the process of reparation is destroyed.

The mere shielding the surface from the action of the atmosphere, by the sprinkling a small quantity of some inert powder—as gum arabic—over it, is also very useful. Balsam of Peru is an excellent application; while, at other times, the most simple substances are the most useful—as clarified mutton suet.

Where the nipples are very bad, we must have recourse to the employment of shields. However, in the slighter cases it is desirable to persevere in the use of other means, so as, if possible, to be enable to dispense with the use of the shield; both because weakly children are sometimes unable to suck through an artificial apparatus, and because it is found that if the mother can overcome this affection, she is not so liable to its recurrence on future occasions.

There is often considerable difficulty in making a child suck by these apparatus. One cause is, that the openings in the shield where the teat is sewed on to the ivory, when applied to the mamma, are not sufficiently closed, so that the child at every effort draws in a quantity of air instead of milk. This may be remedied by a very simple contrivance, viz. placing a portion of moistened wash leather, with an aperture for the nipple, around the base of the shield, which, acting as a valve, enables the child at the very first effort to form a perfect vacuum, and the milk immediately flows into the mouth.

When abscess of the breast has formed, whether from this or any other cause, we must never wait for its approaching the surface: as soon as fluctuation is perceived, even in ever so slight a degree, an opening should be made; for, if this be not done, the abscess will extend about and behind the gland, sometimes burrowing in different directions to a vast extent. Sometimes you may feel fluctuation at the onset, confined to a single lacteal tube, or one lobe of the gland, and, by a timely incision, save the patient a great deal of suffering. It would seem to be limited occasionally to the obstruction of a single duct. Great care should be taken to keep the edges of the incision well opened.

As to the management of the child in these cases. If it be a large abscess, it will generally be necessary to wean the child of that breast at all events, and in some cases of both, as the flow of milk induced by the suckling at the sound breast induces a sympathetic determination to the other. In cases where the abscess is not so extensive, we may at first, at all events, try other means, as the keeping a free exit for the matter, and purging the bowels briskly, in order to keep the secretion somewhat under. Mr. E. has known some cases do very well, where, after the secretion of pus has ceased, that of milk has continued for some time into the cyst.

If consulted in the commencement of these cases, before matter has formed, you may often prevent its formation. In this stage of the affection it is very common to apply cold, but Mr. E. thinks this acts prejudicially; while he has seen great benefit arise from the judicious use of warmth, during the application of which the milk will often flow off in great abundance. A convenient and simple mode of applying warmth is to immerse a wooden bowl in hot water, and having wrapped some flannel around the breast, place it in the bowl. By this means an effectual and equable warmth may be kept up for a considerable length of time. Another equally simple mode of drawing the breasts is to procure a wide-mouthed gooseberry bottle, and expel the air from it by immersing it in hot water. When it is then applied to the breast a vacuum is of course formed, and the milk flows most abundantly. Where, as sometimes happens, there are hardened knots around the gland, the use of common camphorated oil is of the greatest use in dispersing them.

Mr. E in conclusion said, that he would not offer any apology for what might appear unnecessarily minute details, as he hoped some of the expedients he recommended would contribute to the reputation of his auditors and the comfort of their patients, and he added—

"Nihil humani a me alienum puto."

ON STRANGULATED HERNIA.

To the Editor of the London Medical Gazette.

SIR,

THE subject of strangulated hernia is one of such paramount importance to the surgeon, that I feel no apology is necessary for requesting you to give insertion to the following cases, which have occurred to me in little more than twelve months. They do not any of them present this subject under an entirely new point of view, but as there is the same necessity in medicine as in morals for "line upon line, and precept upon precept," their perusal may at least serve to revive the remembrance of established truths, if not to suggest new ones. In their arrangement, I have disregarded the order of time in which they occurred, being rather guided by the degree of difficulty which they severally presented, beginning with the most simple.

CASE I. *Epiplocele successfully treated.*—Mrs. Gibbons, of No. 4, Halfmoon Crescent, White Conduit Fields, a weakly little woman, about 38 years of age, who had borne several children, sent to me in the afternoon of the 6th instant (December), in consequence of the reappearance of an old rupture. She had been the subject of a hernia for several years, and wore a truss at the time this came down.

About an hour and a half before I saw her, in attempting to lift a moderately heavy washing-tub, she suddenly felt a very acute pain in the upper part of the right thigh, and immediately perceived a return of swelling. The pain speedily extended to the abdomen, and was accompanied, both there and in the tumor, by great tenderness on pressure. There was immediate production of nausea, very soon followed by powerful retching, but vomiting had not come on. The pulse was unaffected.

The tumor was about the size of a walnut, round, tense as a foot-ball, and situated close to the outer side of the body of the right os pubes, the spine of the pubes, and the mesial part of Poupart's ligament appearing to bound it superiorly.

I had my patient brought near to the edge of the bed, the back well supported by pillows, so as to place her in the half-sitting position; a doubled pillow was then put under the ham, to keep the thigh flexed upon the pelvis, and the knee was turned inwards over that of the opposite side. Sitting at her right side, I commenced by drawing down the swelling a little, as it appeared slightly inclined to turn over the crural arch, and then compressed it, and especially its neck, with moderate firmness, applying the fingers of both hands equally around it. In about eight minutes the tumor felt suddenly *softened*. I increased the pressure, directing it upwards and slightly inwards, when a portion of it passed up, rendering the immediate reduction of the remainder perfectly easy.

As the reduction was accompanied in this case by no gurgling noise, and as the latter portion of the protrusion required pushing through the crural canal with the point of the finger, as well as from the sensation communicated to the touch immediately before its replacement, I should consider this to have been an epiplocele, notwithstanding the rapid progress of the symptoms.

The abdomen was directed to be fomented: abstinence from every thing but slops, and perfect quietude in bed for twenty-four hours, were strictly enjoined.

I saw her again in the evening, five hours afterwards, and found that the nausea and abdominal tenderness had both left her.

On the following evening I found her sitting up with her truss on, perfectly comfortable in every respect, her bowels having been freely moved without the aid of medicine.

CASE II. *Recurrence of the Complaint—Cure.*—The same person called me in twelve months ago, on a similar account. At that time the rupture had, however, been down for four hours, there was frequent vomiting, and the abdominal tenderness was extreme. The reduction was not effected until after a trial of the taxis of half an hour's duration, and it left her with excessive pain on pressing the abdomen, frequent vomiting, and a small hard pulse of 130 in the minute. I took, therefore, twenty-four ounces of blood from the arm, which produced faintness. The blood was cupped and buffed; and on the following day, the tenderness of the belly continuing, twenty leeches were directed to be applied to it. Under the use of these means, with the assistance of abstinence, rest, purgatives, clysters, and fomentations, the inflammatory symptoms were entirely removed; and at the end of a week she was left with no other inconvenience from her attack than the consequent depression of strength.

CASE III. *Strangulated Femoral Hernia successfully treated.*—I was sent for about ten o'clock in the morning of February 1, 1831, to Mrs. Baker, of Holloway, on account of a strangulated femoral hernia of the left side. She had first observed this rupture about a year before, on getting up after a hard labour, but as it gave her no uneasiness she paid little attention to it, and did not seem to be aware whether it passed up or not when she lay down in bed. The night before, without any particular exertion or straining that she

recollects, the tumor became suddenly enlarged and painful, and soon afterwards she had vomiting, with pain and tenderness of the abdomen.

On examination, I found a tumor about the size of a pigeon's egg at the upper and inner part of the left thigh, in the situation of femoral hernia; it was very tense, *and received no impulse on coughing*. The pulse was 98, feeble, and soft. After placing her in such a situation as to relax the abdominal muscles, and the fasciæ connected with Poupart's ligament, I tried to reduce the swelling by the hand; and as the tenderness, although considerable, was by no means excessive, the attempt was continued for forty minutes without any sensible effect upon its size.

Whilst this was going on, I had given directions to prepare a bath, and the infusion of tobacco in the proportion of a drachm and a half to the pint of water. The bath not being ready, I administered an enema containing half the infusion, which immediately produced great faintness, and strong inclination to go to stool. The bowels were but slightly moved; the faintness, however, continued, and as soon as she could be replaced in her former position, the attempts at reduction were again commenced. The tobacco had rendered this perfectly easy, for a portion of the rupture had already passed into the abdomen, and the remainder yielded immediately to the taxis.

There being a good deal of pain and tenderness of the abdomen, twenty leeches were ordered to be applied, followed by fomentations, and a large bran poultice; and 25 drops of tinctura opii were administered.

In the evening all pain except that occasioned by pressure had disappeared, and this was much diminished. As the bowels had not been again moved, clysters were directed to be given every two hours.

On the following morning (2d day) I found the bowels had been very freely moved, all the unpleasant symptoms had disappeared, and, recommending abstinence for a few days, I left her, with directions to have a truss applied before she rose from bed. This was obtained, and in a few days she returned to her usual occupations.

Case IV. *Strangulated Femoral Hernia—Operation—Recovery.*—Mrs. May, æt. 71, residing at 9, Norfolk-Street, Islington, had been the subject of a femoral hernia of the right side for several years. It first appeared on suddenly becoming thinner after a period of unusual obesity, but did not occasion much inconvenience until about twelve months previous to my seeing her, when finding herself unable to reduce it as usual, and suffering from pain, she was obliged to call in a medical man, who speedily relieved her.

From that time until January 28th, 1831, she was free from any serious symptoms on this account; but the rupture then became suddenly increased in size, painful, and particularly so when pressed upon; and it occasioned a similar state of the abdomen, with vomiting and constipation. For these symptoms she applied to a neighbouring practitioner, who attempted the reduction of the rupture, but without effect, ordered some aperient medicine, which was followed by one motion from the bowels, and advised her being sent to an hospital. Her prejudices prevented a compliance with this advice; she therefore continued to take medicine, which was almost immediately vomited up, and on the fourth day from the occurrence of the strangulation I was requested to see her.

January 31st.—There was frequent vomiting of bilious matter in small quantities; the pain in the abdomen, although constantly present, was much increased at intervals, and at all times greatly so by pressure; she had a slightly furred tongue, thirst, and a rather feeble intermitting pulse of 82 in the minute. In the right thigh was a very tense round tumor, about the size of the broad end of a hen's egg, which was situated close to the spine of the pubes, about four-fifths appearing to be lower down than this process, and the remaining fifth above it. *There was no impulse communicated to the swelling on coughing.*

The taxis was tried in the usual manner for thirty-five minutes, without any avail; and an infusion of tobacco, ʒiss. to the Oss. being prepared, one-third of this was administered as an enema. This was speedily rejected, with a small portion of feculent matter, but without any constitutional effects. I had given orders to get a hot bath ready whilst these things were going on, intending to defer a repetition of the taxis until after this had been employed; there was, however, a good deal of delay, in consequence of the difficulty of obtaining hot water, and in the meantime my friend Mr. Mackmurdo arrived, accompanied by Dr. Millar.

Mr. Mackmurdo having tried for a few minutes to reduce it by the hand, without any effect, it was agreed in consultation to proceed at once to the operation, as it was thought that a further trial of the tobacco clyster, or the hot bath, might produce fatal depression in the old woman, already much exhausted by three days' vomiting, pain, anxiety, and sleeplessness.

The pubes being shaved, she was removed to a moderately high table, receiving a good light from two windows, and covered with a blanket. Her shoulders were slightly raised by pillows, and her legs hanging over one end of the table were supported on a chair, in such a manner that the skin covering the tumor was slightly stretched, yet without giving much pain. By pinching up a fold of

the integuments, and pushing the double-edged scalpel through it from below upwards, a perpendicular incision was made nearly two inches in length, which was sufficient to expose the *superficial fascia* in the whole extent of the tumor. This, and afterwards the *fascia propria*, were now taken up by the forceps layer by layer, and divided by the knife held in the horizontal direction. By this a membrane was exposed, through which the vessels of the intestine could be readily seen running in a circular direction—indeed so readily that, in consequence of this appearance, and the inability to raise any thing more with the forceps, or between the thumb and finger, I concluded we had arrived at the intestine. Hitherto I had confined the dissection to the most prominent part of the swelling, but now having passed a director upwards and downwards beneath the fasciæ from this part, I divided them in the whole length of the external wound. Mr. Mackmurdo, notwithstanding the circumstances above mentioned, did not think the sac was yet opened: I therefore again cautiously attempted to raise the transparent membrane with the forceps, and having at length succeeded in doing so, and dividing it, a small portion of fluid, scarcely amounting to the tenth part of a minim, moistened the edges of the incision.

Sir Astley Cooper has justly considered this as the most difficult part of the operation; for where the sac is unchanged in texture, is in close contact with the intestine, and does not contain fluid, or only the most minute portion of this, as in the present instance, it is very easy to mistake it for the peritoneal coat of the gut itself.

The sac being divided, a mass of small intestine, about the size of the closed hand of a child six or seven years old, was now exposed. As there were no adhesions between the sac and intestine anteriorly and mesially, I readily succeeded in passing a director between them on the inner side. Along the groove of this, a hernia knife with a cutting edge only of about half an inch in extent, and having a blunt end of nearly the same extent, was introduced, and the neck of the sac and Gimbernat's ligament so freely divided, that I could pass my finger into the abdomen. I could not, however, yet return the intestine, on account of adhesions arising from an effusion of lymph, by which it was glued to the neck of the sac in three-fourths of its extent, as well as to nearly the whole of the sac on its outer side and posterior part. The stricture being now removed, I was advised to leave the intestine down, rather than run the risk of rupturing it by the attempts to separate the adhesions. Fearing to leave her in this state, I determined, however, cautiously to persevere in the efforts to complete the operation; and at length, partly by the fingers, partly by the handle of an ivory scalpel, and occasionally by the use of its blade, I succeeded to my wishes, and was enabled to return the protruded parts into the abdomen.

A slight cough came on at this moment, whilst preparing to dress the wound, and occasioned a reprotrusion of the gut, which was of course instantly replaced. The edges of the wound were brought together by a couple of sutures tied over a dossil of lint, and the woman carried to bed; perfect quietude, a cool room, and the strictest abstinence from every thing but slops, being enjoined.

On calling to see her four hours afterwards, I found that the bowels had been moved spontaneously, and that she was free from pain (except on pressure) and sickness. In four hours more I again visited her, and found that the bowels had been again moved without any medicine, and that very copiously. She had coughed slightly two or three times since I saw her before; and on looking at the parts which had been the subject of operation, they appeared to be so much swollen, and the swelling was so prominent in its centre, that I felt it would be unjustifiable on my part to leave her for the remainder of the night, without ascertaining that there was no fresh protrusion. The sutures were therefore removed, and the fore finger introduced into the crural ring, which was found to contain nothing but a portion of the sac. Adhesive plaisters were now applied, to approximate the edges of the wound, without stitches. As the pulse was a little quickened, although no longer intermitting, and the patient thirsty and feeling restless, with a hot dry skin, I ordered her,

P. Ipecac. Co. gr. v. stat.; et Mist. Salin. Effervesc. every three hours afterwards, if awake.

Feb. 1st (day following operation).—She slept well last night, and had a little fever, but no return of pain or sickness.

Contr.

Evening.—Bowels have not been opened to-day, and there is considerable palpitation of the heart.

Pulv. Ipecac. Co. gr. vij.; Calomel, gr. ij. h. s. s. Salinæ rep. in Mag. Sulphate, ʒj. to each dose.

Feb. 2d.—As the first dose of salts occasioned nausea, the simple salines were again employed, with common clysters every two hours; and these speedily produced a very copious evacuation.

Arrow root and milk allowed.

It is unnecessary to enumerate the daily occurrences which took place from this time; suffice it to say, that no untoward circumstance occurred except the slow healing of

the wound, which was not cicatrized until the expiration of the sixth week.

I have heard recently that this old woman still continues to follow her former employment of washing.

Case V. *Strangulated Omentum—Successful Treatment by Operation.*—A lady residing at Islington has had a hernia for sixteen years. For this she was in the habit of wearing a truss, until three or four years ago, when having a prolapsus uteri, and believing this to be made worse by the pressure of the bandage, she left it off. On rising in the morning of the 2d of the present month (December), she had a fit of coughing, and immediately felt a slight pain in the situation of the rupture; but as it speedily passed off, she thought nothing more about it, and attended to her household duties as usual. At about eight o'clock in the evening the part became very painful, and appeared to be larger than usual; she therefore tried to reduce it, but without success. Pains now attacked the abdomen, which, as well as the tumor itself, became extremely tender, and vomiting having supervened, she was induced to send for her surgeon, Mr. M'Crea, about ten o'clock at night.

Notwithstanding the progress of the symptoms, she had no idea that the rupture which had been quiet for so many years could be the cause of her present illness, and therefore did not mention its existence until Mr. M'Crea had especially inquired and sought for it.

The taxis having been tried without effect, and the symptoms increasing in violence, I was requested to see her, and arrived at about half-past eleven.

Her pulse was upwards of 120 in the minute, hard and contracted; there was frequent vomiting, with considerable thirst; the bowels had been opened in the morning, but not since. In the left groin there was a rather tense tumor, of an oval form, with its longest diameter in the direction of Poupart's ligament, which received an impetus on vomiting, was painful to the touch, and from which pains extended to the abdomen. She stated that the rupture had commenced nearer the spine of the ilium—in fact, that it had never been so low down before. It now occupied the inner half of the space between the spines of the ilium and pubes, and the greater part of it lay above (attached to) Poupart's ligament.

The history and appearance of this rupture coincided in declaring it to be a bubonocele, and such was our first opinion. On examining, however, more accurately the relation of its *mesial* portion to the spine of the pubes and Poupart's ligament, it was discovered to be covered by the latter for a small extent, and consequently ascertained to be a femoral hernia.

A trial of the taxis was productive of a good deal of pain, and evidently increased the vomiting; ℥xxiv. of blood were therefore abstracted from the arm, producing extreme faintness, and in this state our attempts at reduction were renewed, but in vain. After recovering a little from the effects of the bleeding, a pint of infusion of tobacco, made with ʒj. of the narcotic, was thrown up at twice. This brought away each time some feculent matter, and occasioned a return of faintness; but neither in this state, nor after the application of a freezing mixture, could we replace the protrusion.

The blood which had been drawn presented a highly-inflammatory surface, the symptoms continued unabated in violence, and the usual means having been all tried, with the exception of the bath, which could not be readily procured, we resolved on the operation without farther delay. An incision made in the way before described was of insufficient extent, in consequence of the great deposit of fat in this case at the lower part of the abdomen; I therefore had to enlarge it upwards to the extent of four inches. There was no fluid in the hernial sac. The protrusion consisted of a piece of omentum somewhat like suet in consistence, and nearly pyriform in shape. Its neck was much flattened, and long enough to curve around the crural ring; its broad end was directed upwards and outwards towards the ilium,—thus it lay over the inguinal canal. The adhesions between its base and the sac were numerous and firm, and required a good deal of dissection for their removal; its neck was, however, quite free. A free division of Gimbernat's ligament not allowing sufficient space for its reduction, I made one or two slight incisions into Poupart's ligament (taking care always to have the knife introduced into the neck of the sac, that this might be divided at the same time), and thus gained sufficient room. The best plan would probably have been to have cut off the greater portion of the protruded part, as by this means the necessity for so free a division of the crural ring would have been obviated; there appeared, however, to be nothing either in the size or state of the part requiring this, and the chance of subsequent hæmorrhage was an objection. The edges of the incision were brought together by a couple of sutures and adhesive straps; and although vomiting occurred even during the time of the operation, it ceased immediately, and never returned afterwards.

Dec. 3d (day following operation).—Mr. M'Crea and I saw her together at nine this morning, and again at half-past four. There was neither nausea, pain, or tenderness of the abdomen, remaining, *except in the neighbourhood of the wound*; yet, as the pulse remained at 120, we thought it right to apply sixteen leeches there, and followed them by

fomentations and a large poultice. In the evening at nine the tenderness was here also diminished, the pulse was still at 120, she had slight rigors, followed by a hot dry skin, and the bowels had not been moved.

Ol. Ricini, ℥ij. to be taken at four in the morning, should the bowels not have been moved in the interim, and to be repeated every four hours until this effect is produced.

Dec. 4th.—The first dose having operated slightly on our visit in the morning, a second had not been given; but as we found a good deal of fulness in the situation of the lower part of the descending colon, with some tenderness on pressure, an enema was administered, and the castor oil, in combination with tincture of hyoscyamus and ipecacuanha wine, on account of a troublesome cough, was directed to be repeated. Pulse soft, small, 124.

5th.—The bowels have been opened twice, and she has passed a good deal of flatus; pulse 114. The skin above the wound is florid, and the subcutaneous cellular tissue considerably swollen. The dressings and sutures were removed, and the wound found to have adhered partially in the centre, and at the extremities. Adhesive straps, with intervals between them, covered by a simple pledget, and a large poultice over all, were now applied.

6th.—Emplastrum cerati saponis substituted as a dressing; erysipelatous blush much paler; going on well in every respect.

9th.—Still improving; pulse 90; tongue clean; bowels either spontaneously moved, or readily excited to act by medicine. She is taking jelly, milk, eggs, and bread.

12th.—Her bowels not having been opened yesterday, she unadvisedly took a couple of doses of active aperient medicine. This occasioned three or four copious evacuations, and brought on much debility. The granulations are large, flabby, and of a tawny colour, and some appear to have been absorbed.

Quinine and meat were directed to be taken.

16th.—Granulations smaller and more florid; depth of wound is much diminished, and superficial extent less.

23d.—Going on slowly but well.

29th.—The wound is now healed, except at its lowest part, for the space of about half an inch.

Remarks.—The first three cases, when compared with each other, illustrate the advantages arising from an early attention to a rupture when incarcerated, both as affording a better chance of effecting its reduction, and also by enabling us to avert the occurrence of peritonitis. The third case also points out the good effects of the tobacco clyster. The fourth shews that, even where there is general adhesion of protruded intestine, its cautious separation, and its replacement into the abdomen, may be followed by the immediate relief of every bad symptom. The fifth is interesting to the practical surgeon in many points of view. It shews, 1st, The necessity of a careful manual examination of the abdomen, and especially of those parts most frequently the seat of hernia in every case of ileus; for had Mr. M'Crea depended on his patient's pointing out this circumstance, if it existed, she would probably have been long since in her grave. 2dly, That the appearances of a femoral rupture, and the account given of its progress, may be such as to lead us to the belief that it is a bubonocele, and consequently may induce us to employ the taxis in a wrong direction. 3dly, That a piece of omentum, when the subject of strangulation, may occasion as severe symptoms in a person disposed to inflammatory action, as a portion of intestine ordinarily produces; and thus that, even if we knew with certainty the rupture to be an epiplocele, we should not therefore be justified in longer delaying the operation than during the employment of those milder means of relief which experience has taught us are so frequently efficacious.

The unexpected length to which this paper has extended, prevents me from pursuing these remarks further at present, but I hope on some future occasion to have an opportunity of examining the subject of hernia in some of its other forms; and am, sir,

Your obedient servant,

H. Bateman.

Islington, April 1832.

PHENOMENA OF THE LARYNX,

Displayed, from its being laid open,

IN ATTEMPTED SUICIDE.

To the Editor of the London Medical Gazette.

Sir,

If the following case, with some remarks which I have made upon it, appear to you of sufficient interest for publication in the Medical Gazette, it is very much at your service.

T. P. ætat. 50, was admitted into the

Middlesex Hospital on the 4th of April. He had attempted to destroy himself by inflicting a deep wound on the fore part of the throat, between the os hyoïdes and the thyreoïd cartilage. The epiglottis had been divided immediately above the thyreoïd, so that the upper part of the arytænoïd cartilages was exposed. The breadth of the opening into the pharynx exceeded an inch; no large blood-vessel had been wounded.

The treatment of this patient, who is rapidly recovering, has been of the simplest nature. Any attempt to keep the surfaces of the wound in contact has been found to give great distress, by preventing the free escape of the mucus which accumulates in the throat, and which the patient is unable to get rid of through the fauces. The wound, left to itself, with the exception of continual cleansing, is covered with healthy granulations, and contracts daily.

The following are the observations which I have had an opportunity of making upon this case.

1. Deglutition is performed without difficulty. Only when liquids are swallowed, a small quantity escapes through the wound. This is not attended with coughing, or any sensible irritation of the larynx.

2. All that can be seen of the interior of the larynx, are the upper part of the arytænoïd cartilages and the folds of membrane reflected from thence towards the epiglottis. During gentle respiration the state of these parts is exactly such as is represented in the figure 1.

Fig. 1.

The aperture of the glottis, under these circumstances, is seen to be considerably expanded and motionless. It does not, however, continue motionless if the patient become agitated, and the breathing disturbed. In the latter case, at each expiration, the aperture is considerably narrowed.

3. When the patient forcibly, and with an effort of straining, closes the glottis, the appearance of the parts is such as is represented in figure 2.

Fig. 2.

The median vertical line is now very distinctly observable, at which the arytænoïd cartilages are pressed against each other. The superior aperture of the glottis, although narrowed, is open. The closure of the glottis takes place, not at its upper border, but at the superior ligaments.

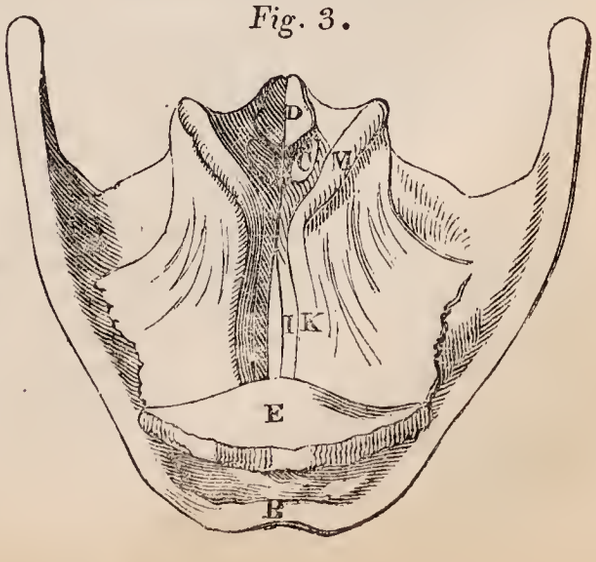
When the patient utters a continued sound, the condition of the parts appears to be exactly the same as when the glottis is forcibly closed. The patient has but one note in his voice.

When the patient passes alternately from one of these actions to the other, now closing the glottis, now uttering a continued sound, no sensible change takes place in the disposition of the parts: the arytænoïd cartilages do not appear more or less closely applied to each other in either case: only while sound is uttered, the whole surface appears tremulous with vibration.

In each of the preceding figures, two strongly projecting parts are drawn, the distance or approximation of which marks the open or closed state of the glottis. I was at first at a loss to understand what these prominent parts were; but upon carefully examining the larynx in an adult male body, and comparing it with the appearances in the present case, I found that they must be the cuneïform cartilages.

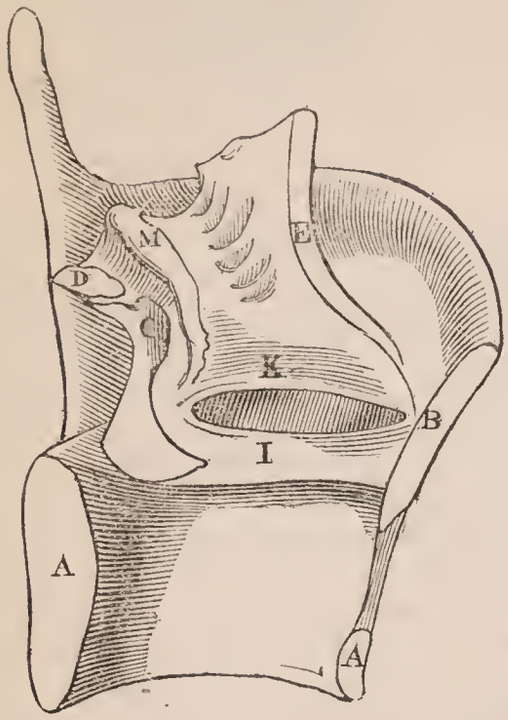
These parts have not, that I know of, been described in their true shape and magnitude; I have therefore given a figure (fig. 3) representing the cuneï-

Fig. 3.



form cartilages as seen from before and above; and another (fig. 4) repre-

Fig. 4.



senting a vertical section of the larynx, and the cartilages of one side, as seen from within. The cuneïform cartilages in the specimen from which these drawings were made were upwards of half an inch in length; their form nearly cylindrical, and their diameter four-fiftieths of an inch. At their inferior ends they became thinner, and were connected with the elastic fibres of the ligamenta superiora glottidis, where the latter are attached to the arytænoïds. Their texture is exactly like that of the cornicula laryngis. The term *cornua* laryngis would be more appropriate to them than that of cuneïform processes. Whether they are ever wanting, or imperfect, I do not know, but I suspect that this is sometimes the case.

The letters in fig. 3 and 4 designate—

A, the thyreoïd cartilage.
B, the cricoïd.
C, the left arytænoïd.
D, the left corniculum laryngis.
E, the epiglottis.
I, the left ligamentum inferius glottidis.
K, the left ligamentum superius.
M, the left *cornu*.

I remain, sir,
Your obedient servant,
HERBERT MAYO.

19, George-Street, Hanover Square,
May 2, 1832.

SALINE TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

THE result of the treatment of cholera adopted at Cold-Bath-Fields Prison, as related in the two last numbers of the Medical Gazette, is very striking, but it appears to me doubtful how far it is to be ascribed to the effect of the saline medicines. In the first of these numbers it is stated, "sinapisms were also applied as early as possible to the region of the stomach, betwixt the shoulders, &c."; and in a preceding number of the Medical Gazette, Mr. J. Earle gives an account of a plan of treatment adopted by him at one of the collieries, where the number of patients was greater, and the practice still more uniformly successful, in which, in many of the most serious cases, no internal medicine was given, but the cure committed wholly to the effects of large mustard cataplasms. Whatever might be the direct effect of the saline medicines, I have no doubt that much of their good effects arose from their superseding the employment of powerful stimulants, the secondary effects of which, in such a disease as the cholera, must be highly prejudicial. As the subject deserves the strictest investigation, Mr. Wakefield will perhaps favour us with a still more particular account of the treatment employed at Cold-Bath-Fields.

Your obedient servant,
A. B.

April 30, 1832.

POISONING WITH COLCHICUM.

To the Editor of the London Medical Gazette.

Dudley, April 19, 1832.

SIR,

REPORTED cases of poisoning by colchicum are not numerous, and I gladly avail myself of the opportunity of furnishing you with the annexed case, assured you will deal with it as its importance merits.

I am, sir,
Your obedient servant,
THOMAS FEREDAY.

David Cole, æt. 44, a stout muscular man, feeling pains in his bowels, to which he was subject, about six o'clock in the

morning of the 8th of March last, swallowed, believing it to be rum, about 2 oz. of wine of the seeds of colchicum. He immediately discovered his error, but, knowing its effects in a small dose, conceived it would be followed by vomiting and purging sufficient to avert mischief. He sought no medical advice until four in the afternoon, when I first saw him. He was sitting in a chair, his elbows on his knees, and told me that he felt no inconvenience for an hour and a half after taking the dose, when pains in the bowels came on, but that he continued his labour until eleven o'clock, when pains in his stomach and bowels, retching, and copious vomiting of a yellowish fluid, compelled him to desist.

4 o'clock P.M.—He describes the pain in the epigastrium as agonizing, and says it is like a knife piercing him. The retching is incessant and extremely violent, but no fluid is evacuated; there is tenesmus; a small quantity only of fæcal matter has passed. No tenderness upon pressure either in the epigastrium or abdomen. The appearance of the tongue is natural; the pulse small, slow, and feeble; breathing not much affected; the feet cold. His countenance is anxious; features sharp; his cheeks, lips, and palpebræ, purple. Upon attempting to walk, says he thinks he shall lose the use of his limbs. A mustard emetic was given, followed by copious draughts of warm water and gruel. These were soon returned, with apparently no mixture. Cathartic medicine was given, and immediately returned. Was put to bed; warm bricks were applied to the feet, and hot flannels to the stomach. To take forty drops of laudanum immediately; gruel and coffee plenteously.

9 P.M.—The retching, vomiting, and pain in the stomach, continue with undiminished violence; the fluid vomited contains a sediment like coffee-grounds; very thirsty; has made little water. Twenty drops of laudanum every two hours; a blister to the epigastrium; sinapisms to the feet; an enema every hour.

9th, 6 o'clock, A.M.—Has passed a sleepless night; the symptoms remain unaltered. The eyes are sunk; the feet warmer; skin generally natural; no perspiration; pulse scarcely to be felt; respiration hurried; great thirst; no urine. Enemata returned without fæcal matter.

Camphor, Calomel, and Opium, every three hours; an effervescing draught, with brandy, every hour.

8 o'clock P.M.—The retchings and pains continued until four o'clock, when the bowels were much distended. Has since had copious liquid stools, dark coloured, and very offensive, and expresses himself better. Makes a few drops only of urine; loses his sight for a minute or two after getting out of bed to the night-chair. The pulse is scarcely perceptible, and occasionally intermits. He is perfectly sensible, but talks with effort; calls continually for water.

Aromatic Confection, Carbonate of Ammonia and Camphor mixture, with brandy, every hour.

10th.—In the course of the night his stools passed involuntarily, and in great numbers; his weakness increased, and he died a few minutes before five o'clock this morning, perfectly sensible to the last moment.

Sectio Cadaveris.—The face, neck, upper and front part of the thorax, insides of the arms, front of each fore-arm, and the insides of the thighs, were covered with patches of a purple efflorescence, as were also the integuments of the scrotum and penis. The muscles of the fore-arm were very rigid, and their fibres contracted into hard knobs. The great omentum, instead of covering the front of the intestines, was turned up between the stomach and convex surface of the liver behind, and the diaphragm in front, from the efforts of vomiting. There was increased redness in a portion of the peritoneum covering the jejunum. The stomach and bowels were coated with a thick, tenacious, but colourless mucous. On a portion of the mucous membrane of the stomach, near the cardiac orifice, and corresponding to its great arch, was a patch of redness about the size of a half-crown piece: its secretion here did not vary in tenacity, quantity, or colour, from that of any other portion of the membrane. Upon dividing it at this part, its section presented nothing beyond its usual appearance; there was no pulpiness, no thickening, but a small quantity of blood was effused between it and the muscular coat, giving the reddened internal appearance. Careful examination of that portion of the reddened peritoneum covering the jejunum, demonstrated the like hæmorrhagic condition of the blood-vessels. Blood was effused between the peritoneal and muscular coats, but the mucous membrane corresponding to this portion was perfectly healthy—at least it was perfectly free from inflammation. No other trace of inflammation was observed in any other portion of the abdominal viscera. The gall-bladder was distended with healthy bile. The urinary bladder was contracted and empty.

The pleuræ costales were much reddened. The lungs were of a most beautiful purple colour externally, did not crepitate, and were gorged with black blood, which had become effused underneath the pleura pulmonales in spots of various sizes: these were very numerous about their roots and edges.

The pericardium contained no fluid, nor

was it reddened; yet numbers of ecchymosed spots were observed in that portion of it attached to the central tendon of the diaphragm, and also thickly interspersed upon the surface of the heart itself, more especially about the centre of the coronary vessels. Heart flabby, and its structure easily broken down. The cavæ, the right auricle and ventricle, and the pulmonary artery, were filled with black blood, partly coagulated, partly fluid; the left auricle and ventricle empty.

I regret that neither my own entreaties nor the influence of the Coroner, were sufficient to obtain permission to examine the head. So little were the functions of the brain disturbed during life, that it is probable no other diseased appearance would have been observed save a participation of its sinuses in the distention of the right side of the heart.

The deleterious effects of the bulb of colchicum are said to depend, in a great measure, upon the season of the year when it is collected; but the seeds, matured only in the spring, must possess uniform properties.

Colchicum is classed by most writers as an acrid or rubefacient poison, and poisons of this class are defined as producing inflammation when applied to the intestinal canal, and if taken in sufficient quantity, the same effects as the corrosive. No such effects, however, followed its exhibition in this case, for in addition to the want of the usual post-mortem appearances of inflammation, I consider the patches of redness in the stomach and peritoneum too partial for inflammation from such a cause. The jejunum, too, possesses a singular immunity from disease, as is shewn by the quotation of Andral's table in Macintosh's practice of physic. The hæmorrhagic condition of the system observed here accords with the observations of Dr. A. Thomson, published at page 281 of the Lancet for 1831, who says that colchicum has an effect of producing effusion of blood from all the mucous tissues, the skin alone excepted, and that a peculiar laxity is observable in the cellular membrane, producing a diminution, if not total destruction, of its adhesive powers. The purple efflorescence of the skin, the effusions of blood between the coats of the stomach and of the jejunum, and underneath the pleuræ and the pericardium in this case, all prove the accuracy of these remarks. I feel satisfied that not the slightest inflammation existed in either of the above-mentioned structures, for with respect to the stomach and bowel, where redness was perceptible, the mucous membrane was unchanged in structure; there was neither pulpiness nor thickening, nor did it present a more vascular section than other portions of the membrane: a very important observation, too, its secretion was not changed in quantity or consistence. With respect to the lungs, it was difficult to trace, or rather distinguish, either blood-vessels or bronchiæ: a section of them seemed more like the section of a clot of venous blood, so greatly were they congested with black blood. The state of the breathing during life bore no kind of proportion to this appearance.

Colchicum is also classed as a poison acting upon the nervous system by absorption, and through this medium exerting a peculiar influence on the arterial circulation, and this appears to be its true modus operandi. Sir Everard Home found by direct experiment* that its effects were the same, whether taken into the stomach or introduced into the jugular vein, only that in the latter case its effects were more quickly developed than in the former, and hence he concludes that its action upon the different parts of the body is through the medium of the circulation, and not from its *immediate* effects upon the stomach and intestines. The primæ viæ have been frequently, and perhaps generally, found inflamed where colchicum has been taken in large quantities; but this is no proof of its *direct* effects upon them, since arsenic will produce inflammation there (the stomach and bowels) when applied to the external surface of the body†! Sir Everard Home found that colchicum reduced the pulse in all cases where it was ordinarily used, and this is a material help in enabling us to trace the order in which the three great functions of the body are severally affected by it. It would seem that the heart is deprived of nervous energy, and in consequence, beats feebly. Now by the natural process of inspiration, the trachea and bronchiæ, with their minute ramifications, together with the air-cells, are distended with atmospheric air—an elastic fluid. At the standard of health the heart contracts with a force probably three hundred times greater than

* Philosophical Transactions for 1816, p. 257.
† Paris's Pharmacologia, 6th edit. vol. 1, p. 248.

the force exerted by the pressure of the air in the lungs, and the blood, instead of being impeded, flows more freely; hence the important connexion between the circulating and respiratory functions. We have seen that colchicum, in an ordinary dose, depresses the heart's action. What must then be expected to follow the administration of a dose like that in question, for it does not appear to be one of those remedies, the principle of whose action is altered by its dose! The action of the heart is almost paralized; it can no longer overbalance the pressure exerted by the air in the lungs; the blood can no longer be propelled as usual through their vessels; congestion takes place in the veins; the blood is unarterialized, (and whether its exposure to the action of the fibrillæ of the ganglial system of nerves, in its passage through the arteries, has any influence in effecting this essential change, the lungs are not able to exert their necessary preparatory action); and unarterialized blood gradually destroys the action of every part through which it circulates. The organs of digestion, assimilation, circulation, and secretion, were severally deranged in function, whilst the intellectual and locomotive powers were little affected. This proves the great extent to which the sympathetic or ganglial system of nerves was affected, and the almost immunity of the cerebro-spinal system.

It must be obvious that no treatment, in this case, held out any hope of the patient's recovery. When first I saw him, he had swallowed the dose ten hours, when time had been allowed, over and over again, for it to enter the circulation. An emetic, or the stomach-pump, employed before this had taken place, would have dislodged it, and probably have saved his life.

Since poisons act in so many different ways upon the animal economy, some primarily through the medium of the nerves without being absorbed, producing death by suffocation from paralysis of the respiratory muscles, or by syncope; others by entering the circulation, and exerting their influence on the heart, brain, and alimentary canal; others through the same medium on the spinal marrow; and, lastly, others having a purely local action on the mucous membrane of the alimentary canal; and, since this difference in their physiological actions renders a plan of treatment safe and justifiable in some instances, which would be followed by the most tragic effects in others, it behoves us to be aware of the order of the parts of the system through which they exert their influence; for, as each has its own modus operandi, it is by a knowledge of this only that we can arrive at scientific and safe indications of cure. Magendie, for instance, found that a state of depletion facilitated absorption, but that a state of congestion impeded it. How important a knowledge of this fact is in the successful treatment of two familiar poisons, arsenic and corrosive sublimate! Dr. Paris has classed the former with those poisons which produce their effects by absorption, the latter with those having a purely local or corrosive action. Both are followed by inflammation of the primæ viæ; yet the experiments of Magendie teach us, that if we deplete to subdue the inflammatory symptoms when arsenic has been taken, so long as any portion of it remains in the stomach and bowels, we necessarily hasten death by promoting its absorption. On the contrary, corrosive sublimate, having a local action, and not being absorbed, needs no such precaution: we may bleed from the outset.

The same principle is applicable to the poison of colchicum. It produces its effects by absorption. We should therefore be very cautious how we bleed, to subdue any inflammatory action of the stomach and bowels, until the poison has been completely ejected from them.

UTERINE HÆMORRHAGE.

To the Editor of the London Medical Gazette.

SIR,

IN page 152 of my Treatise on Uterine Hæmorrhage, the propriety of using the tampon in cases of very formidable exhaustion, is merely suggested. An opportunity having now occurred of enforcing the practice, I consider it a duty incumbent upon me to forward you the particulars of the case, in order that they may be inserted in the Medical Gazette at the time you may be pleased to notice my work in review*.

* As the case is interesting, we have thought it best to give it publicity at once.—ED. GAZ.

I say this, presuming you will think the case sufficiently important to occupy a place in your journal.

I am, sir,
Your obedient servant,
JOHN INGLEBY.

New-Street, Birmingham,
April 30, 1832.

Case of Uterine Hæmorrhage, with attachment of the Placenta to the Cervix Uteri, in which the Plug was employed with very marked advantage.

April 26th, 1832.—At the request of Mr. Heeley, surgeon, of this place, I visited Mrs. —, a very delicate woman, whom he was then attending in labour, and who had reached the full term of utero-gestation. It appeared that the patient was seized three weeks previously with a copious hæmorrhage, which subsided under medical treatment, and did not return until nearly one o'clock this morning, when Mr. Heeley was again called in. The labour pains were feeble, and the discharge very considerable; this, however, subsided at half-past two. At four o'clock the pains had ceased altogether, but the discharge had now materially increased; and as the patient had begun to vomit, a small dose of laudanum was administered. I was called to see her at seven. The bed and bedding were soaked with blood of thin consistence, and possessing little coagulation, but the active hæmorrhage had again materially diminished. The os uteri was quite lax, dilated to about the size of a half-crown, and several small coagula had collected over it. On passing my finger about an inch and a half beyond the uterine orifice, I detected an edge of the placenta quite detached. Nearly the whole of the child's head was above the brim of the pelvis. The patient was faint, cold, the countenance exsanguineous, the respiration very quick, the pulse undulating, and so feeble and frequent as scarcely to be numbered, or felt with distinctness, — though Mr. Heeley on one occasion counted 150 beats in the minute. Under an impression that immediate delivery in this state of exhaustion would be fatal*, and fearing to allow even a draining to go on, I resolved, with Mr. Heeley's concurrence, to use the plug. Having steeped a large and soft silk handkerchief in olive oil, I conveyed as much of it through the os uteri, between the fœtal head (which was rather loosely situated) and the detached portion of placenta, as seemed to be necessary. Fifty drops of laudanum, with some hot brandy and water, were now administered. In about an hour the pulse had improved, and as the plug had begun to excite uterine contractions, I thought it prudent to withdraw it, lest an additional mass of placenta might be separated by the action thus induced. I found the os internum rapidly dilating, the head had almost passed the brim, and the membranes were partially on the stretch. I immediately lacerated them, and a large quantity of water was at once discharged. It was unnecessary to apply the short forceps, for the hæmorrhage had now entirely ceased, and the pains were so efficient that, in less than half an hour from this time, the child was born living, but so feeble that the usual resuscitative means were unavailing. The placenta was soon expelled. The portion which seems to have been detached on the occasion of the first hæmorrhage was marked on its fœtal surface by a defined circular depression; this part was of an oblong form, two inches in width, and an inch and a half in depth; it presented a disorganized appearance, being yellow in patches, like curd or lymph, brown in other places, and having a few scarlet spots interspersed. A fresh placental detachment, covered on its maternal surface by a thin layer of coagulated blood, gave rise, no doubt, to the present hæmorrhage. The patient is suffering severely from the effects of the loss of blood—the disease described by Dr. Marshall Hall.

REFLECTIONS.—In the consideration of this case two points seem to suggest themselves—the one refers to the hæmorrhages being three weeks distant from each other, and the serious effect which the second flooding produced upon the system; the other has reference to the value of the plug, when used during a state of collapse.

* Although the head was entering the brim, yet from the great relaxation of parts, I could have passed my hand in utero very readily. Possibly the long forceps might have been used with effect, on account of the *dilatable* state of the os internum; but any agitation of body might have proved fatal at this juncture.

MEDICAL GAZETTE.

Saturday, May 5, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

IS CHOLERA IN THIS COUNTRY CONTAGIOUS?

If we could feel perfectly convinced that the decisions of a debating society might safely be taken as infallible tests of truth, what a world of trouble it would save us! We should then only have to repair to Sackville-Street in every dilemma, as the votaries of Apollo did of old to the oracle at Delphi, and thus be spared at once the labour of investigation, and the pain of doubt. Know, courteous reader, that the long-disputed question of contagion, as applied to cholera, has been finally set at rest—it has been decided by shew of hands;—a resolution, declaratory of non-contagion, was last Saturday moved at the Westminster Medical Society, seconded, put, and carried (*by a majority of two* *), in defiance of the president, who protested against the innovation. That this method of deciding an intricate and difficult question, so creditable to the sense and judgment of those concerned, should have originated with one professing to instruct the world on the general principles of public health †, and been adopted by a physician who has claimed for himself the designation of "philosopher" *par excellence* ‡, cannot fail to convince the profession of its superiority; nor can we imagine any process by which a decision more likely to be satisfactory could have been attained, unless the parties in question had agreed that it should rest upon the interesting chance of

"Who should draw
The longest straw."

"It was probably fortunate, (says a contemporary—speaking of the contrast afforded by the medical men in Edinburgh) it was probably fortunate for science and for humanity, that the epidemic broke out on the banks of the Thames, where a host of intelligent practitioners were constantly on the watch to mark its track:"—assuredly it was most fortunate, for whether we regard the extraordinary light which the "Association," of which the writer, whom we quote, constitutes a distinguished member, has thrown on the pathology of the disease—the skill which some of them have on divers occasions displayed in the prognosis — the improvements they have made in the treatment — or the original and unanswerable mode by which they have now decided the question of contagion, we are fain on all these points alike to confess our unqualified admiration.

The data on which we grounded our opinion that cholera was a contagious disease, were laid before our readers ere yet the pestilence had visited our shores, and we have reason to believe that they appeared to many others as conclusive as they did to ourselves. No ordinary circumstances would have induced us to recur to a subject of which we have already so fully treated, and which has recently been discussed elsewhere in a manner so disgraceful to science. Events, however, of various kinds having tended to present the question under a different aspect, and subsequent occurrences having conspired to dazzle or perplex the inquirer, we think it right to examine, in the same spirit of candour by which we have heretofore been guided, whether the recent history of cholera be

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* See the report of the meeting in the newspapers.

† See a work entitled Catechism of Health, &c. by A. B. Granville, M.D. &c.

‡ See a work originally entitled "Change of Air, or the Pursuit of Health," &c. but altered, in a second edition, into "Change of Air, or the Diary of a PHILOSOPHER! in Pursuit of Health," &c. by James Johnson, M.D. &c. &c.

such as to require any, and if any, how much modification of the opinions we have formerly expressed; and we feel it the more necessary to do this, because since we formerly addressed our readers upon this interesting subject, the weekly papers, obedient to the breath of trade, have veered right round; while certain of the medical profession having deemed the opportunity of acquiring notoriety too favourable to be lost, have endeavoured to keep themselves before the public by joining the daily press against their brethren, echoing the sentiments of their leaders, and striving to prove first, that the epidemic which has lately prevailed in London was not *the* cholera; and secondly, that it was a disease not capable of being propagated by contagion. And here again it is but just, ere we proceed to offer another tribute of applause to the profound ratiocination of our contemporaries, for while *we* were weak enough to opine, that if the epidemic were not the cholera, the phenomena it displayed could not in any way affect the question of contagion as regards that disease, *they* failed not to discover, that if the malady in London were not contagious, the malignant cholera could not be so either, even though they held it to be a different disease! Time and events, however, gradually produced the universal conviction that the disease which visited London was the same as that which had been witnessed under the name of cholera in India, Persia, and the west of Europe; and now, of course, the necessity for adopting the particular form of logic by which things assumed to be different were yet reasoned upon as identical, no longer existed. Our medical "philosophers" now returned to the old axiom, that "things (only) which are equal to the same, are equal to one another;" and denying that cholera has been contagious in England, extended this principle to the disease in other countries, on the plea of their identity. In this we readily concur, and we shall admit, though it be more than strict reasoning requires, that if it can be demonstrated that the malady which has recently prevailed in Britain is incapable of being transmitted from the sick to the healthy, then is the present pestilence exclusively an epidemic, and the cholera, in all the regions it has hitherto visited, a non-contagious disease.

To the reasons assigned for regarding human intercourse as immediately connected with the progress of the disease from India to Europe, we have nothing new to add; but with regard to those on whom the ceaseless reiteration of the newspaper oracles may have made any impression—like the drop which, though powerless in itself, wears even stone by its repetition—we take leave to refer them to our previous articles*; and we do this, not from any arrogant conceit of the reasoning they display, but from perfect conviction that the data therein detailed must lead every one that is unbiassed to the same inferences as ourselves. Since, however, the discussion has been carried into the columns of the newspapers, the statements made have been in many instances so much at variance with the facts, that they cannot fail to mislead those who trust them, and even candid men thus come at length to confound what is authentic with what is spurious; nor can they shake off the errors of partial assertions, or wilful misrepresentations, except by falling back upon more correct sources of information.

But assuming that the grounds of our opinion, deduced from the history of the disease abroad, were carefully selected, and cleared, in the most satisfactory manner, of adventitious and irrelevant growths, still it may be argued that it is unnecessary any

* See particularly No. 206, page 185.

longer to go abroad for evidence regarding a malady now unfortunately familiar to us at home; and the question naturally follows, what effect the more recent progress of cholera has had upon our opinions as to its mode of propagation? We answer, that nothing has occurred to shake our conviction that it is a contagious disease, but that our estimate of the *degree* of its contagiousness, is very much reduced. We had anticipated, that when it came to this country the doctrine of contagion would have been extended and confirmed, whereas just the reverse has happened; and though a highly-intelligent correspondent has objected to an assertion we made a few weeks ago, namely, that "in London the evidence of contagion has often been wanting," still we must hold to this opinion; for as we believe that the disease is capable of being transmitted from the sick to the healthy, so also we must say that we have seen cases in which no source of contagion could, by the most minute and careful investigation, be detected. The inference which we draw from such instances is not that cholera is non-contagious, but merely this—that it may also originate in other causes besides preceding cases of the malady. As the first person whoever had small-pox, measles, or scarlatina, could not have taken them by contagion, so we conceive that the causes which originated them, may still, under certain circumstances, generate them *de novo*. To this idea it has been objected, that it is "unphilosophical" to admit two causes as productive of one effect; but we are little disposed to subscribe to that philosophy which would set up its own contracted view as the boundary of nature. The truth of the proposition that small-pox, and the other exanthemata, originated at some time or other, is intuitive: they could not in the first instance have been received by contagion, and yet they propagated themselves in that manner. So it may be still with them, and so we conceive it to be with cholera, just as we know it is with other diseases—ophthalmia, for instance—which often originates in accidental causes, and yet becomes contagious. If the being contagionists, obliged us to say (as we perceive some have done) that the disease spreads by contagion and only by contagion, then, indeed, we should find it difficult to defend our creed. Let us not be misunderstood: we have no doubt whatever of the existence of contagion as regards the malignant cholera, for we look upon it as resting on the same grounds as the avowedly contagious diseases; but we think, that though contagion be necessary, it is not all that is necessary to the explanation of the phenomena.

Some persons have an idea that where contagion really exists, it must manifest itself by such outward and visible signs as to leave no room for doubt; but on this point it may be sufficient to remark, that Sydenham seems to have had no notion that small-pox was contagious, and that this very malady had prevailed on many occasions, and during many years, before the mode of its propagation was even suspected;—yet the reality of its contagious nature is now not only universally admitted, but held to be among the few unquestionable principles in medicine. The fate of cholera at the present day, therefore, is no more than that of small-pox formerly was, and it would be unreasonable to expect that the proofs regarding it should be stronger. Now the evidence by which the transmission of malignant cholera, as it has shewn itself in this country, from the sick to the healthy, may be proved to the satisfaction of any candid and unprejudiced person, is of the kind which follows:—

Towards the end of January, two persons met at a hostelry in Morpeth,

a small town in Northumberland, which the epidemic had not then reached: one had come from Hawick (a distance of fifty-five miles), to sell cattle — the other had just arrived from Newcastle, in which cholera was at that time prevalent. During their short stay, the latter was seized with cholera, and died; the former straightway returned to Hawick. He had scarcely reached home ere he was taken ill with the same disease, and which speedily proved fatal to him. His brother, his nephew, and his servant, all in direct communication with him, were next successively attacked; his clothes having been sent to a woman to be washed, she was also seized, as were next her child, and subsequently her husband—who died. The medical man who had been called in to attend these cases was then attacked, and, in forty-eight hours after, his sister, who resided with him, was also taken ill. Although the place contains five thousand inhabitants, only seventeen persons in all were affected; and of those, every individual had been exposed to whatever influence communication with preceding cases of the malady is capable of effecting. We have thus a healthy person, going to a distant town where cholera is unknown—residing in a public-house with a traveller from an infected district, who sickens and dies; we have him returning home to a retired country town, bearing with him a malady with which those only become affected who have intercourse with him mediately or immediately, while the rest of the inhabitants remain free from the epidemic or any other form of the disease. These facts rest on authority which is unimpeached, and, we believe, is unimpeachable *.

Again: a woman (A) being taken ill with cholera (April 5th), was removed to her brother's, in Halfmoon-Street, Bishopsgate; the disease not having hitherto shewn itself in that vicinity. She occupied their only room, and slept first in the bed of one of the boys, but, this being found too small, was subsequently placed in that of her brother and his wife. The boy who had been displaced, gladly took possession of his bed again, the sheets, we are expressly told, having been suffered to remain. As the night advanced, another change was adopted—a temporary bed was placed before the fire, for the invalid, she was removed to it, and her brother now occupied his own bed, thus set at liberty, while his wife remained in attendance on the patient. Next morning their visitor was taken to the Cholera Hospital, and the family returned to their usual habits, no change or purification of bed-clothes having been adopted. April 7th, (B), the boy whose bed the original patient (A) had occupied, was seized with cholera, and died. Same day (C), his father, was taken ill, and died. April 8th, (D), daughter of (C), was seized. April 9th, (E), another daughter, was also seized, and died; while (F), the wife of (C), had the premonitory symptoms, but recovered under prompt and efficient treatment. Here are six cases occurring in a street where no other instances of cholera had taken place; they are confined to the second floor of the house,—those above and those beneath being alike unaffected. Still the evidence of contagion did not satisfy the neighbours; one of whom, occupying a different part of the same house, took compassion on the widow and her orphans, received them into her apartment, and on the 19th she herself died of cholera, being, as the intelligent narrator justly designates it, "a martyr to the doctrine of non-contagion*."

Instances of this nature might be multiplied, but we have selected the

* See Med. Gazette, vol. ix. p. 823.

* See Mr. Tweedie's paper in our last number.

two preceding because they were originally recorded in this journal; and the reader will, therefore, have it in his power to refer to them, and satisfy himself regarding the details. For ourselves, we must say, that we cannot conceive any stronger evidence of a principle, which, as Dr. Bancroft truly remarks "can neither be seen, heard, nor felt:" indeed, there is one method, and only one, so far as we can conceive, of getting over such illustrations, and that is, by denying the facts. But to deny and to disprove are two very different things; and we are convinced, from many circumstances, that a minute investigation of the cases which have occurred in England will much more frequently detect previous communication with the infected, where it had not been suspected, than lead to the discovery of misstatement in cases where this is said to have taken place; a fact of which Mr. Tweedie has given a remarkable illustration in our preceding number.

Now, in order to bear out the doctrine of contagion, one case is as good as a thousand. No quantity of negative evidence can undo it: demonstrate that one person has become affected with cholera in consequence of some influence transmitted by an individual already labouring under that disease, and afterwards shewing that ten or ten thousand have been similarly exposed with impunity, leaves the first case quite untouched. Hence, also, the futility of arguing from cases in which inoculation has been hazarded with impunity; and hence the absurdity of drawing any inference against contagion from the supposed *experimentum crucis* of the nasty—"philosopher," we suppose he will expect to be called—at Glasgow, who lately got into the reeking bed from which the body of a cholera patient had just been removed. Most of our readers must remember that Dr. J. Hunter mentions an instance of twenty persons being bitten by a mad dog, and that yet only one of them became rabid; but no person would argue, from the escape of nineteen, that the twentieth had not had hydrophobia, or that the disease was not contagious. However, the existence of infection being granted, there still remains to be considered the extent or intensity of the principle. A single case settles the question of entity; the consideration of the frequency or infrequency of the event is merely one of numbers; and these two considerations are as distinct, and ought to be as much separated in our minds, as qualitative and quantitative analysis in chemistry. It is in this view that the history of the epidemic in London and Paris becomes so interesting; shewing, in the most striking light, the control which other circumstances, (concerning many of which we are probably quite in the dark,) exert over the progress of this extraordinary disease. Our space, however, compels us to leave this part of the subject, that we may allude to another point more immediately connected with the preceding observations.

It is objected by some, that when a disease is really contagious it progressively extends from any given point, and does not manifest the same uncertainty, and we might almost say caprice, in its movements which marks the progress of cholera. This observation, however, is one which springs entirely from the tendency which exists to make assertions in keeping with what we think likely to be the case, rather than take the trouble to inquire into what has actually occurred. Nothing can be more irregular and uncertain than the phenomena of the most infectious diseases in these respects: small-pox, for example, has been extremely fatal at Brighton during the present season, while many places in the vicinity have re-

mained nearly or altogether free; and the insusceptibility to this generally so communicable disease, sometimes extends to whole districts.

"I have sometimes observed," says Van Swieten, "large towns to be free from small-pox, whilst it raged epidemically in the neighbouring villages; and, on the contrary, some large towns *universally* visited by the complaint, *whilst the villages in the neighbourhood remained in health, though the inhabitants of both mixed daily with each other.*"

The same celebrated writer adds, that he had two patients of his, labouring under small-pox, carried into a large town, without propagating the disease.

Sir John Pringle says,

"The small-pox being carried into a camp by some new-raised recruits, quickly disappeared, without becoming general, although it is notorious that other camp diseases are but too apt to spread themselves."

Again, Dr. Haygarth gives the following account, on the authority of Dr. Otter, of Geneva:—

"We have frequently inoculated, at Geneva, a great number of children, in the years during which small-pox was not epidemic; those children have gone out every day, even after the eruption had broken out; they have been in the streets and in the public walks; they have communicated freely with other children susceptible of the infection; and not only the small-pox did not spread, but there did not occur, to my knowledge, any distinct instance of the communication of the disease from one individual to another in the streets or promenades."

Numerous examples of the same kind are to be found in the records of medicine *, but what we have given are more than sufficient to destroy the force of any argument against the contagion of cholera, deduced from the supposed necessity of its spreading wherever it has once been introduced.

A much more formidable objection to the idea of exclusive contagion, is the extreme rapidity with which cholera sometimes extends over a town or district: as, for instance, in Paris, where we learn, from direct sources of information, it appeared simultaneous in several different quarters. An event such as this almost amounts to demonstration of something more than contagion being concerned; at the same time it is quite obvious that the precipitate declaration of the medical men, and the manner in which the public was admitted to the cholera wards in the hospitals, were proceedings pre-eminently calculated to spread the disease; and when we consider that Perier grasped the hand of a patient dying of the pestilence, and that soon after he himself, his wife, his son, and his sister-in-law, all became affected in succession, we cannot but admit the probability of the disease having been communicated from one to another.

In a discussion of this description, it is extraordinary how common it is for controversialists to become partizans — pertinaciously regarding one side of the question only; and never was this tendency more conspicuous than on the present occasion. The time, we fear, is yet distant when a dispassionate and philosophical view will be taken by the disputants; but when it does come, we are sure that some will look back with regret on the unwarrantable manner in which they have attempted to make the occurrence of such a visitation the stepping-stone to notoriety, frittering away, in the eyes of the profession, whatever reputation they may have previously possessed. We cannot do better than conclude in the words of Dr. Hancock*.

* For some interesting observations on this subject, and much important information on the question of contagion generally, we refer to Dr. Elliotson's Lectures in the preceding and present numbers of the Gazette.

* The reader will find some excellent observations connected with this subject in the pamphlet alluded to, entitled "The Laws and Progress

"There is such a disposition among medical men to adopt an exclusive opinion on one side or the other of this question, that they will neither make terms with their opponents, nor with any who do not go as far as themselves. In short, they will insist either that the epidemic cholera is entirely propagated by contagion, or entirely atmospherical. And, seeing that *eye-witnesses*, or, more properly, observers on the spot, give us contradictory statements in regard to the same event; those who are at a distance are the more at liberty to canvass the subject, according to the best evidence they can procure. Without prepossession, therefore, it would seem reasonable to conclude, that, when both parties carry their opinions to the extreme limit, they are wrong, and that both have some truth in them."

INSTANCE OF THE DEVASTATION OF CHOLERA.

A GENTLEMAN just returned from Paris, where he resided during the hottest of the irruption of cholera, has given us the following illustration of its ravages. A person of his acquaintance kept a lodging-house, entrusting the care of attendance, as is the custom in Paris, to a man who waits upon all the tenants. About a fortnight after the disease had broke out, the porter brought the key of the house to his employer, and told him it was empty. It had been occupied by ten lodgers from different parts of the world, every individual of whom had been cut off by the malady—not one was left to transmit the tale to his distant relatives!

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of the Epidemic Cholera;" and a more elaborate and detailed account of malaria and contagion in the "Dissertations" of Dr. W. Aiton, to whom we have been indebted for many valuable remarks.

COLLEGE OF PHYSICIANS,

Monday, April 30, 1832.

SIR HENRY HALFORD, BART. PRESIDENT, IN THE CHAIR.

Two very interesting papers were laid before the meeting (which was very numerously attended): the first, from the pen of Dr. Heberden, was read by the Registrar. It was entitled,

Some Observations on the Subject of Infection.

After alluding to the difficulties which have generally been experienced in the investigation of the subject, the author suggested that those difficulties might be considerably obviated by extending our views from individual cases to whole communities, and thus estimating the course of a disorder on a large scale. With respect to the cholera, there was nothing peculiar in the pains taken by a large portion of the public to persuade themselves not only that it was not infectious, but that it did not at all exist among them; the same thing occurred more or less in all plagues, and at all times: at first the complaint is not known—then it is not acknowledged—until a desolating mortality shall have fearfully opened the eyes of all. In the plague of Marseilles (1720), a deputation of physicians from Paris professed to have ascertained by personal research, that the plague was by no means infectious. The influence of the seasons, of particular localities, and of certain habits of body, as influencing the nature and progress of a disease, were then touched on; and the author shewed, that it was not the presence of any disease alone, nor of any particular state of the atmosphere, that occasioned a spreading sickness. Among children of the same family, one or more shall have hooping-cough or measles, and the others escape,—yet nobody infers from this that hooping-cough or measles are therefore not infectious; neither should similar instances of individual escape from prevailing disorders induce any sensible man to withhold his credence from the possible infectious nature of those disorders. Dr. Heberden then gave an account of his own seizure with a peculiar spasmodic affection of the larynx, from anxiously hanging over one of his children who was affected with croup, precursory of measles.

A disease like the cholera, which has been fifteen years traversing a portion of the surface of the globe, from China to Ireland, must be dependent for its propagation on some other source than peculiarity in the state of the atmosphere; spreading, as it did, from the rising to the setting sun, from the torrid to the frigid zone, and over every variety of climate and country. The whole evidence and history of the disease prove that it is communicable by infection from man to man. It was not true that in India nobody believed it to be infectious; besides the doctrines to the contrary in Dr. Macmichael's pamphlet, he (Dr. H.) would refer to Sir Thomas Monro's Life. That able officer died of the complaint in India, and it is stated, that, from extensive inquiry, he had convinced himself that it was infectious. To some of his friends who lingered in his tent, saying that they had no apprehensions of infection, he repeated his usual observation: "That point has not been determined; you had better be on the safe side, and leave me."

The author then went on to say that he felt himself bound to lay before the College a testimony that singularly bore against his own position. In consequence of some rumours that had reached him with regard to the appearance of cholera in the Millbank Penitentiary—a place, the very principle of which is seclusion from all communication—he called upon Captain Chapman, the governor, and from him ascertained that the rumours were quite correct. This was on the 3d of April; and six well-defined cases had occurred within the preceding ten days. Three of those cases had proved fatal—one in ten hours, the other two in about fourteen. Of the remaining three, one died subsequently, after eight days' illness; and there was a new case developed the day after Dr. Heberden's visit. Now the prison had been, up to the appearance of those cases, perfectly healthy, and quite free from bowel complaints; but cholera was said to be in the neighbourhood. But in reply to those who would urge this as a refutation of the doctrine of infection, Dr. H. could mention a case of *small-pox* which appeared likewise in the same Penitentiary about the beginning of last year, and a short account of which had been read on a former occasion to the College by the President. The subject was a man who had been several months in the prison, and had had no communication personally or by clothes, or by letter, for a length of time exceeding all possibility of its introduction by such means; nor had any case of small-pox been in the Penitentiary, but the disease was rife in the neighbourhood. There was predisposition, undoubtedly, in the prison, as there must always be in every such place, no matter how cleanly and well-ventilated it might be kept; and Dr. Heberden thought that it was possible to express mathematically the data requisite for the taking of every infectious disorder: if a stood for the infectious matter, and b for the predisposition, then might $a\,b$ represent the influence which would be always capable of producing the disease, whatever might be the value of a and b—provided their product was a given quantity.

In conclusion the author made some remarks on disinfection and prevention, and, for the former object, extolled the efficacy of heat, and the methods of applying it recommended by Mr. Lind and Dr. Henry. He also mentioned a case in which a gentleman, the owner of a school near town, was almost driven to despair by his house being infected with scarlet fever; until, by direction of Sir H. Halford and the author, all the soft goods in the establishment were either boiled or baked, subsequently to which the disease returned no more.

The second paper was,

On Phlegmasia Dolens in the Male. By Sir H. Halford.

This disease (the learned President began by observing), which was so long supposed to be peculiar to women, and, indeed, to be immediately occasioned by a deposit of milk, has of late been tested by a more exact pathology, and attributed, with every appearance of truth, to an inflammation of the veins of the pelvis. But if this view of the nature of the complaint be correct, there should at least be occasional instances of its occurrence among males; and Sir Henry was much mistaken, he said, if he had not seen three such instances of it within the last few years. Two of them he would take leave to lay before the College.

The late Earl of L. (Liverpool) laboured under the complaint for several years anterior to his death, and bore

the marks of it to the last in a swelling of the left leg and thigh, with a varicose state of the veins from the ankle to the groin. The symptoms were palliated from time to time; but the obstruction to the circulation of the blood, occasioned by the original inflammation, without doubt, was the prime cause of that disease of the brain which first incapacitated the noble patient for the business of the state, and ultimately deprived him of life. He had been subject to congestions in the liver for some years; but the extraordinary state of his pulse was what attracted Sir Henry's special notice. It used to beat but 44 pulsations in a minute, a circumstance which induced Sir Astley Cooper ingeniously to surmise that the external iliac vein of the side affected was obliterated. And this proved to be precisely the fact, upon examination of his Lordship's body after death. The left external iliac vein was impervious for several inches, and, what is more, the corresponding vein on the right side was ossified.

Sir Henry Halford thought it not improbable that the apoplectic seizure which Lord L. suffered a year before he died was owing to the obstruction thus produced, and that a large accumulation of blood in the sinuses of the brain, (arising from its difficult ingress from the cava into the auricle, in consequence of the heart's deficient action) occasioned an effusion of serum in the head. This, too, was found to be the case. Above four ounces of serous fluid were effused into an unnatural cavity in that organ. Imperfection of his sight was one of the things his Lordship complained of before his apoplectic attack: he remarked that he lost a word or two in every line he read. His speech failed him almost entirely after the attack: epileptic fits at intervals supervened, and it was in one of these that he expired.

The other case related by Sir Henry, occurred in the person of "an officer of high military reputation." The patient had been ill of an inflammatory affection of the chest, which was in course of treatment, when he began to complain of acute pain about the liver. This, again, was followed by a new complaint—a deep-seated pain in the left groin. Sixteen leeches were applied, and the part was fomented; but on the following day the thigh and leg were considerably swollen; some knots were felt in the course of the veins, and the lymphatics were manifested by red streaks. Leeches and cold lotions were applied three several times, principally on account of the pain; but though the anguish was allayed, the limb has ever since continued somewhat swollen. The inconvenience, however, is not considerable; and now the only thing that Sir Henry regards as serious in this officer's case, is a notable intermission of the pulse, which he cannot help looking upon with suspicion. But hitherto no cerebral symptoms have made their appearance.

Sir Henry Halford concluded his interesting paper by some short general remarks on inflammation of the veins; and by suggesting whether it would not be worth the labour, on the part of practitioners, to trace the connexion (if there were any) between the irregular intermittent pulse of declining life and some past unheeded inflammation of an important vein, occasioning an obliteration of its channel.

ZOOLOGICAL SOCIETY.

April 30, 1832.

Report for the year 1831.

The annual meeting of this society was held on Monday last in the Royal Institution—the president, Lord Stanley, in the chair. The report was most satistory. It will be recollected that the society was only formed in 1827; in that year, it appears, its receipts amounted to 4079*l*; in 1828 they were 11,515*l*.; in 1829, 13,991*l*.; in 1830, 15,806*l*.; and in 1831 they were 17,662*l*. The number of visitors to the gardens in 1830 was 224,745; and during the past year, 258,936, which brought in an income that amounted to 11,425*l*. The present number of Fellows is 2074. Nine acres and a half of additional land in the Regent's Park have been obtained through the Commissioners of Woods and Forests; and a long list of rare and valuable animals and birds has been added to the collection during the year, including the splendid donation of his Majesty of all the animals belonging to the Royal Menagerie of the Tower.

BROUSSAIS ON CHOLERA.

The *Physiologiste* of the Val-de-Grace has given two lectures on cholera, which seem to have created a sensation in Paris; they have been published in a cheap form, for general circulation, and will amuse, no doubt, where they will not instruct. Here are a few of M. Broussais' points:—

He thinks that the cholera has often appeared in the world as an epidemic, and that the *black plague* of the fourteenth century was one of its visitations.

There was no *grippe*, he says, precursory to the cholera in the Val-de-Grace; but for five weeks previous to the breaking out of the disease there, a great susceptibility of the digestive apparatus was observable, and tartar emetic could scarcely in any instance be safely prescribed.

Cholera is not a *contagious* disease, because it has seized persons who have not come *in contact* with people labouring under it. But it is *an infectious disease*—infectious, however, in a manner that it is impossible to characterize. Some facts connected with the mode of propagation of the malady, he thinks, are perfectly puzzling.

The moral affections have much to do with its spread; and fear, above all the passions, especially.

An illustrious personage, a stranger of distinction, (whose name M. Broussais did not wish to make known) had been watching for a long time on the map the progress of the Indian cholera. Several times a week for above eighteen months he had his physician with him, and at every visit was sure to point out what new ground the cholera had been lately making. It was his constant study to calculate how long it would be before it would reach such and such a place, and finally, when it would be in France. At last come it did, and then, said he, "here is the disease in Paris, as I predicted; and now I shall surely be attacked." He went on watching with anxiety the daily returns of the mortality, and for some time was not affected. But the preliminary diarrhœa at length seized this great personage; and though the earliest assistance was afforded him, nothing could arrest the fatal termination of his complaint.

"The cholera is a disease essentially inflammatory. It attacks the whole length of the alimentary canal in every instance—no part of the canal is exempt from the inflammation." Such is M. Broussais's main proposition. "I insist," says he, "upon this proposition, though I am aware that it is at variance with most accounts of the disease. I insist upon it because it is true, and because it seems to verify several important points."

But cholera is not mere inflammation; there is a something superadded—a cause unknown—"which," says M. Broussais, "I would compare with the cause unknown of small-pox: all, be it recollected, that we can *see* in either complaint, is inflammation.

"Cholera, in short," he continues, "I hold to be an universal inflammation of the inner membrane of the digestive tube—the primary determining cause of which inflammation I know not, though I am quite satisfied as to the predominant and subsequent causes."

As *we* understand M. Broussais, the cholera is a something superadded to *gastro-enterite*.

M. B. is in a rage with the English treatment of cholera, because it is so thoroughly infected with *Brunonian* principles! He describes the English treatment as purely stimulating. "It was adopted originally by the English," he says, "in their factories and possessions in India,—and that for this simple reason: Brown's system is the prevailing one in England, and the practitioners who go out from that country have no other system to go by, in whatever part of the world it may be their lot to practise!"

So much for the learned ignorance of the Val-de-Grace professor; but we have no more room for him at present.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

SILEX IN THE URINE.

The circumstances which give rise to the appearance of silex in the urine, are enveloped in the utmost obscurity. I believe it has not been observed to separate from the urine (after being passed) in a crystallized form spontaneously, nor can it be effected by art. I certainly once observed a deposition of something like crystallized silex on the sides of a tall jar, in which the urine of one of the patients, whose case I have described at length in an earlier number of this journal, had been suffered to stand for several days; but I must observe upon this subject, that, owing to an accident, I had not an opportunity of verifying my supposition by a chemical examination. The quantity observed in the present case is so minute,

that possibly it may be looked upon rather as an accidental ingredient. To this, however, it may be objected, that its appearance in the crystallized form is not exactly compatible with such a view. That pulverulent, or finely comminuted silex, might be introduced with drink into the stomach, and pass (in the gelatinous state), as fluids frequently do, to the kidneys by some less circuitous route than the circulation, it is possible to conceive; but that it could pass in a crystallized state, either by this shorter route, or through the more circuitous one of the circulation, I think will not readily be admitted. There is no other way, then, of accounting for the appearance of this singular deposit, but its secretion by the kidney, and its separation, as other morbid concretions. Nor is there more difficulty in conceiving the kidney capable, under certain circumstances, of such an elimination, than an operation of which there can be no question—the formation of the cystic oxide.—*Journal of the Royal Institution of Great Britain.*

AVERAGE OF AGES.

According to M. Moreau de Jonnès, Ireland is the country in which there exists the greatest number of individuals under 20 years of age; they being as 1 to 2, or composing one-half the population: while in France they are only in the proportion of 1 to 3; and in Paris especially, only as 1 to 4. Children under 5 years of age bear, in Ireland, a proportion to the population of 1 to 5; in London, of 1 to 7; in Paris, of 1 to 14; and in all France, of 1 to 8. The proportion of individuals between 20 and 30 years of age, M. Moreau finds to be very nearly the same in each country of Europe, forming one-sixth of the population. In Paris they constitute one-fifth of the population. It is at this period of life that man is best enabled to resist the causes of destruction. Of persons between 30 and 40 years is composed one-seventh of the population of France and Sweden; and one-eighth of that of England and Scotland. One-eighth of the population of France, and one-ninth of that of England, consist of individuals between 40 and 50 years of age. In France, the proportion of persons between 50 and 60 years is as 1 to 11; in England, as 1 to 15. France possesses a greater proportion of individuals between 60 and 80 than England; while the proportion of those between 90 and 100 is the greatest in England. M. Moreau finds, that of those between 60 and 70 years, the proportion is twice as great in Paris as in London; and that of individuals between 90 and 100, the proportion is six times greater in London than in Paris. M. Moreau therefore concludes, that the population, or that composed of individuals between 15 and 60 years of age, constitutes, in France, two-thirds of its inhabitants; while in every other European state it forms but one half. Hence it results that France, from an equal population, is able to bring into the field more soldiers, by one-eighth, than England.

LIABILITY OF FRENCH ACCOUCHEURS.

A case was decided lately, before the tribunal of Domfront, against a M. Hélie, an accoucheur. This gentleman attended a patient whose labour proved to be very difficult: there was an arm presentation, and the practitioner thought it necessary, in order to facilitate the delivery, to cut both arms off. The child, however, which was supposed to be dead, survived; and an action was brought by the parents against M. Hélie for pecuniary compensation. The Academie de Médecine being consulted, gave an opinion decidedly in favour of the defendant; but the tribunal held otherwise, and amerced M. Hélie in the whole costs (said to amount to a considerable sum), and to pay the child the sum of 300 francs per annum for life.—*Gazette des Hopitaux.*

PECULIAR AFFECTION OF THE ORGANS OF TASTE.

Dr. Chandler Robbins relates in the Boston Medical and Surgical Journal, for May 31st, a case in which the sense of taste on one side of the tongue was impaired by want of exercise. It occurred in a lady who had one of the left molares so far decayed as to be extremely sensible to slight pressure, yet so sound in other respects as to dissuade her from parting with it; she therefore became accustomed to throw the entire burden of mastication on the teeth of the right side. Liquids, too, whether hot or cold, were at first cautiously, and at length habitually, passed through the mouth, without coming in contact with the decayed tooth or its immediate neighbourhood. This state of things having continued about two years, it became necessary to extract this tooth. When the soreness of the gum had abated, and she began to use that side of the mouth, she was surprised to find that articles of food which were ordinarily pleasant to her, assumed a different and a disagreeable flavour when masticated there. The same was true of tea, coffee, and other liquids; and to this day,

which is about a year from the extraction, the sense of taste on this side is both impaired in acuteness, and, what is still more remarkable, fails to perceive the true flavour of whatever is subjected to its action.

Dr. C. was at first inclined to suspect there was more fancy than philosophy in this lady's account of herself, although, so far as he had observed, she was the least of all persons given to vain imaginings; but further observation and inquiry have convinced him that the facts are as above stated; and he is the more confirmed in this opinion by the subsequent occurrence of another case precisely similar in all its details, excepting that it occurred in an old lady, whilst the subject of the former was young.

NOTE FROM DR. GREGORY, OF EDINBURGH.

To the Editor of the London Medical Gazette.

Edinburgh, 24th April, 1832.

Sir,

Allow me to trespass on your valuable pages for the purpose of correcting a mistake, which occurs in a paper, by Dr. Robertson, on the Salts of Morphia, in the last number of the Edinburgh Medical and Surgical Journal,—a mistake originating in my own inadvertency, and which might injure a gentleman personally unknown to me, but for whose character I have the highest respect.

Dr. Robertson, in giving an extract from a letter written by Dr. Loudon, mentions him as an eminent practitioner in Cheltenham. Now Leamington ought to have been the place mentioned; and it is because Dr. Loudon's friends might imagine he had left Leamington, which is not the case, that I have taken the liberty to trouble you with this.

I remain, sir,

Yours, &c. &c.

William Gregory, M.D.

NOTE FROM PROFESSOR BADHAM,

RESPECTING

TWO RECENT COMMUNICATIONS IN THE MEDICAL GAZETTE.

To the Editor of the London Medical Gazette.

Sir,

I beg to intimate that, together with the papers you have been so good as to insert from time to time in the Medical Gazette, which were furnished by, and properly attributed to, my son, David Badham, M.B. the recent paper on *Suppuration* (which would pass for *mine* but for this letter) claims the same authorship. You have been led into this error by its appearing to be the report of "a lecture;" but the lectures on the Theory of Medicine were all delivered by him during the last session of the University.—I remain, sir,

Your obedient servant,

Charles Badham,

Professor of Medicine in the University of Glasgow.

West-End, by Southampton, May 1, 1832.

REPORT OF CHOLERA, UP TO FRIDAY, MAY 4, 1832.

New cases in London since our last report	35
Deaths	22
Total number of cases in London since the commencement of the disease	2567
Deaths	1356
New cases in other parts of Great Britain since our last report	442
Deaths	202
Total number of cases throughout Great Britain since the commencement of the disease	11805
Deaths	4753

In Paris the cholera is rapidly decreasing; in Dublin, and other parts of Ireland, we regret to say, it is on the increase.

The number of persons attacked with cholera in Hungary is officially estimated at 538,339, and the deaths at 237,408. The disease has at length ceased in that country.

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

April 1832.	Thermometer.		Barometer.	
Thursday . 26	from 36	to 45	29·73	to 29·79
Friday. . . 27	35	53	29·84	29·79
Saturday . 28	28	53	29·73	29·63
Sunday . . 29	36	57	29·53	29·49
Monday. . 30	39	60	29·30	29·44
May.				
Tuesday . 1	32	55	29·40	29·29
Wednesday 2	40	57	29 32	29 38

Wind variable, N.E. prevailing.

Except the 28th and 29th, cloudy; with frequent heavy rain.

Rain fallen ·925 of an inch.

Charles Henry Adams.

NOTICE.

"A Country Bookseller." The Gazette is published *regularly* every Saturday morning at nine o'clock. If he does not receive them with the parcel of that day, the fault must be with his agent.

ERRATUM.

In our last number, last page, *for* Sir J. Chermside, *read* Sir R. Chermside.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 12, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Part I.—Lecture XXXI.

Continued Fever—continued.

In considering, gentlemen, the contagion of typhus fever, I thought it right to begin with some general observations on contagion—I endeavoured to define what is usually meant by contagion—and I afterwards mentioned a number of circumstances in which contagious diseases differ from, or bear a resemblance to, each other; and a number of classes into which different contagious diseases might be arranged.

I mentioned that there was a great difference of opinion as to the contagion of this particular disease—typhus fever; that it undoubtedly was not contagious to the extent that many believed; that great cleanliness, with plenty of ventilation, will dissipate the disease, so that no practitioner, if in good health and spirits, and well fed, need be afraid of it unless it exists in great intensity, and the emanations are applied to him in a concentrated state. In order to imbibe the disease, if the practitioner be in good health, and there is free ventilation, he must go so near to the patient as to inhale his exhalations in full concentration; but if he be out of health, the disease intense, and ventilation indifferent, he may easily catch it.

I mentioned that this must make a great difference in determining whether the disease is contagious or not, for you find ten thousand instances in which the disease does not appear to be contagious; yet it is to be considered, that in these cases, if the bystanders had been predisposed by debility, &c., and there had been no ventilation, it might then have proved contagious. The question, however, must be determined by a great number of cases.

I mentioned many mistakes that had been made relative to some diseases which were said not to be contagious, but which we now know to be so, and *vice versâ*. I felt it right to tell you frankly that I never myself saw an instance of typhus fever being contagious; but I stated that I understood that at the fever-hospital every officer and servant had had it, whether medical or civil, whether male or female—that it had gone the round of all, and that a great many had died of it. The difficulty here might be in determining whether it is from the situation of the place or from the patients, because all these persons have caught it at the place. But in answer to this, it is urged, that at another building on the same grass-plat—the Small-Pox Hospital, typhus fever has never appeared, and that the fever of the place had been taken to it. I mentioned that many persons who have caught the fever at the hospital have carried it home and given it to others who had not gone near the hospital. Whether, however, this were the case or not, there are instances without end of persons having visited patients with the disease, and then given the disease to others where they have gone.

I mentioned as to dirt and filth, that it is quite certain that mere crowds of people will not, by their closeness and their stink, produce the disease, because we have numbers of instances on record of persons being crowded together, and yet never having fever. If there be no other circumstance to throw them out of health, if they be all fed well, and are in good spirits. it does not appear that the crowd and stink of human beings will produce the disease. Neither does it appear that merely the putrefaction of dead animal matter has the effect of inducing typhus fever, because many persons are employed in the most offensive occupations with putrid animal matter, and yet fever never breaks out amongst them if they possess

other causes of health, and are well fed and cheerful. But if these persons, in the midst of stench, were in a state of famine, and consequently in great depression of mind, then in all probability fever would occur. Those who are interested particularly with this point will immediately remember the examples adduced by Mr. Thackrah, in his work on the diseases of the different occupations of life. Killing-butchers are exposed to more or less stink; glue manufacturers, buckram makers, and tallow-chandlers, are all exposed to stink, and yet they enjoy excellent health—are ten times more healthy than bakers. Orfila mentions the healthiness of knackeries, where there is immense putrefaction in summer, and the soil has been saturated for years.

To shew that these exhalations do not hasten the decomposition of even dead animal matter, the following fact is mentioned. In the neighbourhood of a dissecting-room behind the Hôtel Dieu, where the stink was so great that after numerous petitions the dissecting-room was removed, ragouts, bouilli, and all sorts of good things, kept as sweet in the midst of this stink, in a place accessible to this horrid smell, as in any other situation. A veteran of the imperial guard turned gut-maker, and said, that, although his premises were truly offensive, filled with enormous masses of corruption, yet nothing spoiled on his premises.

It cannot, however, be denied that although these things do not generate fever, yet they operate strongly when other causes come into play: though they all may be thrown off as water is from an oiled surface when a person has no other cause of ill health, and is well supplied with good food, and has good spirits, yet they do produce mischief when other causes are combined with them. It is also to be insisted upon, that if actual contagion be introduced into a place which is offensive, then these things appear to tell, and the mischief is greater, on account of the closeness of the situation and the exhalations from the putrifying animal matter.

Can Contagion be generated de novo?—Now there is another question, whether this contagion, (allowing that it is a contagion, because I do not feel myself authorized to speak in the positive manner on this subject that I do on some others, having never seen the disease contagious, and only placing reliance on the accounts which have been given us,) there is another question—whether this contagion may be generated afresh? There is no *proof* that certain contagions can be generated afresh, but others unquestionably may. If there be such a thing as contagious typhus fever, can it be generated *de novo?* Having never seen the disease contagious, I cannot say whether that is the case. Want of good food, want of rest, closeness of situation, depression of spirits, exposure to bad air, may, by all conspiring together, occasion fever; but whether they occasion contagious fever, I do not know: I have never seen an instance of it. I have seen many young men ill from close studying, from being much in a dissecting-room, being very anxious, especially when they came fresh from the country, not accustomed to a dissecting-room, nor to hard study, and being very anxious about their studies. Young men, when about to pass examination, when their anxiety has been so much the more increased, have certainly had fever, but I never saw their disease spread contagiously. Others say that they have seen it spread under such circumstances: they tell us typhus is a disease, the contagion of which may be generated *de novo;* but, though this is highly probable, yet one should always be cautious in listening to assertions in medicine, because so many assertions are made without any ground whatever.

With respect to persons being crowded together, I mentioned a number of striking circumstances of persons being crowded together in prisons where the discipline was frightful, where they were treated like beasts, in a way that *we* should not think of treating beasts, crowded together in the utmost filth, and yet no fever was produced. But you will find in authors an account of a disease which broke out at Oxford in the year 1577, in the reign of Queen Elizabeth, at what was called "the black assizes." Some prisoners were brought out of prison, where they had been in a state of great confinement, with very little ventilation, and they were exceedingly dirty. A strong stench arose, and was supposed to proceed from the prisoners. Some of the judges and justices, the sheriff, and most of the jury, were taken ill, and died in a day or two. Six hundred persons sickened the same night; in the next three days, three hundred more sickened; and before five weeks had elapsed, five hundred and ten had died. The symptoms are said to have been violent pain of the head and belly, with delirium. Now many contended that this was a disease produced by the confinement of these prisoners—that a contagion was generated from them; that although they did not suffer, a contagion passed from them, and produced this disease. But others of equal judgment are of a different opinion, and urge, on the other hand, that the disease was not contagious. It is said that the court was held in the yard of the castle, that was only a small distance from the river Isis, the banks of which were low; and it is actually recorded that a great damp arose—a *breath,* as it is called, or a *fog,* arose among the people; and some said that at the same time an intolerable stench was perceived, and some seemed smothered. The weather was intensely hot. The disease was not the plague, and the physicians would not name it. From

all these circumstances, some writers contend that the disease had nothing to do with the prisoners, but that it was an exhalation owing to the dampness of the earth—some peculiar kind of exhalation; that we have no proof of its contagiousness, and typhous contagion rarely affects so instantaneously. Women, children, and poor people, are said to have escaped. How this may be I do not know; but to shew the bigotry of the times, I may mention that it is said that others took up a very different opinion, and alleged that it was neither contagion nor exhalation, but that it was entirely owing to the poor Roman Catholics, who used art magic. They stated that it arose from diabolical and papistical arts—that it was produced by popish blasts, which emanated from the lowest depths of hell. You will find this particularly mentioned in Wood's history of Oxford. "Some have thought, and do think, that it was devised by the Roman Catholics, who used art magic in the design; and that also, as a certain note witnesseth, (register of Merton College,) it sprung *ex artificiosis, diabolicis, et plane papisticis, flatibus, e Lovanensi barathro excitatis, et ad nos scelestissime, et clam emissis.*" This absurd opinion seemed favoured by the circumstance of the damp or fog having arisen in the court as soon as sentence of loss of ears was passed upon a book-binder, who continually had spoken against Elizabeth's government, and the reformed religion. Another singular instance is recorded, which occurred at the Old Bailey in 1750, where several persons were taken ill after the prisoners had been brought into court subsequently to close confinement. This has been supposed to be exactly a similar instance, but it appears that there are objections to it, and many contend that it arose from a window being opened, and a draught of air coming on those who suffered. How this might be I do not pretend to say, because there are ten thousand circumstances that would alter one's opinion if we had an opportunity of making minute inquiry. I can only state my belief that typhus fever is sometimes contagious—that it may be made so under very unfavourable circumstances—that it is, perhaps, always in one sense contagious, only that the contagion is so mild and diluted that it comes to nothing, and that it belongs, like cholera, to those contagions which are not only powerless by moderate dilution, but powerless upon a frame free from disease, in full vigour, and with all or most of the causes of good health about it. We know from Thucydides that before the plague of Athens, from Livy that before the plague of Rome, and from Hodges that before our plague in 1665, great crowds were collected in each respective city; that in the latter case the sky was serene and the air stagnant, and that the rich escaped so much that the disease is said by Clarendon to have acquired the name of the *poor man's plague.* Whether the contagion can be generated afresh, I will not pretend to assert, but I believe it. If a person be near another labouring under typhus, and a considerable time afterwards is exposed to fatigue, and other predisposing causes of fever, and is seized with it, we cannot say whether the contagion has lain dormant all the time, or whether the mere force of the predisposing causes alone have produced the disease. Either side may be adopted, and neither disproved.

Emanations from Volcanoes, &c.—Now I may just mention, by the way, that besides malaria being a cause of remittent and intermittent fever—besides contagion being a cause of continued fever—it is very possible that there are certain other exhalations which do harm; which produce actual disease, I mean. Sydenham had an idea that epidemics arose from some peculiar changes in the bowels of the earth. It was only a fancy of his; but it is very possible that some of these peculiar causes of disease are exhalations let loose from particular spots. Berzelius mentions a curious circumstance: that he was making experiments with saleniuretted hydrogen, and after a certain period, not immediately, catarrh came on, and continued a very great length of time. A quantity which he inhaled while making the experiment did not produce any great irritation at first—he did not suffer any immediate inconvenience—but after a certain period had elapsed, then it began to operate, as all specific poisons do, and he had long-continued catarrh. Now some, from such facts as these, conceive that a volcano may let loose a peculiar substance capable of producing a peculiar operation on the human body. It is not altogether improbable that exhalations of various sorts may arise *out* of the earth, independently of the exhalations of diseased animal bodies, and independently of the exhalations of putrifying vegetable matter from the surface of the earth. The subject is not at all understood, but certainly it is an inquiry worthy of being attended to whenever an opportunity occurs.

I ought to mention that the bodies of persons dead of typhus, rarely, if indeed ever, give the disease; and that typhus, whether contagious or not, when epidemic, grows, like other epidemics, epidemically milder and milder the longer it lasts, though it be spreading more and more.

Having considered the remote, we next come to consider the

Proximate Cause.—Respecting the proximate cause of fever, there have been, I might say, thousands of hypotheses. In these hypotheses the writer has generally assumed some one fact which has had no existence, or taken up one particular circumstance among all the circumstances of the disease, and placed that as the cause of the whole. Some assume it as a fact that the disease is all in the solids; others assume it as a fact

that it is all in the fluids. Those who ascribe every thing in a disease to the fluids, are said to be *humoral pathologists.*

Their fancy is this: that a certain something is in the fluids which is deleterious to the body: that a process is going on analogous to fermentation, which they call *concoction*; that then the peccant matter is separated, and the process is called *despumation*, and provided it is thrown off entirely, there is an end of the disease. Now there is not the least proof of any such process occurring. There is proof of the depravation of the fluids, but there is no proof of concoction and despumation—of a fermenting and despumatory salutary reform of the fluids, if I may so speak. There is nothing peculiar thrown off when an excessive secretion takes place, as at the crisis when great diarrhœa or sweating occurs; or at least, if there be, we know it not, and it is a mere fancy to say that it does occur. So when there is hæmorrhage, there is no proof that that portion of blood which exudes is more vitiated than the rest; and it is to be remembered that it is only a part of the blood which escapes. All these changes of the fluids are most probably owing to the solids in the first instance. A morbid cause makes a peculiar impression upon the body as a living system—it impresses the solids, and by the operation of the solids the fluids are secreted in a vicious manner and of a depraved quality. It is probable that the vitiated state of the fluids arises from the solids not having manufactured them properly; unless, indeed, a quantity of improper materials are supplied to the body, which the body, without any fault of its own, can make nothing of. In that case, the solids certainly are not in fault; they do their best, but they are obliged to manufacture (if I may so speak) a bad article, because they have bad materials. With this exception, I imagine that the fluids must become depraved through the solids, though of course they again must exert an evil influence. Again, it is a fact that fever will continually cease without any discharge—without sweating or diarrhœa, or loss of blood at all. It is most probable, that when these things take place they are the result of the whole healthy change together. In fever, you may get a patient to sweat profusely, but perhaps he will not be any better for it; if, however, you get him better, then probably he sweats. This, however, is not the result of the sweating; you have brought him into an improved condition, and sweating then takes place, as it were, spontaneously. Frequently, in fever, diarrhœa occurs without any relief; you stop it, and the patient is all the better for the suppression. The critical discharge, too, when it does occur, and when a patient is improved *with* it, is for the most part too small to explain the improvement. It would seem that the discharge is rather the consequence of the improvement than otherwise; the improvement occurs, and the discharge takes place almost as a matter of course.

Then some have imagined that, in fever, the blood is too thick—that it will not flow; but that is a mere assumption. Again, other authors have asserted that the affection is owing to a spasm of all the small vessels. I presume that there is spasm in fever—that so long as the solids do not secrete, we must suppose that the vessels are closed, that the fluids cannot escape; but though there be spasm at a certain period, when the secretions are all deficient—though we allow that this is *one* striking circumstance, yet there is no reason to imagine that it causes *all* the symptoms. Then, again, some have assumed that debility is the cause of fever; but people are weak enough every day without having fever; and if debility be *one* of the circumstances of the disease, yet there is no proof that debility is the cause of the symptoms.

Then, again, some assume that it is inflammation. Some will say that it is an universal inflammatory state, while others contend that it is only a local inflammatory state; and some maintain that this local inflammatory state is situated in one part, and some in another. Now that the body, in fever, is frequently throughout in an inflammatory state, there can be no doubt at all; neither can there be a doubt that in fever there is continually local inflammation; but there certainly is something more than all this in fever. You have a peculiar feeling of debility; you have a peculiar look of countenance; you have pains generally, at the first, in the loins; a tremulous tongue, an universal disturbance, such as you will not have from any simple local inflammation in any one part; and many of these symptoms certainly are not referable to an inflammatory state; and if the disease be contagious, then this shews that there is something more than mere inflammation. The local inflammation which occurs is by no means proportionate to the violence of the fever. You will frequently have violent fever, and, although there is local inflammation present, yet the latter is not at all in proportion to the former. In some instances you will have death take place at the very first, without any inflammatory state being produced, just as in small-pox, which unquestionably is a contagious disease, you will sometimes have the patient die before any inflammation becomes visible; death taking place merely from the depressed state of the system. Mere inflammation, whether general or local, although it is a circumstance that frequently occurs in fever, is nevertheless quite insufficient to explain the whole of the symptoms. It is one fact in the disease, but not the whole of the facts; nor is it a fact from which any one can prove that the other set of symptoms arises.

Locality.—As to its locality, with respect to those who consider that it is neither situated in the fluids nor the solids generally, but in some one part, I may mention that Hoffman thought it was a disease of the nervous system. The nervous system certainly is affected, and so likewise are the secretory organs; and therefore others have just as much right to say that it is a disease of the secreting system as Hoffman had to ascribe it to the nervous system. Dr. Wilson Philip supposes it to be an affection of the capillaries throughout the body; but then there is a peculiar affection of the nervous system in general, and there is particularly a disturbance of the abdominal organs. Some have fixed upon inflammation of the brain. Former writers have done that, and a modern physician, in this town, has done the same; he considers it a mere inflammation of the brain. Others, again, residing in Paris at this moment, fix upon the abdomen. Broussais considers it to be inflammation of the stomach and bowels — what he calls *gastro-enteritis.* Some patronize one organ, some another.

Now I believe, as I just now mentioned, that the head is continually in a state of inflammation, and so is the abdomen; but occasionally the head is far more affected than the abdomen, and in other cases the abdomen is far more affected than the head. I stated that the local inflammation frequently bore no proportion to the general symptoms of fever, and sometimes we have violent local inflammation without any corresponding symptoms of fever; and the same is the case with regard to the relative affection of the head and the abdomen. Sometimes the head is more affected than the abdomen, and *vice versâ;* they do not bear any proportion to each other. If we assume local inflammation to be the cause, and if we observe the phenomena of many cases, we have no more right to settle the affection in the head than we have in the abdomen. Frequently you will find the disease running on and proving fatal, without any decided marks of inflammation at all—any thing decided, such as bears a proportion to the general sinking of the system. Besides, there are peculiar symptoms in fever, which mere inflammation would not at all explain, such as the particular look of the face, the tremulous tongue, and the extreme feeling of debility of the body. Again, it is to be remembered that the fluids, in fact, undergo a peculiar change. As the disease advances and the debility goes on, the saline matters of the blood are more or less deficient, and the blood becomes more and more watery. It is said by a friend of mine, Dr. Stevens, that we can remedy this state, not by the remedies for inflammation, but by supplying the deficient substances in the blood; of that, however, I shall presently speak. All these I consider to be mere circumstances constituting a general collection of truths; but I do not see any reason to suppose that there is disease of one part more than of another.

Some think that inflammation will explain every thing—not only changes of structure, upon the absurdity of which I before dwelt, but every thing else that occurs morbidly to the body. It certainly is the first circumstance that takes place in many changes, and it accompanies many changes; but there is no proof that this is always the case. Syphilis is an inflammatory disease; the first thing that occurs is a pustule, or a mere inflammatory speck, followed by ulceration; and then the next occurrence is an inflamed gland; and then, when other symptoms arise, they are more or less inflammatory. If you bleed during the secondary symptoms you often find the blood buffed, and you have inflammation in different parts of the body. Syphilis, however, is something more than inflammation; so it is with cancer, so it is with encephaloid disease and melanosis, and so I think it is with fever. Inflammation forms a part of a large number of cases; but it is not sufficient to explain them, or otherwise a mere case of phrenitis would be in every instance a case of fever; every case of inflammation of the stomach, of inflammation of the bowels, would, if there were a certain degree of excitement, be a mere case of fever, which undoubtedly is not the case. Another argument against fever being mere inflammation is, that it is frequently cut short by an emetic, or by cold affusion—not ablution, but affusion—in a very early stage. Now this you could not do if the disease were nothing more than local inflammation, or inflammation at all. Again, inflammation will not explain the difference between typhus fever and plague, which, though they are different diseases, are nevertheless in many respects analogous. Inflammation will not explain scarlet fever, nor will it explain measles, yet they are both inflammatory diseases. The symptoms are decidedly those of inflammation in a great number of cases, but there is something more than that; the system is in a peculiar state, and inflammation is merely one of the circumstances. Some have imagined that inflammation produces even intermittent fever, but we have no explanation of it, and this is a mere assumption. Any thing may be said in physic, but any thing may not be right.

Although I deem it necessary to make these remarks, yet I shall not attempt to state what I believe fever to be. I really do not know what it is; and I think it is always a great blessing to know one's ignorance. It is a great blessing not to fancy one's self acquainted with things which we are not; because if we fancy we are, we then sit down contented, and never think of further examining the subject. It is a great point in study, I think, to ascertain whether we really do understand a thing or not;

not to deceive ourselves by fancying that we are fully acquainted with it, when we are in the most perfect state of ignorance. You will find an excellent observation of this kind in Voltaire's Philosophical Dictionary, under the word "idea," where a person is lamenting that he has got so many ideas—that his cerebral matter is full of ideas, but, that he is perfectly miserable because he cannot tell what an idea is. He fancies it is this, that, and the other; he indulges first in one hypothesis and then in another, and says it is a shocking thing not to know what an idea is. His friend tells him that it is a much more shocking thing to sit down contented with imagining that an idea is what it is not. "*Il est bien triste d'avoir tant d'idées, et de ne savoir pas au juste la nature des idées.*" "*Je l'avoue,*" replies the other, "*mais il est bien plus triste, et beaucoup plus sot, de croire savoir ce qu'on ne sait pas.*" I imagine that we do not know the peculiar state of the system in typhus fever, any more than in measles or hooping-cough. A peculiar cause has operated upon the body, and a peculiar state has been thereby induced, the *effects* of which only we can observe.

Treatment.

However, notwithstanding this, the treatment of fever is in the highest degree rational, and in the highest degree successful.

Ventilation, Washing, &c.—In the cure of fever we must aim, in the first place, at having free ventilation, and the most perfect cleanliness; plenty of washing, plenty of clean linen, and plenty of fresh air. With fresh air and fresh water we may go on very well; but if there be any smell which ventilation and washing will not remove, the chlorides, I need not say, are excellent things, sprinkled upon the bed, or sprinkled upon the floor, disposed in saucers, or in rags dipped in the solutions and hung on the backs of chairs about the room. A solution of the chloride of lime ought to be put into the utensils which the patient employs, that no unpleasant smell may arise in the room—no contamination. When you first see a patient, if he be dirty, before prescribing any thing else, it is right to prescribe soap and water. Before any thing else is done, I would always have a patient got perfectly clean. When he is well cleansed with soap and water all over his body, and especially his lower half and extremities, a portion which is sometimes exceedingly filthy, you should have him well washed with plain water several times a-day. Soap is no longer required, but sponging several times a-day is always of importance. I believe it is always safe in fever to wash a patient with warm water; but if he say that he is hot, or if, when you place your hand upon him, you feel that he is hot, you may employ cold water.

Temperature.—If the temperature of the patient be steadily above 98°, as ascertained by a thermometer placed under the tongue, or placed in the axilla; if there be no pulmonic affection, no general profuse sweating, and the patient himself does not say that he is chilly, you may take him out of bed and throw a pail of cold water upon him, which, of course, is cold affusion; and this may be repeated. The patient must be dried and put to bed, and as soon as he grows hot again the same measures may be adopted. This plan makes him exceedingly comfortable; frequently it induces perspiration, and frequently it sends him to sleep; but if it fail of this, it nevertheless makes him very comfortable, and sometimes it cuts short the fever. Upon this subject you should read Dr. Currie's *Medical Reports.* But in general I do not find the heat steadily above 98°—I do not find patients free from a certain degree of chilliness, and I therefore content myself with tepid or cold ablution. I have never had occasion for affusion, but it is always safe under the restrictions that I have laid down. The cold bath is too chilling a thing; it would strike too suddenly. But when it would not be safe to take a patient out of bed and throw cold water upon him, you may always have recourse to ablution, either cold or tepid. I always make it a rule to consult the patient's feelings on this point; and if I think he cannot bear much cold, I have him stripped and sponged all over with tepid water; and between this treatment basins of cold water should be brought to the bed-side, and he should be allowed to put his hands into them as often as he thinks proper, and his face too should be continually washed. If there be any pulmonary affection, or if the patient say that he is chilly from the ablution, then you should use it tepid, because I need not say that tepid ablution is a great source of relief, and that it cools the patient considerably. It does not produce the impression that is made by cold water, but yet a great degree of evaporation takes place, and it extracts a certain portion of the patient's temperature. When it is applied, its temperature need not be 98°; from 80° to 90° is sufficient; and it cools him by its subsequent evaporation.

To co-operate with this the patient should have but few clothes upon him, and the windows and doors should be opened, so that he may have not only a free ventilation, but at the same time a cool temperature. The doors and windows should be wide open, unless the patient feels chilly, or the draught seems too strong for him. As the disease lasts longer, you must apply cold the less. You will find that there is not the same power of generating heat—that the patient is not so hot as before, and therefore the application of cold is less advisable, and you find it necessary to use tepid ablution where previously you used cold; and, indeed, as the disease advances, and the patient is get-

ting better, there is little occasion for much abstraction of temperature; you find ablution, whether warm or cold, less and less frequently wanted, and you find low temperature less and less required. But while in general ablution is employed for the purpose of cooling, it should still be partially employed for the purpose of cleansing. The hands, the feet, and the head, ought to be looked after. The same rule is to be observed as at the beginning with regard to the temperature of the room—that is to say, the patient's feelings ought to be consulted. If he be delirious, of course you will not attend to him, but judge for yourself; but if he be not delirious, and say that the temperature of the room is unpleasant to him, then you ought not so freely to diminish it.

Emetics, Purgatives, &c.—While you attend thus to the surface of the body, you likewise have to attend to the inner surface, to the alimentary canal. Now it is a good practice in the beginning to give an emetic; but if you find tenderness of the epigastrium—if you find tenderness on making pressure on any part of the abdomen, I would not have recourse to any such measure. If, indeed, there should be a violent determination of blood to the head, I do not know that I should have recourse to it then. Frequently, however, in the beginning of fever, it is an excellent practice to give an emetic; for instance, a grain of tartar emetic, with a scruple of ipecacuanha; but I should never think of giving it without first ascertaining whether the abdomen was tender on pressure or not. Sydenham gives very good advice on this point; he advises us to premise bleeding before we exhibit the emetic. This practice is not always necessary; but if the pulse were full, and there were great signs of a determination of blood to the head, I would bleed first. But whether we give emetics or not, there is no doubt of the propriety, in every case of fever, of seeing that the bowels are regularly opened—that no filth collects in them any more than on the surface; they should be freely opened for the most part every day, at least at the beginning of fever. If they be confined, then one of the best things is a large dose of calomel; of course it must vary from to two or five to ten, fifteen, or twenty grains, according to circumstances; but, for the most part, five grains of calomel, at the utmost, followed by castor oil every two hours, will answer every purpose. Now and then you will have obstinate constipation—a patient may not have had a stool for many days, and then you may give ten grains or a scruple of calomel, and follow it up by castor oil; but it would be wrong to give a dose of that description if there were every probability that a few grains would answer the purpose. Nothing is better than to follow it up with castor oil; but at the same time, I need not say that you are likely to accelerate its operation if you give a common injection. This, however, is not to be done if the bowels be sufficiently open themselves—if they be open once a day. But sometimes it will happen that they are too open—that there is the opposite state, that of excitement, and then, of course, purgatives would be highly improper. This plan, of course, is to be put in practice at the onset; but, in regard to ablution, I have stated that I would use it in the progress of the disease. As to purging, although you clear the bowels out well in the first instance by means of calomel, and it certainly does clear them out better than any thing else, although it certainly does usually require another purgative to set it off, you nevertheless find it often a good practice to go on with smaller doses of calomel, or other preparations of mercury, during the course of the disease. As, however, this is another branch of the subject, I had better defer its consideration till the next lecture.

THE POSSIBILITY OF VOLUNTARY LOCOMOTION,

TO A CONSIDERABLE DISTANCE,

AFTER THE COMPLETE DIVISION OF THE CAROTID ARTERY AND OTHER LARGE VESSELS OF THE THROAT.

From a Lecture on Medical Jurisprudence,

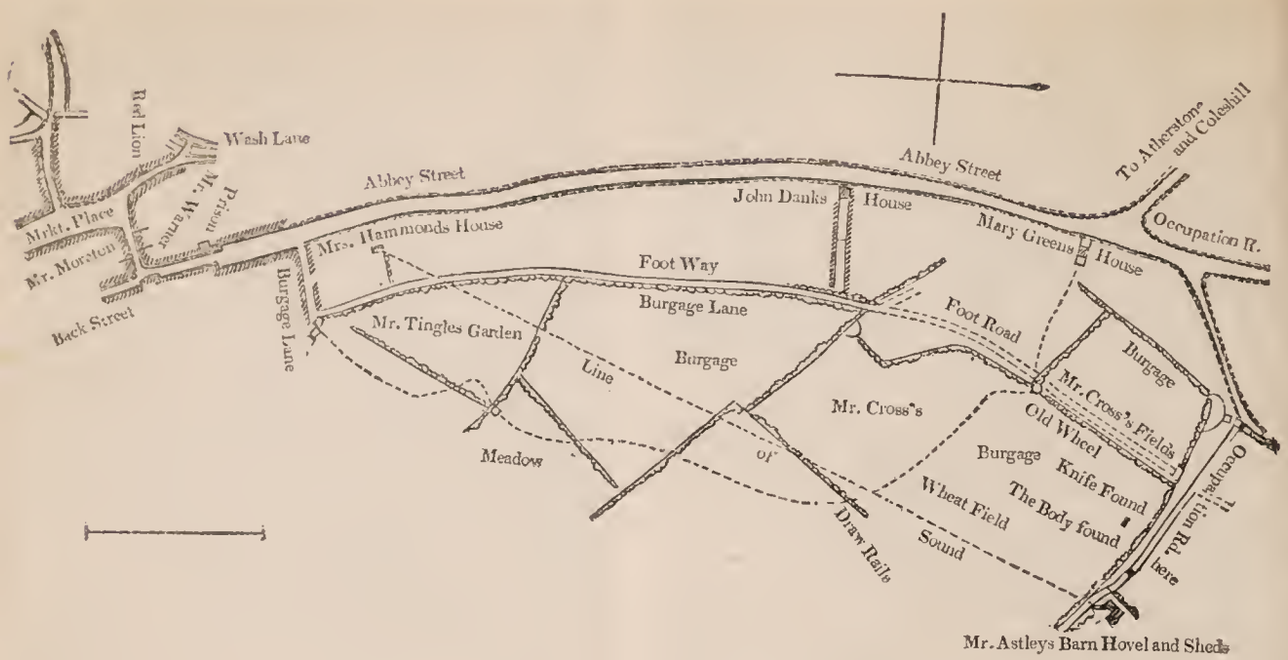
By Professor Amos,

AT THE LONDON UNIVERSITY.

(With a Woodcut.)

On resuming his lectures after the spring circuit, Mr. Amos stated to his class the following particulars concerning a late trial for murder, in which he had been professionally engaged.

At the Warwick assizes, a man of the name of John Danks was tried for the murder of Mary Green. The prisoner was a farmer's labourer of the better sort; he was married to a second wife, by whom he had no children. The deceased was a woman of loose character, having had six illegitimate children, and being far advanced in pregnancy with a seventh, which she had some time since sworn to the prisoner. He still, however, continued to occasionally cohabit with her. It appears that, on the evening on which the murder was committed (Saturday the 18th February last), the prisoner left his home. Soon after he made his appearance at Mary Green's, and, opening the door of her house, invited her out to talk with him. They were seen by the deceased's daughter conversing together for a few minutes, after which Green returned to her dwelling, but shortly afterwards went out again to meet the prisoner. The parties were then seen proceeding together to their



usual haunt across the fields, to the place in the plan marked Astley's Hovel.

The next material circumstance in the history of the murder is, that shortly after this a cry of distress was heard, which reached the house of a woman of the name of Hammond, and came from the direction of Astley's Hovel. Mrs. Hammond called up her neighbour, John Harris, and besought him to find out what was the matter. He put on his hat, and as soon as he got out of his door he heard the cry repeated; he described it as a very dreadful cry; but after pursuing the direction in which it came for a little distance, he stopped to listen, and hearing no more of it, returned quietly to his house. Between eight and nine o'clock the next morning (Sunday) the body of the deceased was brought home, having been found in the road, about twenty-three yards from Astley's Hovel.

Immediately upon the alarm being given, the constable of Nun-Eaton reached the spot where the body was lying in the road; he examined the hovel, and found that somebody had been cut there; he traced the blood from the hovel to a gate some yards from it on the road side, and over that gate to the spot where the deceased lay. The prisoner was speedily apprehended, and on his clothes were found some marks of blood; a button, with blood on it, had been picked up at the gate already mentioned, and this button was found to correspond with those of the prisoner's waistcoat. The knife with which the murder was committed was found in the adjoining wheat-field, and identified as belonging to the prisoner. At first the man attempted to exculpate himself by a weak *alibi*, and endeavoured to account for the blood on his clothes by saying that he had assisted in the killing of a pig—a story which was subsequently proved to be a falsehood. After the coroner's inquest, however, upon his being committed to jail, he made a declaration or confession to the constable to this effect:—that he and Green had walked together to the hovel, and were there for about a quarter of an hour, "when," said he, "I up with my fist and struck her on the temples, and knocked her down. I fell at the back of her, and held her down, and she hooted very loud; I cut her once while I lay at the back of her, but it did not stop her hooting; and then I cut her again, and that stopped her hooting, and I was sure she was done for. I got up, left the hovel, and got over the gate, and came up the road to the top of the Abbey-end; and as I was coming along the road, about one hundred yards from the hovel, I thought I heard the footsteps of a man coming after, but upon turning round, I could see nobody. I then shut my knife, and threw it into the wheat-field, and made my way home, where I washed my hands, and went to bed." He added, that he fell at the woman's back in order to escape the flow of blood.

We shall now take notice of the condition in which the body was found. The constable deposed, that when he came to the spot he perceived the dead woman lying on her face, with one arm under her, and her head towards the Abbey-street; it was impossible for any one as the body then lay, and without moving it, to see that the throat was cut; her cap was not on her head, but off behind, and part of it stuck to the blood on her face. The body was taken home on a door, and kept there in the same position in which it was found, until it was seen by the surgeon. The constable's evidence went further, to the identification of the prisoner's footmarks made by him in running across a wheat-field adjoining the place where the body was found; he took the prisoner's boots and compared them with the footmarks, and found that they fitted exactly; and the identification was made still more satisfactory by comparing those marks with an impression taken

in cement. In the hovel the constable deposed that he found a large quantity of blood upon the straw, and thence to the gate, he said, were also marks of blood, upon the woodwork of the side of the hovel; also on the bars of the gate was a quantity of blood, and on the topmost bar were marks as though it had been pressed by a heavy hand.

The evidence of Mr. Richard Lloyd, the surgeon who was called in to see the body, was to the effect that he found, at the upper part of the throat, near the angle of the left jaw, a gaping incised wound, carried to the pharynx and commencement of the œsophagus, to the posterior part of, and obliquely down, the right side of the neck, to the fourth cervical vertebra; such a wound, he deposed, including a complete division of the trunk of the carotid artery, and all the principal branches of the external carotid and the jugulars, must of necessity be mortal, and occasion death *immediately*, or, at least, *in a very short time*—so short, as to render it highly improbable, but not impossible, that the woman could have gone the distance of twenty-three yards, besides getting over the gate, in that dreadful condition. The wound measured seven inches in length, and three in depth. There was also another incised wound two inches below the former, in front of the throat, but not deep enough to divide the trachea; with *that* wound alone, he said she might have been able to have reached even her home, which, indeed, he thought the cause of her leaving the hovel previous to her receiving the last mortal wound. Mr. Lloyd had assisted the constable in comparing the footsteps in the wheat-field with the plaster impressions. It should be mentioned, that this gentleman's evidence as to the nature of the wounds, and the almost utter impossibility of the larger one being inflicted in the hovel, was given before the prisoner's voluntary confession.

Such, then, were the principal facts connected with this murder. Danks was, of course, found guilty and executed. Of his guilt, no possible or reasonable doubt can be entertained; but still the circumstances of the case are of a very singular nature in a medico-legal point of view. Did the murderer really inflict the mortal wound in the hovel, or on the spot where the body was found? And if the former, is it not a very extraordinary fact that after such a wound, in which the main trunk of the carotid, its external branches, and the jugulars, were severed, the power of locomotion to so large an extent should still remain? Mr. Lloyd's opinion is obviously such as the majority of intelligent medical witnesses would have given, and such only as they would most probably feel themselves justified in giving;—from all that experience and history have hitherto taught us, no other conclusion apparently could be arrived at. All the facts, however, and circumstances of the case being put together, and duly weighed—and we are deeply indebted to Mr. James Williams Buchanan, the solicitor, of Nun-Eaton, for the great industry and ability with which he has collected the evidence—we can come to no other conclusion than that the murderer's declaration was perfectly true; that *both* the wounds were inflicted in the hovel, and after all, that the miserable woman was able to get on her feet, to proceed to the gate, and over the gate, to the place where she fell at last from utter exhaustion.

The first wound, as Mr. Buchanan observes, was quite inconsiderable. It was situated within an inch of the clavicle, and very superficial; the integuments were merely cut through, and the cartilage of the trachea was scarcely grazed. From such a wound, the quantity of blood in the hovel could never, by any possibility, have flowed. On the other hand, it would seem that there had been from the second wound a most copious flow of blood, which was thrown off by the prominence of her belly (she being far advanced in pregnancy) to the side of the hovel, as she passed along to the gate; then on the gate a similar transfer of blood was manifest, and there also were the marks of both her hands. The prisoner's hand was also marked on the gate, but at a different place. Mr. Buchanan further informs us, that he went himself over the ground, the Sunday after the trial, and it took him fifteen seconds to pass from the spot where the wound was inflicted, over the gate, to the place where the body was found; and in a subsequent experiment, in which he proceeded at a slower pace, it occupied him twenty seconds. The obstruction of the gate, he thinks, might be equivalent to eight or ten yards more*. It is a curious fact, as observed by this gentleman, that there was scarcely any blood between the gate and where the body fell; but this he very reasonably accounts for, by her having guarded the wound with her cap, and closed it still further by holding down her head. There was, however, a large quantity of blood found to have flowed down between the breasts and lodged about the pubes; to such an extent, indeed, was it there accumulated, that the women who stripped the body thought at first that there must have been an attempt made to take away the child—but this was shewn to be not the case upon medical examination. There was also, it appears, a great absorption of blood by the under-garments.

But in estimating the time that elapsed from the infliction of the wound till the un-

* It was an ordinary five-bar gate, three feet ten inches in height.

fortunate woman dropped, (that is, supposing the wound to have been inflicted in the hovel, and Danks's confession to be true,) other considerations must also enter into the account. The fifteen or twenty seconds calculated by Mr. Buchanan would seem to be considerably short of the actual time. We should begin to reckon from the moment in which the murderer "stopped her hooting," the trachea having most probably then been divided, together with the great blood-vessels, after which the murderer arose from his prostrate position, and having left her for dead, went his way. She in the meantime must be supposed to have lain tranquil, at least while Danks was in the hovel, and to have rallied after a temporary collapse,—after which she had to regain her feet, and to set herself in motion in the direction along which she was traced. All this could not have been done in an instant; on the contrary several seconds must have passed,—how many it is quite impossible, with any degree of exactness, to conjecture, but probably not less than ten more, at the very least, must be allowed. Then it is not the mere time of crossing the gate that should be calculated, but the expenditure of strength and the loss of blood that must have occurred in accomplishing this object.

And many more difficulties doubtless will not fail to present themselves to the mind of whoever takes the circumstances of this curious case into consideration; though probably, after all, the chief difficulty, as in the famous miracle of St. Denys, (without levity might it be alluded to,) shall be thought to have existed in those movements which had necessarily to be made in the first instance,—and, as was observed in the case of the French miracle, perhaps we too shall be inclined to conclude that, "*il n'y a que le premier pas qui coute.*" Once allow that the unfortunate woman was able, after her "hooting was stopped," and she had recovered her legs, to recover also her faculties, and to put herself in motion, and much of her remaining difficulties must vanish. At all events several seconds should clearly be allowed for this period, in estimating the whole time elapsed from the division of the great vessels until she sunk from exhaustion.

It may just be added, that, to the hour of his execution, the prisoner declared that he never touched the woman except in the hovel, where he left her for dead. He persisted, however, in affirming that he certainly heard a footstep when on the road, that of a third person, as he conceived, coming behind him; and in that conviction he died.

CONTRIBUTIONS TO PATHOLOGY.

By John Alexander, M.D.

One of the Medical Officers to the General Dispensary for Children, Manchester.

No. I.

Case of Inflammation of the Spinal Cord.

December 16, 1831, Friday.—I have this day been desired to visit Mrs. Hanlon, a publican's wife, æt. 26, of stout frame, though low stature, residing in George Leigh Street, who states the following particulars:—

Subsequent to her third confinement, which took place fourteen months ago, she has enjoyed a good state of health until Sunday last, when she was attacked with considerable uneasiness in the loins, which gradually on Monday and Tuesday became more and more severe. On Wednesday her usual medical attendant was sent for, and ordered four-and-twenty leeches to be applied to the part complained of, anodyne fomentations to be frequently used, and various purgatives to be administered.

This morning (Friday) her symptoms are, intolerable suffering referred to the site of the third, fourth, and fifth lumbar vertebræ, extending laterally about a hand's breadth. On particularly examining the part complained of, there is no decided increase of pain on pressure, unusual heat, tumefaction, any discolouration, or, in a word, deviation from a healthy appearance of the loins; the mind is anxious, and countenance flushed from suffering; pulse 125, rather weak, but bearing the digital pressure tolerably; tongue whitish and dry; bowels obstinately confined, not having been relieved, although calomel and colocynth pills, infusion of senna, and castor oil, have been, during yesterday and this morning, successively administered; urine scanty, but not high coloured or made with difficulty.

We have prescribed a repetition of the leeches, followed by the application of a large blister; and two drops of the croton oil, in form of pill, with warm gruel, every two hours, until the bowels shall be acted upon.

10, p.m.—This evening we find our patient much easier. The leeches bled freely, and the blister is now rising. The pills have been followed by three dark, offensive, copious motions; more urine made. We have given her an anodyne, containing sixty minims of laudanum, in order to induce sleep, which she longs for.

17th.—The night has been tranquil; but to-day Mrs. Hanlon's case presents a serious feature, that of complete paraplegia. The lumbar pain has entirely subsided, but her command over the lower part of the body is wholly gone, being unable to move either leg, to command the retention of her fæces,

or to evacuate the contents of the bladder. She appears not worse of herself, the pulse being 100; tongue moist; and some appetite mentioned. We have directed the blister to be dressed with savine ointment, and the urine to be drawn off in the evening.

18th. — Much as yesterday, but feels weaker. A large caustic issue has been placed this morning on each side of the lumbar vertebræ; opiate repeated.

20th.—Not the slightest return of command over the limbs, which, though so powerless, retain a temperature equal to that of any other part of the body. The alvine dejections are frequent and offensive; catheter still required; pulse 125, weak. We have ordered her decoction of bark, with nitric acid, three times a day, and her draught to be doubled in strength, in order to secure sleep, of which she enjoyed very little last night.

24th.—The slough on the site of the issues is inconsiderable, and the separation, aided by poultices, is nearly effected; but there does not appear the slightest amelioration in the symptoms. Indeed, the woman's strength is rapidly giving way. Her rest during the two last nights has been much disturbed by startings, dreams, &c. She has been requested to drink port-wine freely; and we have given her an addition to her opiate of twenty-five minims.

25th.—Slough separated, pulse 140; says she feels very much weaker; has slept pretty well during the last night, although still observed to start and twitch during her artificial slumber. Her appetite is entirely gone, but considerable thirst is present.

26th.—Yesterday evening, a slight but troublesome cough was observed to annoy her, and has continued since to do so; it is unattended with any expectoration: says she is perfectly free from pain; has taken a bottle of port-wine during the last twenty-four hours, and complains chiefly of a sense of sinking; pulse 148, extremely feeble; speech almost inarticulate; abdomen tympanitic; the fæces and urine are now both passed involuntarily.

27th.— Mrs. Hanlon was taken in the evening with difficulty of breathing, and was soon relieved from further suffering.

The above notes are from my case-book. Three days after decease, with great difficulty, leave to institute an inspection was obtained; for the following particulars of which, owing to unavoidable absence, I am indebted to Dr. Stephens, a talented and indefatigable cultivator of pathology and lecturer upon anatomy in Manchester :—

Post-mortem Examination. — An incision was made from the middle of the back part of the neck to the os coccygis, and then the muscles were dissected back from each side of the spinous and transverse processes and arches of the vertebræ. In doing this, about the ninth or tenth dorsal vertebra, or perhaps a little higher, but precisely between the two scapulæ, we opened into a small abscess on each side, and lying close up to the vertebral column, and accompanied by slight caries of the walling vertebræ. The pus contained in these abscesses was what is termed laudable, and amounting to near four ounces in quantity. The vertebral canal was then laid open to its whole extent. The theca vertebralis, for about four inches between the shoulder-blades, and corresponding to the caries and abscesses, was rough, covered with lymph and pus, and greatly inflamed.

On slitting up the theca, we found it of a beautiful pink-colour on its inner face, corresponding to the diseased external part. Its remaining portion, both internal and external, appeared quite healthy. There was not any purulent matter within the theca, but at the lower part, opposite the bulb of the spinal cord, we found about two ounces and a half of a clear limpid fluid, distending forcibly the theca.

The cord having been completely exposed, we now proceeded to its examination, and found the vessels much more turgid than is natural to them. This vascular infiltration was particularly remarked opposite to the ninth and tenth dorsal vertebræ, corresponding to the inflamed portion of the theca before alluded to; and here there was an effusion of coagulable lymph (a thin layer about the diameter of a shilling) on the medulla under the arachnoid. There was observed also a patch, of a smaller size, on the bulb of the cord.

On very particular inquiry from the family, it was elicited that Mrs. Hanlon had suffered occasionally, for months past, from pain in the back, between the shoulders, and was in the habit of requesting a friend to forcibly rub the part complained of, which never failed to relieve her. But during this last illness she referred all her pain to the lumbar region. Dr. Alexander repeatedly examined the spinal column and adjoining parts, and could not detect any pain, or fluctuation, on pressure, in any part of the back; and I can testify that the dead body presented no outward signs of abscess or disease corresponding to that particular part between the shoulders which, though not the only, I feel disposed to consider the primary, seat of the disease under which she died.

Remarks. — Spinal affections, although much has been written, and undoubtedly well written, upon them, still constitute a *terra incognita* in the great

chart of medical science; nor is this fact, although lamentable, much to be wondered at, or difficult to be explained. The nervous system generally, although performing the most vital part in the organization of man, exercises its functions in a manner not cognizable to the senses, and the important operations of that system are therefore only darkly viewed through the effects which we presume to be consequent upon them.

The case above related is submitted to perusal, not from its possessing any peculiar novelty of feature, but from its embracing considerations, an allusion to which can scarcely prove uninteresting.

The first point to be mentioned is the *concomitant existence of the two abscesses, and spinal disease in the dorsal region.* From the circumstance of uneasiness having long existed in that part —from the presence of well-concocted pus in the abscesses—from the reported appearance of the spinal membranes, and the caries of the vertebræ—it is clear that this joint affection (although overlooked by myself during life) was of no recent origin. Taking this for granted, the interesting question arises to the mind, were the abscesses productive of the spinal disease, or was the latter the cause of the abscesses? That the spinal affection was the primary complaint, although unable to prove, I am inclined to believe from the following circumstances. The dorsal muscles do not present an usual site for common inflammation and its consequence; and had they been such in this case, great length of time would not have elapsed without the latter producing more striking evidence of its existence. Again, the presence of an abscess *on each side* of the spinal column can more readily be accounted for on the explanation of their cause originating in one common centre, than on the more improbable supposition that two symmetrical abscesses should spontaneously arise. And, lastly, on this point we find in a lower, the lumbar region, (the seat of the poor woman's recent suffering) a similar morbid state of the medulla spinalis had arisen.

As in most such instances of disease, obstinate costiveness was an early, and paralysis of the natural functions, a later symptom. There is, perhaps, scarcely a tyro in the practice of medicine to be found who has not observed the remarkable difference of effects induced by slight and considerable pressure on the nervous mass, whether that pressure be operating within the skull, on the spinal column, or within the delicate theca of an individual nerve. Yet the most experienced in the Hippocratic art remain stationary on the same step of inquiry's threshold; and fail, however desirous or indefatigable, of discovering the rationale of this interesting fact in pathological science.

Had the above case assumed a more chronic character, and the inflammatory symptoms abated—in other words, been a less severe one—the strychnine should have been employed according to the plan recommended by my friend Dr. James Bardsley, (see "Hospital Facts and Observations") to whom, I conceive, the profession and mankind are indebted for his practical monograph on the employment of that singular medicine. But to return:—

In the above report of postmortem appearances, it is stated that a thin *layer of coagulated* lymph was observed under the *arachnoid* membrane of the spinal marrow. Now as anatomical ingenuity has failed to detect any vessels, however minute, in that delicate tissue, I am induced to consider that lymph to have been chiefly produced by inflammation of the pia mater and spinal medulla, the sources also of the *limpid fluid* within the theca. This point, however, (as possessing little *practical* import) would not have been alluded to, but the respected, though peculiar opinions of Ollivier and Ribes, on inflammation of the spinal membranes, particularly of the arachnoid, bestows upon it a reflected interest.

In conclusion, it is an opinion of Sanson's, as quoted by Ollivier, that paralysis of the limbs, without being attended by contraction of them, depends upon inflammation of the *nervous structure alone;* whilst the *tonic contraction* of the extremities, frequently seen, is to be attributed to the complication of *inflammation of the membranes* along with that of the spinal medulla. Now in Mrs. Hanlon's case, the meningeal inflammation (as Dr. Stephens' inspection proves) was considerable, and that of the medulla less so; yet that tonic rigidity never once presented itself, either in the earlier or later stages of the paraplegic affection.

SALINE TREATMENT IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

As an impartial umpire, and as one of the conservators of the interests and respectability of the medical profession, I confidently anticipate that you will afford me a brief space in your journal whilst I again call the attention of the profession to the use of certain alkaline salts, now so satisfactorily developed in the Cold-Bath Fields Prison, though in doing so I should descend to notice my pretensions to priority and originality in this practice. From the statements, originating from medical men holding important official situations, published lately in the Medical Gazette, and other medical periodicals, exemplifying the curative powers of certain saline preparations in the treatment of typhus fever and cholera asphyxia, and from the total silence observed by them relative to the cases that I have from time to time published, illustrative of this practice, I am led to apprehend that I may have laboured under a mistake when I considered my mode of treating typhus fevers, and the cholera of these climates, to be not only original, but to have been calculated to form no less than an epoch in the annals of medicine. You yourself, sir, appear to be fixing a watchful eye on this practice: I rejoice in this, though I had not the honour to rouse the attention. Have I claimed that which belongs to another? I am not aware that I have, and I feel convinced that I have not; but if I have, permit me without delay to disclaim any such intention; a disclaimer which the papers you have already published for me relative to this practice will fully bear out, incontestibly shewing that I have acted with good faith. I am led into this train of thought chiefly by the letter of Mr. Wakefield relative to the "saline practice recommended by Dr. Stevens," and adopted in the treatment of the patients in the Cold-Bath Fields Prison. I have hitherto imagined that my letter in "The Times" of the 21st June, 1831, was the first publication that, *founded on practice*, advised, and, I trust, on scientific principles too, and not as a vague speculation, the *saline treatment* in the cholera asphyxia; and that the case I published in another journal, in 1828 *, was the first case on record wherein the carbonate of soda has been stated to be given, as a "*principle of practice*," in a fever frequently of the most dangerous typhoid species; and that the saline treatment with the carbonates and subcarbonates of soda and potassa, occasionally joined with preparations of the nitras potassæ, for the cure of typhus fever, adopted in 1825, the particulars of part of which you have done me the honour to publish in your journal, was the earliest time that these medicines had been so applied. With this impression, and being ignorant that either Dr. Stevens or any other man has any prior claim, I beg permission to reiterate my pretensions, though they are only denied by implication, a mode frequently as insidious as it is impertinent; and to insist upon my right, whatever that may be. *I have given dates, places, and persons' names*; I have never used any kind of secresy; I have never communicated my views to a few privileged individuals only; openly I have practised, and as openly I have stated the results. This, sir, is the basis on which I rest my claim to the merit of instituting a new, and, as I think, a greatly improved practice in the treatment of typhus fever, and fevers with the typhoid type, as well as in cholera asphyxia. It is very easy to shew if I am in error, and I challenge any one to do so; but at the same time I disclaim every intention, every wish, to appropriate to myself that which, in my conscience, I do not believe to belong to me.—I am, sir,

Your most obedient servant,

PAUL SLADE KNIGHT, M.D.

15, Connaught Terrace,
May 5, 1832.

* I have before stated this to have been published in 1827; but, on referring to the journal, I see it was not published till the 26th January, 1828, p. 624.

FRACTURES OF FORE-ARM.

To the Editor of the London Medical Gazette.

SIR,

As the observations I made on fractures of the fore-arm, in your journal of March 17th, have been followed by some remarks from Mr. Rodick, in your last number, (April 14th) I should like, if

not trespassing too much on your valuable pages, to make a few additional observations in reply.

My attention was first directed to the subject from seeing cases of fracture of the fore-arm where, when the bones had united, there remained an inability to bring the hand into perfect supination. This defect appeared to me to arise from the position into which the fore-arm is generally placed in the treatment of such fractures, and I accordingly tried that position recommended in the number of your journal referred to, and certainly found, that in the cases in which I tried it this evil was prevented, and that no distortion of the bones remained, such as presented itself in many of those cases where the ordinary treatment had been employed.

Mr. Rodick asks, "supposing a fracture to occur below the insertion of the pronator radii teres, is it not reasonable to suppose that the upper part of the bone, in consequence of the action of that muscle, would be more inclined to a state of pronation than supination?"

I should certainly say, if this muscle stood by itself, without any antagonists, it might then act in this way; but surely the biceps flexor cubiti is capable of exercising a power far superior to that of the pronator radii teres, and so bring the upper portion of the bone supine, even if there were not the supinator radii brevis to assist in this action, which muscle Mr. R. seems to think I had solely depended upon for this said position of the bone: it is the biceps muscle that I think brings the upper portion of the bone into the supine position. Besides, the very action of this muscle, when it bends the elbow in order to apply any mode of treatment, will be to bring the portion of bone into a state of supination, upon which the weight of the hand no longer acts, for those muscles which tend to counteract the action of the biceps muscle, as far as supination is concerned, when there is no fracture (viz. those arising from the inner condyle, and inserted into the hand), lose their influence on the upper portion of it, where the bone is broken; and, consequently, the only resistance the biceps flexor cubiti has, is the pronator radii teres.

Mr. Rodick says he has not seen a single instance in which the patients have not had the complete power of pronation and supination: he has been very fortunate. But if he will allow that there is a probability of such an evil as I have described taking place, where the patients are not so fortunate as to come under his care, and that there is any likelihood of the evil being prevented by adopting the mode of treatment I have recommended, I think that they will but be doing right who adopt it.

If you can insert the above, I shall feel greatly obliged.

I remain, sir,
Your obedient servant,
EDWARD LONSDALE,
House-Surgeon,

Middlesex Hospital,
April 19, 1832.

CANCEROUS TUMOR OF LIP.

To the Editor of the London Medical Gazette.

SIR,

MAY I beg the favour of your insertion of the following case in your valuable journal, if you think it worthy of a place?

I am, sir,
Your obedient servant,
A. T. S. DODD,
Surgeon to the Chichester Infirmary, and late Demonstrator of Anatomy at Guy's Hospital.

Richard Boxall, æt. 38, a labourer, and in good general health, was admitted into our Infirmary last September, with a cancerous tumor of the lip, about the size of a walnut, which was removed by the knife, and he was discharged apparently cured. He applied for admission again in November following, the disease having returned in the angle of the mouth, and was then about the size of a large marble, having two ulcerated spots about the size of peas. The lip and the surrounding integuments were perfectly healthy. On account of the loss of substance by the former operation, and the size of the present tumor, it was now necessary to take away so much of the lip that a frightful deformity must have been the result of an operation, while a very great additional discomfort must have arisen to the patient from the inability to retain the saliva. The return of dis-

ease after the former operation, had also shewn that it was necessary to remove freely the parts in the immediate neighbourhood of the tumor. I therefore determined to supply the deficiency occasioned by removal of the disease, by dissecting a flap from the neck, and thus forming a partial new lip. I considered that, by this proceeding, I should incur less risk of the disease returning after the new lip had become attached, than if I had left the Taliacotian operation to a future time; and by it thus have produced an irritation which might alone have been sufficient to rekindle the smothered flame of disease. I therefore performed the operation upon this plan, and secured the part in its new situation by T ligatures.*

On the second day there was sensibility in the new lip, and every thing went on perfectly well, with the exception of troublesome hæmorrhage from one of the coronary arteries, which was checked by pressure. The lip has now been for some time entirely healed, and the new portion natural in appearance, and is, of course, a great comfort to the poor fellow in speaking and retaining his saliva.

The only observations that I would offer on this case, are, that although the Taliacotian operation has been performed on the lip, for the purpose of filling up the chasm made by the ravages of disease, I am not aware that surgeons have hitherto ventured to replace a cancerous lip by a sound one at the time of removal of the cancer.

The only peculiarity in the mode of operating, worth noticing, was, that in raising the flap from the neck, I made the neck of the flap transverse in relation to the chin of the patient. This, I conceive, has the advantage of preventing so great a twisting, and so complete a strangulation of the vessels entering the part, as must be occasioned by the attachment of the flap being perpendicular, which is the usual form of it in operating for a new nose, &c. This, I think, is likely to be a frequent cause of failure in that operation.

Chichester, April 11, 1832.

* We have not thought it necessary to insert the figure, as it did not appear to us to render the subject more intelligible. The drawings have been sent to the publishers.—E. G.

SCIRRHUS OF THE BRAIN.

To the Editor of the London Medical Gazette.

Sir,

If you should consider the following case worth publishing, you will oblige me by inserting it, as I believe there are not many cases of scirrhus of the brain on record.

Your obedient servant,
Samuel Jolly.

9, Jeffery-Square,
May 4, 1832.

George N——, aged 4 years and 3 months. This child, after having been inactive and sleepy for several months, was seized on the 9th of August, 1831, with an epileptic fit, which lasted some hours, but was ultimately relieved by venesection, leeches, and purgatives, with cold applications to the head. His stools were at this time without bile; urine high coloured; and notwithstanding the daily use of calomel, combined with scammony and mercurial ointment, until the 24th, (amounting to sixty grains of calomel and one ounce of the ung. hydr.) no change was effected in the secretions; and after that aloes were resorted to, when the stools immediately assumed a natural appearance, and the urine lost its high colour, and the child was apparently well. Medicine soon after discontinued; but, however, before long he again gradually became sleepy, forgetful, and he would frequently lay his head down, as if it were too heavy to support. For the last two months previous to his death, his intellect seemed impaired, though not to any great extent. He occasionally forgot words when he was speaking, and paid very little attention to passing events, though he always understood what was said to him.

On the 19th of February, 1832, medical aid was again sought. Bleeding, purgatives, and blistering, were directed; but the parents, feeling convinced of the inutility of these measures, declined adopting them. He died on the 24th, having laid two days in a state of coma.

Postmortem Appearances.—On cutting through the dura mater, the brain bulged out through the incision, being evidently distended. The convolutions were slightly unfolded, and the sulci between them partially obliterated. Neither the arachnoid, pia mater, or sub-

stance of the brain, were more vascular than usual. The right lateral ventricle was found distended by fluid; the septum lucidum bulged in from the pressure of the fluid in the opposite ventricle; the arachnoid lining the ventricles was much thickened; the quantity of fluid contained in both ventricles was about six ounces.

On proceeding with the section of the left hemisphere, a tumor was discovered in the posterior lobe; it was about the size of a hen's egg, but not perfectly homogeneous in its texture; the most external portion looked as if it consisted merely of a deposition of fibrin, situated in which there were many vascular points, and also small irregular patches of a thick yellow fluid, varying in size, from a pin's head to a pea; this deposition formed a sort of coating round the central tumor, which was as large as a pigeon's egg; though its edges were not distinctly defined, it was nearly as firm as cartilage, of a pearly grey white colour, slightly granular on section, with claws, as it were, shooting out from the centre; it was separated from the posterior cornua by a very thin layer of medullary matter, and the thickened arachnoid which lined the ventricle. The rest of the brain perfectly healthy.

EXPLANATION OF DR. BADHAM'S CASE OF DISEASE OF THE HEART.

To the Editor of the London Medical Gazette.

Sir,

In your number for April 21st, Dr. Badham describes a case in which "the first sound, with its concomitant impulse and pulse, having been, as usual, followed by the second, this second sound was instantly reduplicated, occasionally even to the fourth time, before the first sound was again heard." *The pulse was only forty-five per minute.* From this case he infers that the second sound is auricular—not ventricular; as he does not see by what action of the ventricles it could be produced. The explanation, according to the principles of Dr. Hope, I conceive to be as follows:—After the ventricular contraction producing sound, impulse, and pulse, and followed by its corresponding second sound, there succeeds a second ventricular contraction, so feeble as not to produce either impulse, pulse, or sound, but sufficient to expel a small quantity of blood from the ventricles, and thus give rise to a diastole. As the first sound is *naturally* more suppressed than the second, when both are diminished in an equal degree, the first may become extinct while the second remains audible; and such, I have little doubt, was the case in the instance to which Dr. Badham refers.

Such cases are by no means rare, as he imagines; they are referred to by Dr. Hope, in p. 332 and 587 of his Treatise; several have presented themselves to the writer, and he believes nearly all to be of this nature in which the pulse is peculiarly slow—as, for instance, below fifty. In the generality, the first sound is feebly audible, as well as the second, while there is no pulse or impulse, and in a few I have heard the first sound accompany some pulseless beats and not others, while the recurrence of the second sound at the correct intervals, as well as the peculiarities of its nature, proved that the sounds intermitted were the ventricular. Dr. Badham does not appear to be aware of the fact attested by the distinguished physiologists who witnessed the experiments of Dr. Hope on the ass—that both the sounds were produced in perfection by the ventricles, while the auricles remained immoveable.

I am, sir,
Your obedient servant,
Auscultator.

May 1, 1832.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

An Account of the Life, Lectures, and Writings of William Cullen, M.D. Professor of the Practice of Physic in the University of Edinburgh. By John Thomson, M.D. F.R.S. L. & E. Professor of Medicine and General Pathology in the University of Edinburgh. In 2 vols. Vol. I. Blackwood, Edinburgh.

Whatever the old admirers of Cullen may fancy to the contrary, we believe that the present generation in the medi-

cal world has been singularly unmindful of its obligations to that most distinguished luminary of the Scottish school. Half a century has scarcely elapsed since Cullen's fame was in its meridian, yet it seems but too true that half a century can effect much in the way of obscuration of well-earned celebrity: it was half a century, however, be it remembered, that included, and still includes, within its span, the names of many illustrious individuals. Both the Hunters (but John in particular) and Baillie, and Bichat, and Bell, by their example, and the striking nature of their researches, have taught us moderns to think less of authority, and more of the value of actual observation, than was the case under the Cullenian dynasty. System has lost much of its charms for the rising race of physiologists, and it is certain that the dogmas of Boerhaave, Hoffman, Stahl, and even of Cullen himself, are for the most part now eschewed as religiously as those of Van Helmont and Paracelsus. Whether this be a change for better or for worse, in the mode of conducting modern medical education, posterity will be best able to decide; but meantime we believe it may be safely affirmed that all who have had leisure to inquire into the labours of the old school, have risen from their inquiry both surprised and improved. Professor Thomson candidly confesses (so, at least, we understand him) that his admiration of Cullen's genius and labours was neither the result of education, nor tradition, nor was it intuitive: it grew upon him in the course of his researches while engaged in collecting materials for the present biography; and it was, as he says, the desire excited by the perusal of the papers and writings in his possession, to do justice to the memory of Cullen, that urged him to persevere.

There can be no doubt but that Cullen made for himself a character that was truly European, (if we had an epithet indicative of a still wider extent of celebrity, we should use it;) latterly, however, his fame has been far more European than English or Scottish—more foreign than national—and the grounds of it perhaps better understood abroad than at home. One cause of this we have already alluded to; another is, that we have never had till now an authentic history of the man.

The fame of Cullen, which for the last fifty years has been floating over the ocean of medical literature, without a pilot to protect it from running among the breakers, or a commander to save it from the pirates, and the hostile attempts of the enemy, has at length obtained both in the person of Dr. John Thomson, an officer well versed in all the soundings that may present themselves in the course of the long and difficult voyage that is before him, and valorous in a high degree to fight nobly in defence of the gallant ship committed to his charge. But, metaphor apart, this is a very valuable performance, and a rich accession to our medical literature, while it is, indeed, a tribute but too long due to the memory of an illustrious man. We shall be anxious to have the remaining volume, which is to complete Dr. Thomson's view of the life and labours of Dr. Cullen: the one before us goes down no farther than to his sole appointment to the chair of the practice of physic in 1773; but we scarcely remember to have seen any biographical work that contains a larger store of medical learning. Here we have a full and comprehensive digest of the physiological doctrines of Stahl, Hoffman, Boerhaave, Haller, Whytt, and the French school of Montpelier and Paris, up to the period of Cullen's accession to the chair of the theory of medicine: after which we find an ample analysis of the lectures of Cullen in the several branches which that various genius from time to time professed. Some remarks on medical education, originating in the attempt made by Cullen to prevent the sale of degrees in certain Scotch universities, add much to the interest of the volume: nor should the appendix be forgotten, which is enriched with a number of hitherto unpublished letters, and other documents of an authentic and illustrative description. On the whole, we are so much pleased with what we have here found, that we will expect the completion of the work, as well for our own personal gratification as for the credit which it is likely to reflect on the profession to which we belong.

It is extraordinary what a mass of errors have been going the rounds of the biographical collections under the title of Lives of Dr. Cullen: those errors, too, happen principally to relate to the early portion of his life, about the time when his acquaintance with

William Hunter first began; and what makes the matter more extraordinary is, that there is in existence an excellent biography of W. Hunter, written in Cullen's lifetime, and containing the data for correcting those errors. Yet such has been the indolence or inveterate stupidity of the compilers of successive lives of Cullen, that they not only copy one another most servilely, but in some instances, where the memoirs of both Cullen and Hunter are given in the same volume, the grossest contradictions are allowed to stand and give the lie to each other. One example of this may be observed in the thousand-and-one-times-told tale of the compact between Cullen and Hunter, as brother students in very straitened circumstances, bargaining to study and keep shop season about, and in consequence of which Cullen was enabled to attend the classes at Edinburgh for one season, and so forth. Now Dr. Simmons, so early as the year 1783, set this story right in his excellent account of W. Hunter: but those who undertook to write memoirs of Cullen did not conceive that they had any thing to do with Simmons's work, or, in fact, any thing else to do than to work up what was already written about Cullen. We observe that Professor Thomson takes no notice of this absurd story in his work: our reason for noticing it is, that it has been not long since repeated once more in a volume professing to be a popular manual of medical biography. The facts, authenticated by dates, are these. Cullen had completed his studies at Edinburgh, and was settled professionally at Hamilton, when Hunter was introduced to him, and became his pupil. Hunter was at this time 19 years of age, Cullen 27: they lived together in this relation for about three years, and then it so fell out, that W. Hunter, coming to London to improve himself, was induced to settle here. Nor is it true, what is so pathetically reiterated by the compilers, that after this separation the two friends never met again: they met in ten years after, when Dr. Hunter paid a visit of a few weeks to his native country, and they always maintained a most intimate and warm intercourse by letters. Another story recently repeated, though long since refuted, and shewn by the authenticated statements in the work before us to be utterly false, is that which attributes Cullen's first appointment and settlement in Glasgow to the Duke of Hamilton, to whom he (Cullen) was introduced by the Duke of Argyll, the former nobleman never having contemplated, and being in fact deceased before, any such appointment, and the latter not being known to Cullen till above ten years after. But it is needless to pursue this exposure further: if readers will henceforth, after due warning, consult the lying oracles in place of the true one, the fault is theirs, and not ours.

In turning to those passages in the Life which struck us as the most noticeable in our perusal, we find that we have particularly marked the following: —"In entering upon the duties of a teacher of medicine, Dr. Cullen ventured to make another change in the established mode of instruction" [the first was the practice of unwritten lecturing] "by laying aside the use of the Latin language in the composition and delivery of his lectures." This was, indeed, an important change, of the extent and boldness of which no adequate notion can at present be formed; it will, however, be generally appreciated as mainly instrumental in aiding that display of talent for which the Scotch medical schools were so soon afterwards distinguished. The fetters—the remnants of the scholastic times—which would have tied and bound both teacher and learner engaged about a new science, such as chemistry, and some of the other then newly-opened branches of medical inquiry, were now happily removed, and the rapid improvement which has ensued in almost every instance wherein the example has been followed, cannot but impress us with a deep sense of gratitude to Dr. Cullen, in so important a respect the first successful innovator.

It is another of the current stories about Cullen that he was *invited* to Edinburgh to fill the chemical chair. Nothing can be more remote from the truth: and the whole truth is now known. It was by the dint of management on his own part, and that of a few staunch friends, that he was ever enabled to teach in the Edinburgh school. "It happened," says Professor Thomson, "fortunately for Dr. Cullen's success, that the Duke of Argyll arrived in Edinburgh during the canvass for the chemical chair. Finding the wishes of those who had a right

(the Town Council) to nominate a successor to Dr. Plummer divided *chiefly between the other two candidates*, his Grace was under the necessity of employing the whole weight of his influence in favour of Dr. Cullen; and, *but for the exertions of that public-spirited and intelligent nobleman upon this occasion, it seems doubtful whether Dr. Cullen would ever have obtained a seat in the University of Edinburgh.*" Nor does it appear that even with all this interest and interference in his behalf, he was enabled to take that position at once to which his talents and reputation so well entitled him: he was obliged to contend for months with some of his brother professors, who actually entered a protest against the informality of his election. And, perhaps, we should be still nearer to the truth if we put years for months: for years of struggling against repeated cabals did elapse before Dr. Cullen was fixed in his proper place. In 1755 he received his first appointment in Edinburgh: it was not until 1773 (when now in the 63d year of his age) that he was appointed sole Professor of the Practice of Physic, upon recording which event Dr. Thomson gives utterance to the following appropriate reflection: — "Such were the difficulties to be overcome, and such the exertions required to procure, first, a place in the University of Edinburgh, and afterwards the proper situation in it, for the man whose genius, talents, and industry, shed such a lustre over the institution, and contributed in so remarkable a degree to extend and to perpetuate the fame of its medical school!"

MEDICAL GAZETTE.

Saturday, May 12, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

MEDICAL SOCIETIES.

THE silly manœuvre by which a few persons in a certain assembly have contrived to seal with final disgrace all their late absurd proceedings, (implicating necessarily, in some degree, the body to which they belong) has attracted so much contemptuous notice from the better-informed portion of the public, that we cannot refrain from recurring to the circumstances, and saying something on the nature of such societies generally, and their present condition, as considered with reference to the objects for which they were originally intended. Never, perhaps, since the first establishment of the bodies in question, was the public eye so intently fixed upon their proceedings as during the last six months; and that eye was invited and allured in every possible way, be it remembered, by the chief actors in those scenes. All that puffs, squibs, garbled reports, advertisements, and announcements of accommodation for all comers, could do, was done; the drama has now been performed; the curtain has fallen; the actors have retired into their original insignificance; but the public, who all but damned those egregiously foolish exhibitions during the representation, have not forgotten their general effect, and confess themselves perfectly astounded by the last act of consummate absurdity by which those antics have been distinguished. That this "most lame and impotent conclusion" was not, and luckily could not be, attended with any mischief, was not the fault of the actors; they did their best to precipitate themselves into this very folly long ago; they would have done what some of their lively *confrères* of the Parisian hospitals have recently done; they would, if left to themselves, have signed what might have proved a death-warrant for thousands of their fellow-citizens; but luckily till the danger was over they were not wholly abandoned to their own counsels: as soon as they were, the consequences that might be anticipated were immediately seen.

It is a very mistaken notion which, we fear, but too many of the junior folk who are about to enter the profession are apt to fall into, that debating clubs for the *viva voce* discussion of medical topics, are of singular benefit in the advancement of medical science. That certain older people encourage the notion, and are anxious to support that sort of establishment from which they derive all the notoriety that they are capable of acquiring, is well known in the profession—but, unfortunately, not sufficiently so, out of it. There are few well-informed medical persons who do not understand, and have not their eyes fully opened to, the true nature of the system. Where is the fit arena for all those who, eaten up with a bad ambition, an overweening confidence in themselves, and a craving appetite for notoriety of any sort, are yet as destitute of talents and learning, and of the other requisites for professional success, as they are of modesty and good sense? Is not their fitting-place in a bear-garden, such as we have seen some societies of a till-then respectable name recently converted into? Our ambitious gentry know well that it is so: they frequent the element that is suited to their condition: they, in a great degree, only comply with the necessities of their nature: but it behoves the lookers on to see that they do no mischief, and above all, that they entrap no unwary associates into a participation of their "pranks before high heaven."

Time was when the Fothergills, the Hunters, the Brocklesby's, the Simses, and the Lettsoms, (all, by the way, members, and some of them presidents and founders of one of the societies to which we allude) deemed it advisable to meet at stated periods, and to communicate to one another the results of their invaluable experience, not by merely verbal flippancy, or quackish displays of miserable self-importance, but through the medium of well-digested and original papers, by the publication of which the world has much profited. The volumes of the *Medical Observations and Enquiries*, and the library which was founded by the society, (still existing, though, alas! how fallen) which regularly published those volumes, must stand up as a lasting memorial of the motives which led to the first formation of those learned associations. There were at the period of which we speak no public journals suited to receive their communications—no professional periodicals expressly set apart for the publication of such papers—and the example of the Royal Society, and some of the learned bodies on the continent, taught them that there was no alternative but to co-operate by subscription, and have Transactions of their own: hence the origin of the oldest medical society in the metropolis. It has been said that the corruption of the best things becomes the worst: but there is another body that dates its origin from a much later period, and which without ever having published Transactions, or having, perhaps, any worthy of being published, or being possessed of a library, or probably intending ever to possess one — may serve well to keep the other in countenance. Whatever may have been the intentions of the founders of this latter society as to the principal objects which were to be promoted—and it is not so ancient an institution as that its original objects should be already apocryphal—it is now pretty well known to the profession as a mere debating club, and to the public, especially within the last six months, on various occasions, to be no better than a common "ring" for riotous disputation.

To such base uses, we are aware, may

some of the best things come at last. But what is the end to be in the case before us? Shall the proceedings of this last session be made a precedent for outrages still more gross in sessions yet to come? Or shall not what has passed be recorded as utterly disgraceful in the Societies' annals, and made the date of a reformation as extensive as the occasion which calls for it is grievous? In short, are all the talent, learning, science, and respectability which have hitherto been enrolled among the members to be scared away—and all hopes of amendment cut off—in consequence of the egregious misconduct of a noisy crew who, though capable of creating so much confusion and mischief, are yet, we are sure, a mere handful when compared with the more orderly members upon the lists? As long as the present state of things is permitted to continue, it is perfectly out of the question to expect that those will lend their countenance at the meetings who might, if present, act as a check upon the intemperance, and a stumbling-block to the ignorance, of the chief exhibitors. And if they thus withhold their countenance, (as we are far from advising them not to do) we shall call upon them to go farther, and withdraw their names also from those associations; nor shall we, we promise for our part, ever be guilty of giving publicity to any of their future proceedings, unless their character be entirely altered.

But the truth is, that a radical change must be effected in the conduct of those unruly bodies. If they will not choose to be deserted by all good men, or perhaps cried down for perfect nuisances, they must choose a model, and be reformed accordingly. What we should recommend them would be to return to the sober, steady, and simple regulations of their founders: but if they are desirous of something more showy, something endowed with a more extensive range of object, in which every body may find wherein to make himself useful, at the same time that he has a fair field for the display of his peculiar talents, let them study to imitate the French Academy of Medicine. But in this let them avoid making any mistake. The French hospital surgeons, who recently signed the anti-contagion protocol after four days' experience, and whom our worthy Westminster Society most gallantly imitated, are not the French Academy. This body has never perpetrated a deed so rash. The Academie de Médecine is an institution which watches over the interests of all classes of the profession; and which not only protects the profession but the public, against the encroachments of ignorance and quackery; it is a body which, in its internal arrangements, is so admirably managed, that every possible subject of medical inquiry has its special section to which it may be referred; and which, in its external relations, is so well ordered as to have its commissioners abroad, in foreign countries, in search of knowledge wherever it is to be found! This, we repeat, is not the body which has committed itself by a rash and wrong-headed decision about cholera.

Above all, it will be incumbent on them, whatever regulations they adopt, to enforce a strict obedience, for the future, from all their members: if they allow the turbulent and notoriety-loving few ever again to expose themselves, and to degrade the Society for their own paltry and selfish purposes, as they have done, they will deserve nothing, and, we trust, obtain nothing, but unmitigated contempt from the public, and incur the entire neglect of the whole medical world.

CHOLERA IN IRELAND.

Our letters from Dublin, which are up to Wednesday last (9th), leave it rather uncertain whether the disease has yet reached its acme there. The new cases on Tuesday were 108, though the Board were congratulating the inhabitants the day before on the number having fallen to 79, from 101. The Townsend-Street Cholera Hospital contains at present about fifty patients, the majority of whom are doing well; but there are not above five of them cases of genuine blue disease.

The malady has decidedly altered its type since first introduced. Not one patient in thirty has it now in the original form. Fever, with cramps, vomiting, and sometimes purging, is the form which at present is most prevalent; and it generally yields to antiphlogistics.

A patient was carried into the Townsend-Street Cholera Hospital on Tuesday night. She was far advanced in pregnancy, and by a stethoscopic examination it was ascertained that the child was alive. On Wednesday the woman was dying, and, from the cessation of the fœtal pulsation, it was judged that the child was already dead. This was the second case in which it had been observed there, that, in cholera, the child in the womb died before its parent.

Trinity College has been visited by the disease. A case occurred there on Sunday night, which terminated fatally in twelve hours. Another bad case occurred on Tuesday, and the utmost alarm was prevailing through the University.

In Cork the disease seems to be modified as in Dublin. In both cities much *cholerine* has been experienced.

COLLAPSE OF CHOLERA—SUCCESSFULLY TREATED BY SALINES.

To the Editor of the London Medical Gazette.

36, Paternoster-Row, St. Paul's,
May 11, 1832.

Sir,

I take the liberty of forwarding for your perusal, and for insertion in your journal, if you think proper, the details of a case of cholera which I have been called upon to attend since the appearance of that epidemic in London.

I also inclose a little pamphlet published by me prior to the first case having occurred in London—indeed, immediately after the disease broke out in Sunderland, by which you will perceive that the use of soda and ammonia was first successfully practised by me in India in the year 1827, and was by me strongly recommended to the profession here; and I have understood that soda has in several instances been given with decided success.

I shall not stop to inquire whether or not those who have tried it had read my pamphlet; but I do conceive that, should soda and ammonia prove a specific for this disease (which I have strong reasons to think they will), I am fully entitled to claim the priority of the discovery.

I remain very truly, sir,
Your most obedient humble servant,
John Goss.

John Johnson, aged 42, porter to an oil warehouseman in the city, residing at No. 6, Fisher-Street, Red Lion Square; temperate and cleanly in his habits, well fed, clothed, and lodged; of previous good health; was attacked on the evening of Friday, May 4th, with diarrhœa. The bowels acted seven times before bed-time. He passed the night without any disturbance, the bowels remaining quiet till the morning, between which and half-past 10 (my first visit) they acted four times, with considerable griping pain.

I first saw him at half-past 10 on Saturday morning, at his master's, as he had come to his work, as usual, a distance of a mile and a half. He was suffering much from the pain and diarrhœa, and I accordingly gave him an aromatic cretaceous mixture, containing a small quantity of Tr. Opii, to be

taken after each evacuation. This was instantly vomited, and he continued getting gradually worse up to 3 P.M. when his master sent him home in his cart.

I visited him again at 5 P.M. and found him in bed, vomiting, with a cold clammy skin, the face and hands of a perfect lead colour, suffering very acutely from cramps, which had so much of the character of opisthotonos as to raise his buttocks from the bed; the pulse quite imperceptible at the wrist. He passed a stool of perfect rice-water appearance, with a large quantity of flocculi in it. The skin of the hands was shrivelled, and the countenance so changed as scarcely to be recognized as the same individual, although I had known him for two years. His thirst was most urgent, and his voice feeble. He had passed no urine since the previous night.

Quarter past 5.—I gave him sodæ carbon. ʒj. in water, *after which he never had any spasm.* In a very few minutes the pulse began to be perceptible, and he said he "felt as if in heaven." At half-past 5, I gave ʒij. more, and then applied a large mustard poultice to the epigastrium. The pulse continued improving, the leaden hue to diminish, and the warmth of the skin to return. At a quarter past 6 P.M. I gave him Sodæ Carbon. ℈iv. Ammon. Carbon. gr. xiij. Aq. ℥iss. As the poultice distressed him, it was occasionally shifted. A quarter before 7 P.M. the soda and ammonia were repeated, and almost immediately rejected; at a quarter past seven it was again given, and retained; at 8 P.M. the bowels again acted, the evacuation bearing the same character. Up to this time friction had been used to the extremities with flannel, and for the last hour he had been rubbed with the Bochnia Spiritu, or Jew's remedy* (having a man at each extremity), which was now discontinued, from the great irritation it produced, so much so as to cause extensive vesication of the right arm. The pulse gradually rose to 90, and he broke out into perspiration, which shortly became profuse.

Ordered Sodæ Carbon. ʒss. Ammoniæ Carbon. gr. xxv. Sp. Aq. Menth Pip. ʒij. Mist. Camphor. ʒx. to be taken with Acid. Citric. ℈j. in a state of effervescence, and a pill containing Hydr. Submur. gr. iij.

At 9 P.M. repeated the pill and draught. I now found his stomach and bowels quiet; perspiration very profuse; pulse full, soft, about 90; tongue tolerably clean; thirst abated.

Ten minutes past 10 P.M.—Has slept soundly since the last draught and pill; repeated the pill and draught. I gave him, at a quarter before 11 P.M. four ounces of spiced beef-tea (the first thing, except medicine, which he has taken), and left him under the care of my assistant, well covered with blankets, with strict orders not to be moved, and all occurrences to be immediately noted.

Has slept since the beef-tea, which was now (quarter before 12 P.M.) repeated, and at a quarter after 12 P.M. his pill and draught. No perceptible change; stomach tranquil; perspiration profuse. Quarter before one A.M. has again had beef tea; quarter after one A.M. feels a desire to pass a motion, but on attempting could not; he has passed half a tea cupful of urine; (this, on testing, was found acid.) Is becoming very restless from the perspiration, which is very great. Half-past one A.M. repeated his pill and draught; quarter to two A.M. bowels have just acted, but scantily; took some more beef tea. Quarter past two, bowels again acted, of a darker and somewhat muddy appearance; pulse 108, soft and full. At three A.M. took some beef tea: at half-past three A.M. his pill and draught; perspiration continues profuse; thirst more abated. At four A.M. took beef tea, and soon after his bowels acted again; from this time to a quarter before five A.M. he slept. Pill and draught repeated; quarter before six A.M. pill and draught; complained of nausea; the bowels acted again. Half-past six A.M. pulse 110, soft and full; tongue rather white, but moist. My assistant now came home, leaving directions for him to repeat the draught without the pill at half-past eight A.M.

I visited him again at half-past nine A.M., and found the pulse 120, hard and jerking; much drowsiness; the eyes

* The following is its formula. Strong spirit of wine, one pint; good white wine vinegar, half a pint; powdered camphor, one ounce; mustard seed bruised, or in powder, one ounce; ground pepper, a quarter of an ounce; bruised garlick, a tea-spoonful; powdered cantharides, half an ounce. Mix well together in a bottle; let it stand for twelve hours in a warm place, and shake it repeatedly. To be used warm to the extremities.

heavy, and slightly suffused; tongue dry, and other febrile symptoms.

Venesection to ℥xviij.

This decidedly affected his pulse.

A cold evaporating lotion to the head; a large blister between the shoulders. Hydrarg. Submur. gr. vj. in pill, followed immediately by Magnes Calcin. ʒiij., Sp. Ammon. Comp. ʒss. in a bottle of soda water. In two hours to take Potass. Carbon. ℈j., Ammon. Subcarbon. gr. ij., Mist. Camphor. aa. Aq. vj., with Acid. Citric. gtt. xviij. whilst effervescing, and to repeat this every second hour. Mutton broth and soda water alternately.

At three P.M. my assistant visited him, and reported favourably; the restlessness, which was considerable after the bleeding, had now subsided.

At seven P.M., on making my visit, I found him improved in every respect; pulse 112, soft and regular; tongue clean and moist; skin perspiring; countenance and manner natural; complains of no pain except from the blister. The bowels have acted several times; the evacuations dark, muddy, and more offensive; has passed a pint of urine (still very acid); the blood, taken in four tea cups, is in three of them cupped and buffed; crassamentum tolerably firm.

Gave Hydrarg. Submur. gr. vij., Pulv. Ipecac. Comp. gr. v. in pills. To take a draught at nine, and another at eleven P.M. At twelve P.M. he is to take Ol. Ricini ʒiij., Liq. Potassæ ʒss., Aq. Menth. Pip. ℥iss., and the following draught every succeeding two hours:—Potass. Carbon. ʒss., Mist. Camph. ʒvj., Aq. ʒvj., Acid. Citric. ℈j. during its effervescence. To continue his soda water and mutton broth.

At nine A.M. ate a slice of dry toast, and took three cups of tea; passed a tranquil night, with occasional sleep. The bowels have acted three times; the motions evidently contain bile; continues to pass urine. Pulse 120; skin moist; tongue has a brown fur, but is not dry.

Gave him double the quantity of his castor oil medicine.

At one P.M. visited him again; pulse about 100; tongue much improved; bowels acted on once.

Continue his draughts. To take calves-foot jelly.

At half-past seven P.M. evidently improving in every respect; pulse 95; skin and tongue moist; only complains of feeling weak. Has passed two more bilious motions, and a moderate quantity of urine.

To continue his draughts. Hydr. Submur. gr. ij., Extr. Rhei gr. vj. night and morning. Diet the same.

May 8th, eleven A.M.—Has passed a good night; all febrile symptoms have subsided; bowels have acted once in a perfectly natural manner; pulse 86. Returned as recovered*.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

STATISTICS OF HUMAN GENERATION.

On the 29th of September, M. Mathieu read a report on a memoir by M. Giroux de Busaringues, containing a statistical account of the marriages, and the births of infants of both sexes, in France, classed in months. M. Giroux imagines the reproduction of man to be subjected to the same laws as that of domestic animals, and that whatever tends to increase the motive power of the man, or to diminish that of the woman, promotes the procreation of male children, and *vice versâ*; so that a man may render himself more or less apt to procreate boys or girls, according as he addicts himself to exercises productive of muscular force, or to slothfulness,—to sobriety, or to intemperance. M. Giroux's facts are drawn from the official returns of every part of France, for ten years, commencing from 1817. He finds that, with respect to the number of marriages, the months are thus to be arranged,—February, January, November, June, May, July, October, April, September, December, August, and March. This depends on the periods of religious festivals, and on those of rural labours, marriages being rare during those periods and most numerous in the months immediately pre-

* With the above case a letter has been forwarded to us, addressed to Mr. Goss by Mr. Clarke, of Lamb's Conduit-Street, and Mr. Steerer (?) of Drury Lane; it is confirmatory of the statements of our correspondent.—E.G.

ceding them. Thus March, the month in which Lent falls, is last on the list; while the preceding month, February, is first. The births of legitimate children are thus distributed,—February, March, January, April, November, September, December, October, May, August, June, July: counting backwards for nine months, we have the following order as that of the conceptions,—May, June, April, July, February, December, March, January, August, November, September, October. It will be observed that the months in which the greatest number of conceptions take place are not those in which there are the greatest number of marriages, which proves that conception rarely takes place in the first month of marriage; on an average it attains its maximum about two or three months afterwards. The greatest number of conceptions are in spring and summer, and the smallest number in autumn and winter; but M. Giroux imagines that this is not so much owing to the direct influence of the seasons as to the fact of each season bringing periodically the recurrence of labours and habits more or less favourable to the procreative power. Thus it appears that the greatest number of conceptions are in those months in which the strength of man is developed by exercise and labour.

POPULATION.

M. Moreau de Jonnès has lately brought forward some striking facts relative to the increase of population in Europe. Uninterrupted by those obstacles which perpetually interfere with it, the multiplication of the species would be alarmingly prodigious. A single couple, on an average, would give birth to 6 children in 33 years; and 4 of these children would survive their parents*. These 4 marrying, in their turn would double themselves in 33 years, like their predecessors; and so on. But the actual increase is far from proceeding at this rate. France, for example, which under the Romans contained 4 millions of inhabitants, has required 1860 years to attain its present population of 32 millions, that is to say, the period of doubling has proved to be 18 times greater than that above stated, the births exceeding the deaths annually by no more than 1 in 1000.

The actual rate of increase, however, in Europe, according to M. Moreau de Jonnès, is as follows:—

	Years.
Prussia doubles its population in..	39
Austria	44
Russia in Europe	48
Poland and Denmark	50
Great Britain and Ireland	52
Sweden, Norway, Switzerland, and Portugal	56
Spain	62
Italy	68
Greece and Turkey in Europe.....	70
Netherlands	84
Germany	120
France	125

Thus, the northern nations seem to require 50 years, as their period of doubling; the southern, 80. The average for all Europe is 68.

M. de J. calculates that since the Deluge, the population of the world has doubled only 28 times; and the average period of doubling has been about 150 years.

CASE OF LIGATURE OF THE PENIS.

The following is a curious case, and might be interesting as a medico-legal fact. A Jew-boy, four years old, was suckled by a nurse whom the family became dissatisfied with, and dismissed. Two days afterwards Dr. G. was sent for, on account of painful swelling of the boy's penis. On accurate examination, he found a long hair of the head five times wound round the root of the penis, precisely in the same furrow, and each time tied firmly with a knot. With great difficulty the hair was discovered and removed, from the deep incision it had made; after which, emollient applications speedily healed it, but had it remained a short time longer, gangrene would inevitably have occurred. "Very probably," says Dr. G. "the nurse wished in this manner to avenge herself for her dismissal." —*Rust's Mag.* 1831. *Quoted in the Dublin Medical Journal*, No. II.

INVERSIO UTERI.

A woman, aged 32, who had borne four children, and had a remarkable easy time with each, was delivered of her fifth child after a very short labour. Immediately after the infant came, the uterus inverted, and

* Were the whole 6 to survive, the rate would be 6 in 33 years, 12 in 66; say 24 in a century, 192 in 200 years, 98,000 in 500 years, above 30 millions in 1000 years! Thus, all France might have been peopled by a single couple living in the time of Philip-Augustus; all Europe by a couple living in the time of Hugh Capet; and the whole earth from a single family existing in the time of Charlemagne!

with the placenta attached to it. The midwife endeavoured to return it to its place, but the woman died suddenly in convulsions. The midwife declared to the physician who was called in, that she had not pulled the navel-string, or done any thing else that could have caused the accident, and her statement was supported by the testimony of some of the neighbours of the patient, who had been present the whole time. It is probable, therefore, that the inversion was owing to the width of the pelvis, which was remarkably great, and the rapidity of the labour.—*Ibid.*

FOREIGN SUBSTANCES IN THE BODY.

Glass.—A man accidentally broke a bottle in pieces in his hands; he picked out all the fragments, as he thought, and the wounds soon healed. Still, when working, he constantly felt pains in the one hand, and sometimes it used to swell greatly. In twelve years and a half after the accident, he had occasion to make a great exertion with the same hand, whereupon he felt a violent pain in it, which lasted for some weeks. An opening then appeared on the palm, and a surgeon on probing found some foreign substance, at the depth of about half an inch from the surface. He enlarged the opening, and extracted a splinter of glass one-third of an inch long, and a line in thickness, together with three smaller pieces, which had probably been broken off from the larger one.

A Needle.—A man felt a stinging pain in the upper part of the right arm; a surgeon put a plaister on the spot, and for years after he felt neither pain nor swelling. On a sudden, he felt the same kind of stinging at the opposite side of the arm, and a needle nearly three inches long was extracted from it. He could not account for how the needle got into the arm at all.

A Pin.—A woman got a blow upon her breast, and thought she felt a pin stick in her. Seven years afterwards, as she was washing herself, she observed a pin protruding from the skin of the part, and succeeded in extracting it.—*Ibid.*

HUNTERIAN MEDICAL SOCIETY OF EDINBURGH.

(From a Correspondent.)

At the last meeting of this society for the session which has just terminated, Mr. Harrison read a paper on injuries of the head; and Mr. W. Rogers, senior-president, delivered (as is usual) a valedictory address. Mr. Rogers took a retrospective view of the proceedings during the session, and stated that it gave him great pleasure to announce, that in the accession of members, as well as in interesting communications and valuable papers laid before the society, this had been one of the most fortunate sessions since its institution. Taking leave of the society for a while, he enlarged on topics of more general interest, particularly on the all-engrossing subject of cholera. He remarked on the prompt and efficient attention which this fearfully important subject had received, both in that society and in that city, and quoted the highly-flattering eulogium of Dr. Chalmers, on the medical arrangements of Edinburgh—arrangements which had been the theme of admiration unqualified and universal. This led to some observations on the reception which the deadly visitant had met with in London, and on the earnest but unheeded warnings of the medical journals. A short review was taken of the progress of medical literature during the past year, adverting particularly to the Cyclopædia of Practical Medicine, and the Dublin Medical Journal; the latter of which led the way to some remarks on the distinguished medical corps, and rising medical schools of that city—a city which presents the anomaly of great advantages little improved.

Mr. Rogers delivered his address to a very numerous audience. It was throughout characterized by chaste composition and dignified sentiments—but of the effect produced by the introductory and concluding remarks, it would be difficult for one not present to form any adequate idea.

ROYAL INSTITUTION.

Friday, May 4th, 1832.

On the Application of Cast Iron to Bearing Purposes.

Mr. Cottam, who delivered a very interesting lecture on this subject, dwelt principally upon the law discovered by Mr. Tredgold, of the correctness of which he gave, in the course of the evening, several practical examples. The principle of the law is, that the bearing power of cast iron is to be estimated from its elastic, and not from its

breaking force. Great mistakes have been committed in this respect by engineers and architects; some studying economy at the expense of safety, and others safety at a very extravagant sacrifice of money. The difference of opinion amongst them with regard to a particular project of this nature, has on some occasions amounted to an enormous degree, one party maintaining that such an estimated cast-iron supporter would be 13 or 14 times too strong; another, that it would not be *by half* strong enough. Such variances arose from adopting wrong rules, these gentlemen taking, in fact, the breaking force into their calculation instead of the elasticity of the material. Mr. Tredgold's formula for ascertaining the elastic force is a function of the length and breadth with a constant multiplier 850, the result being the force expressed in pounds weight. Mr. Turnbull adopts it, and it has been also approved of by Professor Leslie. In estimating practically the elastic force of a bar by suspending weights to it, it is found that the weight suspended from the centre is to the weight uniformly distributed as about 6 to 10. A bar of cast iron also possesses an elasticity inversely as its length: thus, the bar 5 feet in length and an inch (square) in breadth, which supports a weight, suspended from its centre, of 189 ℔ (which is the elastic force of a bar of those dimensions); if reduced to half the length will support 378 ℔. It has been common to calculate the bearing power of cast iron at one-third of the load that will break it: but this Mr. Cottam proved to be an unsafe criterion, as the bar, for example, whose elasticity would be destroyed by exceeding 189 ℔, would require 1500 ℔ to break it. In illustration of his doctrines, Mr. Cottam broke several bars in the course of the evening, to the no small discomfiture of some nervous gentlemen among the audience.

The lecturer, among other practical remarks, exposed the mischievous practice of studying fanciful forms in the casting of iron girders and supporters, by which, with little or no saving of metal, much difficulty was created for the founder, and much disappointment generally for all parties. It was almost utterly impossible, he shewed, to avoid sand blows, wind flaws, and other accidents in the casting. Ludicrous mistakes had also been made in trusting, for economy sake, stone bases for the support of metal roofs and pillars: the bases have been *punched completely through*. Also the bearing powers of floors have been occasionally grossly miscalculated. According to Mr. Cottam, in the most densely-crowded throng, the pressure on each square foot of flooring cannot exceed 120 ℔; and in a ball-room, allowing an average of 6 feet square to each person, the pressure is not above 28 ℔ to the square foot; or 60 ℔ to the square foot, if all the dancers spring together; and Mr. C. said, that he allowed 12 stone for the weight of each lady and gentleman present!

In the library, among other novelties exhibited, were two new forms of the camera lucida; also Redfern's new patent gun, which discharges its two barrels successively with one trigger, and the construction of which is as safe as it is simple.

ADDRESS

TO THE

STUDENTS OF THE CLASS OF MATERIA MEDICA & THERAPEUTICS,

OF THE

UNIVERSITY OF LONDON,

At the Close of the Session, 1832.

By Dr. A. T. Thomson.

I have now, gentlemen, brought this course of lectures to a conclusion, and I might take my leave of you for this session by wishing you all health and happiness; but this is not my present intention. It is utterly impossible to say how many of us may meet again; I am therefore unwilling to lose this opportunity of addressing a few parting words to you, in the spirit of friendship—in the anxiety of a teacher—in the solicitude, I would almost say, of a parent.

You have all, gentlemen, chosen your profession; and, to enable you to acquire that knowledge which is requisite for practising it with credit to yourselves, and advantage to society, you have flocked to this University for the instruction which its theatres afford. Most of you have attained that interesting period of life, between boyhood and the moment when a man begins to regard himself as fitted to engage with the entanglements of common life—to fill a part in the great drama of society—to risk the misery, or to enjoy the happiness, which it may bestow—and to struggle for those honours of time and immortality, which a mind properly regulated cannot fail to long for, and the hope of which is the most cherished of the anticipations, short of those which religion affords, attending this sublunary state of existence. Need I urge you to make a proper use of this irrecoverable hour, "which," in the emphatic language of a distinguished orator, "is sweeping by you, not only with the velocity, but with the fearful silence of the wind." I do not mention this fact because I have remarked symptoms either of indifference or inattention amongst you; on the contrary, I am fully satisfied with the interest which most of you have taken in the objects of this course; and I am authorized

by my colleagues to express the same satisfaction on their parts. Were even reproaches necessary, the expression of them would be checked by a retrospect of the time when, like yourselves, I sat as a student, to gain that information which it is now my duty to communicate. I well know what is often felt by youthful minds for the folly of past frivolity—how keenly they are accused by that inward monitor, whose whisperings, occasionally stifled, are frequently listened to like the admonitions of the warmest friend; and, to any who have neglected the opportunities which they have enjoyed of gaining instruction, I simply say, listen to that feeling of regret which those whisperings inspire; it will prove to you the day-spring which shall chase the shades of darkness from your path, and open to you a glorious day of exertion and success.

In the four sessions which have passed in this University, one thing has most forcibly pressed upon my attention, as arising out of the nature of the plan of education adopted in it. I refer to a change in the opinions of students respecting those attainments which, if they be not absolutely necessary for the practice of the medical profession, at least cast a lustre upon it; these pursuits, both of classical literature and of science, which follow gracefully by its side, relieving and heightening the labours of the practitioner.

Now is the time, in the interval between the present and the next session, for those who feel themselves deficient in classical learning to supply that defect. Much is not required, but some portion is essential. No time is too late to gain it; and in the resolution to acquire, the task is already half accomplished.

What are the rewards of erudition and science? Look, I beseech you, on those two characters, the learned and the scientific practitioner, and he who merely works for lucre. The latter may gain his object, but when it is attained, he is unequal to the enjoyment of it; on the contrary, the scientific practitioner, like the practical warrior, is prepared for every fortune. Concentred in himself, if he do not succeed, he has still much enjoyment; if he succeed, he rises equal to the highest rank to which he can possibly be elevated by well-merited success. Whilst, however, I thus advise you to close and assiduous application, it is my duty equally to warn you not to carry it beyond a certain point: labour is sweetened by relaxation; without this, study and business resemble a state of disease: they swallow up all other considerations, and are utterly subversive of the exercise, sometimes even of the consciousness, of the best affections of our nature. A recent critical writer has truly said:—"there is something particularly humanizing in the study and practice of medicine. No profession is so distinguished for its taste in general literature, for liberality in matters of religion, and for every-day benevolence, which turns out in all weathers, and answers to all claims." Be it your aim, gentlemen, to maintain this character of your profession: and let me urge those amongst you who are destined to practise in the country, not to be misled by the fallacy, that your exertions are to be cramped because your sphere of action is not to be in crowded cities and towns; for no opinion is more erroneous than "that the motives of individual exertion rise in proportion to the magnitude of the scene." All cannot settle in the metropolis, or even in large towns; and you ought to be informed, that it is seldom there that the highest qualities of the practitioner shine forth most srikingly; on the contrary, like wisdom and virtue, they often lose their attractions on such a stage. Settled in the metropolis, fashion, which sways every thing, also too frequently determines the reputation of the physician; and, with the exception of a few illustrious examples, whose genius would pierce the darkness of the densest atmosphere, and shed a radiance on any spot, how small a number of the doctors, whose chariot wheels make much of the mimic thunder which day and night rolls upon the ear in this great abode of men, have effected any thing by which they have left their profession better or wiser than they found it. If London, in modern times, can boast her Fordyce, her Hunters, and her Baillie; have not the provinces their Darwin, their Currie, their Hey, and their Pritchard? The fashionable physician, it is true, allures the eye of the young student by the splendour of his establishment: it appears an enviable state; but, when its possessor dies, he falls unmissed: his place, like that of a soldier in the bustle of the field of battle, is instantly supplied;—"the machine (as it has been well expressed) goes on without perceiving that the blue-bottle or the gnat has fallen from the wheel." I mention these things, fellow students, to shew you the necessity of study in all situations. It is the consciousness of knowing our profession well, that is our source of happiness: it is the proper exercise of it wherever our lot is cast, which quickens the moral pulse, expands the heart, and elevates the practitioner; and, when duly practised, it is, indeed, one of which you may be justly proud.

> "A wise physician, skilled to heal,
> Is more than armies to the public weal."

To enable you, students, to attain this character, your professors labour; and they are well aware both of their responsibility and their reward. They feel proud in the honourable stations which they hold; they are conscious of the extent of their duties, and their inclinations and their feelings

prompt them to endeavour to perform them with credit, and in such a manner as shall gain them distinction and respect. They know that they must learn, in order to be able to teach; and it is their object, by constant cultivation, "to keep themselves, if not in the van, at least abreast of the advancing knowledge of the age." They know the pains and the penalties of the rank to which they have attained, but they also know its pleasures, and there is not any enjoyment so great as that of watching the opening germs of youthful excellence, and, wherever they appear, fostering them by personal kindness. The real interest which they feel in the future prosperity of their pupils has been more than once evinced by the situations which some of them have gained by their means, and in the manner which an appointment to India, lately placed at their disposal, was bestowed. They well know, that although talent will burst through all obstacles, and find its way in spite of every opposition, like the fire which burnt most intensely from the weight laid above it; but they also know that many natures, calculated to shine hereafter, require support at first. In the language of an eloquent writer, "There are imaginations darkened by clouds of their own creation, over which the purest light might be shed. There are temperaments not yet irrecoverable, in danger of becoming permanently and wretchedly morbid;" and knowing this, they feel that every germ of improvement should be cultivated, and are most anxious, both as friends and as instructors, to make the soil congenial for the seed which they have to cast into it.

Such, gentlemen, are our relative situations as students and as professors—such the duties we have respectively to fulfil, as teachers and as pupils. In one respect all distinction is broken down; in one thing the same obligation is placed upon both—in the duty which we owe to this rising seat of learning. We, as professors, feel that to our exertions it must owe its permanent prosperity. It possesses in itself every thing requisite which can contribute to form the accomplished physician, as far as respects elementary instruction; and the morning of the day is now dawning which will place its medical school on such a basis as will render it admirably fitted to direct you in the right path for practical success.

Gentlemen, much may be done by you, also, in advancing the celebrity of the University: its reputation can only live in the lives of its illustrious students. Every one of you is the atlas upon which its celebrity is to rest. Do not disappoint its hopes. Wherever you go, remember what you owe to this University—your alma mater. In the words of one whom I have already quoted, "it is not bound to be satisfied with the gratitude of words, but is entitled to the genuine homage of a well-spent life." In doing our utmost to promote its prosperity, let us all unite our best efforts. Every one may contribute something. The mite thrown into the treasury is not a worthless offering. Let us cast our eyes steadily before us; the land of promise spreads fair and wide, and inviting; if each bring his intellectual weapons into the combat, its conquest is achieved.

"Hoc opus, hoc studium parvi properemus et ampli;
Si patriæ volumus, si nobis vivere cari."

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

ST. BARTHOLOMEW'S HOSPITAL.

Injury to the Head, attended with a profuse discharge of colourless fluid from the external Meatus of the Ear.

Harriet Hardy, æt. 18, of robust constitution, was admitted into the hospital March 8th, under the care of Mr. Earle. On the morning of the 5th, whilst in the act of cleaning a sky-light, she lost her balance, and fell a height of thirty feet, her head striking the ground. She was taken up quite insensible; she soon, however, recovered herself, and was better on losing some blood. Nevertheless, she suffered much from the effects of the accident, which induced her to apply at the hospital. On her admission, there was observed a slight wound of the scalp over the occiput; she complained of very severe pain in the head; the countenance was flushed and very anxious; the pulse quick and full; great pain and throbbing in the left ear, and a profuse discharge of limpid serum from the corresponding meatus. She was immediately cupped from the back of the neck to the extent of sixteen ounces, and well purged.

9th.—Much relieved by the cupping; the discharge from the ear very profuse, wetting the greater part of the pillow.

The cupping to be repeated, and the head shaved.

10th.—In about the same state as when she was admitted; great pain in the head, aggravated by the slightest noise; intolerance of light, with knitting of the brows; countenance very anxious, and suffering acutely in the left ear, from which the discharge continues unabated.

Emplastrum Lyttæ nuchæ. Cal. grs. iij. Opii, gr. ½, 6tis horis.

11th.—The intense headache continuing, ten ounces of blood were taken from the temples by cupping.

13th.—Somewhat better; the pain and intolerance of light however still continue.

Ice ordered to be kept constantly to the head.

These symptoms remaining unabated, it was thought there might probably be a fracture at the basis of the cranium. She was several times observed to support and steady the head with her hands. The discharge from the left ear amounted probably to four or five ounces in the course of the night. The meatus was carefully examined, but not the slightest wound in it could be discovered, or any injury to the membrana tympani. The cuticle lining the meatus appeared very soft and villous.

15th.—The discharge from the ear has suddenly and entirely ceased, but has been followed by intense sensibility to sound; is better, but passed a very restless night.

17th.—Is improved, but still very irritable, and disturbed by the slightest noise. The blister on the nape of the neck was ordered to be kept open, and ice to be continued to the head, from which she expressed herself as deriving great benefit.

An erysipelatous blush made its appearance on the 18th over the face and forehead, attended with considerable tumefaction; this was subdued by leeches, with calomel and antimony. From this time her improvement was daily progressive; every bad symptom entirely disappeared, and in about three weeks she was perfectly well. A small quantity of the fluid from the ear was analyzed, and its component parts were found to consist of albumen, water, and a small portion of cerumen. No return of this discharge has since taken place, and her hearing is perfectly natural.

Hæmatocele cured by an Operation.

William Jefferson, æt. 48, by profession a sailor, having served under Lord Nelson in several engagements, was admitted into the hospital February 17th, with a swelling in the right half of the scrotum, nearly the size of two fists. He stated that some years back he had received a blow on the part, which caused him considerable pain at the time, and was shortly after followed by a gradual swelling. When the first effects of the blow had subsided, he suffered no inconvenience except what arose from its bulk. He had been treated by several medical gentlemen without success; some considering it to be an hydrocele, others a diseased testicle, and upon this supposition mercury was administered. On a careful examination of the tumor, Mr. Earle gave it as his opinion that it was an hæmatocele. The length of time it had existed, the peculiar elastic feel, different from that of an hydrocele or fungoid disease of the testicle, were the reasons for such an opinion. An operation was therefore determined upon, and was performed in the following manner:—

A lancet was first plunged into the tumor, and a small quantity of thick dirty-brown fluid followed; this opening was then enlarged to the extent of about an inch and a half; more of this fluid escaped, and being received into a bowl, presented a peculiar shining surface, being covered with particles possessing an almost metallic lustre. This Mr. Earle had remarked in other cases, and depending, as Dr. Bostock found by analysis, on the presence of a substance analogous to spermaceti. The sac containing this fluid was the tunica vaginalis, converted into a tough cartilaginous substance, very much resembling in appearance the gizzard of a fowl; portions of bone were interspersed in its texture, and offered some resistance to the knife. Besides the above fluid, it contained a quantity of matter somewhat similar to the layers of fibrine in an aneurismal sac, which have been broken down. This being removed, the whole of the indurated tunica vaginalis was cut away, with the exception of that portion connected with the testicle. Two or three vessels were secured, and cold lotion applied to the wound, which was left open. Three or four hours after the operation, hæmorrhage occurred to some extent. The coagula being turned out, about twenty small vessels were secured. Salines and purgatives were administered, and suppuration took place, which was quickly followed by healthy granulations over the whole surface, not excepting the small portion of the indurated and thickened tunica vaginalis which had been left behind. The wound quickly closed, and the patient has left the hospital perfectly well.

ST. GEORGE'S HOSPITAL.

Lithotomy—Removal of a very large Calculus.

Robert Walker, a shoe-maker, æt. 68, admitted March 28, 1832, under the care of Mr. Keate, labouring under the usual symptoms of stone in the bladder, which first manifested themselves about fifteen years ago.

On passing a sound into the bladder, a stone was immediately felt, and from the sensation communicated to the fingers on

passing the instrument over the surface of the calculus, it was thought to be one of very large size, and hard. The finger introduced per rectum, detected the stone pressing upon the gut. Prostate slightly enlarged.

Ordered to be kept quiet in bed. Broth diet.

March 28th.—Mist. Camph. ℥iss. Sodæ Carbon. ʒss. Magnes. Carbon. gr. x. M. ft. haust. bis terne die sumend. Tinct. Opii, ♏v. q. Cyatho Decoct. Herdei. ter die. Extract. Conii, gr. v. o. h.

30th.—On testing the urine for albumen, by means of nitric acid and heat, very little or none can be detected. The urine is either alkaline or acid, according to the frequency, or otherwise, of taking the alkaline mixture. It is clearer than it was, and there is less of ropy mucus mixed with it; he is very much easier, and feels much better.

April 2d.—His health has greatly improved; he now sleeps well at night, being only disturbed about twice to void his urine, which passes in a fuller stream and with much less pain, and is nearly double in quantity. The urine itself improves daily in appearance. He takes the alkaline mixture once, twice, or three times a day, according to the state of the urine in the morning. Pulse softer; skin natural; tongue cleaner, but still rather white and furred. Bowels not open without Ol. Ricini.

5th.—Improving daily, but has some slight pain in the loins.

Cucurb. Cruent. lumbis ad ℥viij.

12th.—Bowels well evacuated last night by Ol. Ricini, ʒvi. An injection could not be given per rectum, the stone not allowing of the introduction of the pipe.

1 P.M. Operation performed. First incision with scalpel was made very low down, by the side of the rectum. Bistoire caché introduced upon the groove of the staff, upon the withdrawal of which a gush of urine followed.

On passing the forceps (Assalini's curved) into the bladder, slight difficulty was experienced in getting hold of the stone, from the coats of the bladder contracting strongly round the stone, so that Mr. Keate was obliged to push the folds of the bladder on one side before he could get a proper hold; after doing which, some little time elapsed before the stone was extracted, the operator suffering the parts to dilate rather than lacerate them. About ℥x. of blood lost at the operation. The stone was very large, being about three inches in its longest, and two inches in its shortest diameter. It weighed 3 oz. 6 dr. 45 grs. Externally the triple phosphates appeared to prevail. The annexed cut gives a correct representation of its size and figure. The patient had thirty drops of the Liq. Opii Sedativ. and has done well. Should any change occur, we shall notice it.

SIR PATRICK DUN'S HOSPITAL.

Violent Delirium without Tremors—No benefit derived from Blood-letting and Tartrate of Antimony till the use of Opium was commenced.

Eliza Cooley, æt. 29, a servant, unmarried; admitted to Dr. Osborne's wards 26th January, in a state of phrensy. No sleep; constant exclamations that the devil is in the apartment. Will not allow her pulse to be felt; catamenia regular; bowels free. Delirium commenced yesterday; the day before, she was in a state of strong excitement on the subject of religion, after having been exposed to great fatigue and anxiety whilst sitting up with a sick child for two nights successively.

Mit. ex arteria temporali sanguinis, ℥vij.

27th.—Last night she could not be restrained, and the waistcoat was necessarily applied.

Rep. arteriotomia; Tart. Ant. half a grain, to be taken in solution till vomiting takes place.

28th.—Was more tranquil during the day yesterday, but at night she again became furious. Pulse above 100.

Dover's powder, xiv. grs. at bed-time; continue the Antimonial solution.

29th.—Is now quite rational; slept well; some headache; bowels confined; tongue white; pulse 96.

Sennæ Mistura; rep. Pulv. Doveri.

Feb. 3d.—Since last report no return of delirium; sleep disturbed by dreams; slight pain at vertex, increased by cough; bowels free; pulse 100, short.

Vesicatorium vertici; Mass. Pil. Hyd. gr. vj. h. s.

In a few days she recovered, and was dismissed.

Proof that Blisters continue to increase the action of the subjacent absorbents for some time after they have been applied.

In a case of commencing phthisis (under the care of Dr. Osborne), a small blister was applied under the clavicle, in order to act on the part of the lung in which a moist crepitation indicated the softening of tubercles. A few days after the vesicated surface was completely healed, an application of leeches was directed to the same part. Immediately after their application each leech-bite presented the usual circle of extravasated blood surrounding the triangular incision. On stripping the patient's chest on the second day after the application, it attracted attention that those bites which were on the seat of the blister were surrounded by much paler circles of ecchymosis than those beyond it; and in a day or two afterwards it was found that the ecchymoses on the seat of the blister were quite absorbed, while those beyond it were still of a purple colour, except some which were near the margin of the blister, and which were of a greenish hue. As this fact can be explained on no other principle, except on that of increased absorption, in consequence of the previous application of the blister, it appears worthy of being recorded, and points out the use which may be derived, in the discussion of tumors, from the frequent application of blisters.

REPORT OF CHOLERA, UP TO FRIDAY, MAY 11, 1832.

New cases in London since our last report	39
Deaths	15
Total number of cases in London since the commencement of the disease	2606
Deaths	1371
New cases in other parts of Great Britain since our last report	392
Deaths	186
Total number of cases throughout Great Britain since the commencement of the disease	12236
Deaths	4954

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

May 1832.		Thermometer.		Barometer.	
Thursday	3	from 41	to 55	29·32	to 29·29
Friday	4	41	52	29·64	29·90
Saturday	5	30	57	30·02	Stat.
Sunday	6	46	68	29·99	29·96
Monday	7	50	77	29·76	29·80
Tuesday	8	43	65	29·86	29·98
Wednesday	9	43	53	30·02	30·18

Prevailing winds S.W. and N.W.

Except the 8th and 9th, generally cloudy; rain on the 3d and 5th. The sudden change in the temperature on the afternoon of the 6th, and storm of thunder and lightning on the morning of the 7th, together with the extreme warmth of that day, are deserving a particular remark.

Rain fallen, ·225 of an inch.

Charles Henry Adams.

NOTICE.

We regret that we are obliged to decline the insertion of Dr. Howison's Extracts from his Lectures on Materia Medica.

ERRATA.

In our leading article last number, page 166, *for* "weekly papers," *read* "daily papers;" same page, *for* "west of Europe," *read* "east of Europe;" and page 170, *for* "simultaneous," *read* "simultaneously."

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 19, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Part I.—Lecture XXXII.

Treatment of Fever continued.

In speaking, gentlemen, of the treatment of fever, I began by insisting upon the necessity of extreme cleanliness, washing the patient, frequently changing his linen, and accompanying this with free ventilation. If all this were not sufficient to remove any smell that existed, I mentioned the necessity of employing the chlorides, either sprinkled about the rooms, or by means of rags soaked in a solution, and that it was always well to put a solution of the chlorides at the bottom of a vessel before it was employed to receive the patient's evacuations.

I also mentioned the use of employing water for more purposes than mere cleanliness—for the purpose of reducing the temperature, and that we might employ it with a sponge, in order to wet the whole surface of the body, or that we might use it by pouring it over the body—in fact, that we might resort to ablution or affusion. I mentioned that ablution, or affusion, might be either cold, tepid, or warm. If the heat be steadily above 98 degrees, as ascertained by a thermometer placed under the tongue, or in the axilla—if the patient complain not of chilliness—if there be no profuse general sweating, and no internal inflammation, I stated that you may have recourse to cold affusion—that you may take the patient out of bed and pour cold water upon him, and be almost sure to do a great deal of good. This, however, ought not to be done if there be profuse perspiration—if the temperature be not steadily above 98 degrees—if the patient, though hot, complains of chilliness, or, perhaps, if there be pulmonary or other internal affection. I say perhaps, because it is quite certain that it would be improper in the case of profuse general sweating—in the case of the temperature not being above 98 degrees, and in the case of the patient being chilly. Any of these conditions ought to make us avoid it; but if the opposites of these things exist, I do not know that I should be deterred from cold affusion or ablution by the existence of an internal inflammation, when we cannot use cold affusion, I mentioned that we may always with safety have recourse to tepid ablution—that this may be employed almost at any period of the disease—that in some instances it will suffice, and that, so long as it is grateful, it may be persevered in. Should it feel uncomfortable to the patient, then we must employ the water warmer. The employment of these various measures, whether affusion or ablution, cold, tepid, or really warm, should bear a relation to the degree of morbid heat, and to what is grateful to the patient. If he feel uncomfortable from affusion or ablution, it ought not to be repeated till such uncomfortable feelings are gone, and then it ought to be employed at a higher temperature than that which was disagreeable to him.

I mentioned that, besides this, it was of great use, in general, to clear out the alimentary canal, by an emetic above and purgatives below; but that, if there were any tenderness of the epigastrium, it would be very wrong to employ an emetic even at the beginning of the disease. If, however, there be no tenderness of the epigastrium, you will find that an emetic exhibited early appears to lessen the subsequent symptoms that take place. I mentioned that it was also advisable to clear out the bowels well: that, if they be torpid, you must give a large dose of medicine; but, whether they be so or not, it is always right to clear them out thoroughly. By removing a quantity of depraved secretions—by removing a quantity of fæces

which are more or less of a vitiated character, you will certainly do a great extent of good. I stated that the best medicine for this purpose is a dose of calomel, followed by such a medicine as castor oil. You should exhibit five, ten, fifteen, or twenty grains, according to the torpidity of the bowels. Five grains generally answer well; but, if you have reason to suspect torpidity, you may give more. An injection is also proper. If the stomach or the intestines be irritable, we ought not to do more than give an injection. Accumulation and torpidity, however, do not always exist; for sometimes we have diarrhœa, and the whole of the abdomen is tender, and of course purgatives, under such a state, are likely to do more harm than good. The dose of calomel which you generally find it necessary to give early in the disease, and perhaps shortly after the emetic, you may repeat, of course, sometimes every day or every other day; but I shall presently have to mention that, in the case of fever, you find it an excellent practice to give a small dose of calomel, or some other form of mercury, at intervals, during the twenty-four hours. This may prove quite sufficient to keep the bowels open without any thing in addition; but, if two to five grains, exhibited every eight, six, or four hours, do not effect that object, then of course you must accelerate its action from time to time, either by an injection below, or by a moderate dose of castor oil. Senna and salts are given by some practitioners, but upon the whole I think castor oil is best. It is right before debility arises to have one, two, or three stools a day; but, if they take place without medicine, and be of a watery character, it is necessary to restrain them, lest they should sink the patient. As a general rule, where there is no irritation of the bowels, or much real debility, we ought to make a point of procuring two or three stools a day. There is no rule for the dose of calomel; but, if you give mercury in small and repeated doses, it generally keeps the bowels in an open state; sometimes, indeed, more open than you wish.

The use of purgatives in fever is very great, but undoubtedly it has been exaggerated by certain writers. You will find, if you consult some books, that you have only to turn the patient inside out in order to cure him; but I am quite certain that such is not the case. Within the last two or three years, there has been found a great disposition to diarrhœa, which it has been absolutely necessary to restrain; but no one can dispute the propriety of removing all filth once or twice a day from the alimentary canal. Costiveness ought never to be allowed; for it causes the tongue to be brown and dry; and, except in the last stage of fever, where there is frighful debility, you ought to have one stool in the twenty-four hours.

Venesection.—Now small doses of mercury, with the rest of the treatment already mentioned, will enable you in the greater number of instances to get rid of fever, such, at least, as I see; in other cases, however, it is necessary to be more active—it is necessary to take away blood, and of course you may do it either at the arm, or by what we call *local means.* Venesection is certainly not required for mere generally increased action. If no organ particularly suffer, if you cannot discover great excitement of any one organ, if there be mere general excitement of the system, I do not believe that venesection is required. However, if you know that the epidemic is of such a character that inflammation is sure to come on, it is as well then to bleed in the arm, to prevent such inflammation, when it does come on, from being as violent as it otherwise would. But, as a general rule in London, in the fevers that I see (it would be wrong for me to speak of what people see in other places, because fevers differ in different situations,) venesection is not demanded in one case out of thirty or forty. In the country, however, where people are strong and plethoric, and in hot countries where the excitement is sometimes exceedingly great — the congestion within the head, chest, or abdomen, very considerable—the lancet is the sheet anchor; whereas, in the majority of continued fevers which I see, certainly venesection is not necessary, and I am sure that those who employ it extensively in this disease, if they do not destroy their patients, yet they protract their cases.

However, you will find it of great importance to employ local bleeding, and, except in hot countries, I think every good may be obtained from it, in the majority of cases, without that shock which general bleeding produces, and which is very desirable in mere inflammation. In fever this is not demanded; and in comparatively mild fevers it would make the disease severe, and in those of an asthenic character it would knock the patient down.

Necessity of seeking for Local Inflammation.—It is necessary in every case of fever to be constantly on the look out for local inflammation—every day to ascertain what is the state of the affection of the head, of the affection of the chest, and of the affection of the abdomen. You should always ask if the patient complain of headache: you should look at his eyes, and see whether they be red or not; ascertain if his pulse be full, and inquire whether there is any throbbing of the head. So with respect to the chest: you should observe whether there is difficulty of breathing, and if there be, it is well to apply the stethoscope, and ascertain what rattling there is. The abdomen ought to be carefully felt every day, to see whether the stomach, intestines, liver,

peritoneum, or other parts, are inflamed. When we find a sufficient degree of inflammatory disturbance of these parts, then it is right to take away blood locally. If the head be affected, then you should cup at the back of that organ, or apply leeches to the forehead, temples, or behind the ears; but if it be the abdomen which is affected, then it is always better to employ leeches. If the pain be situated at the front of the head, it is better to employ leeches there. I mentioned, when speaking of inflammation, that the effect of leeches or cupping is often very local. I have seen parts relieved where they were applied, while others in the neighbourhood remained as painful as before. At whatever part of the head the pain is felt, there you should direct your means of treatment. If there be delirium, and this delirium be accompanied by pain and heat of the head, or throbbing, or if the eyes be red, or if there be great vivacity, like incipient delirium of an active kind, you ought always to shave the head, to apply a cold wash, or employ a bladder of ice, which is one of the best things, and put plenty of leeches on, or employ cupping at the occiput. If there be vomiting, or tenderness at the epigastrium without it, you should apply leeches there, which are the best remedy for it; because, when there is tenderness or vomiting, it generally arises from inflammation of the mucous membrane, and leeches will remove it, by removing the causes of inflammation. So with regard to the abdomen at large: when that is tender, there is generally more or less diarrhœa; the intestines are acting too violently, and leeches freely applied are the best mode of restraining it. After they have been applied, you may employ a blister; but you should always remember, that a blister will not take the place of local bleeding, if the inflammation be considerable. If there be local inflammation, you will frequently find that the application of leeches will remove it; and after it has greatly subsided, slight inflammation, mere irritation, may be left, and you may find a blister of the greatest use. We shall see, when I come to speak of diarrhœa, that leeches and blisters are frequently the best remedy for it.

After a proper detraction of blood, or in a case where the loss of blood is not advisable, blisters applied to the nape of the neck, to the forehead, and also behind the ears, are very useful. A blister applied to the summit of the head is generally a painful thing, and I would not have recourse to it but as a last measure. Before the inflammatory state has much subsided, and before there is really more of irritation than of inflammation, the application of blisters would of course be doubtful almost any where, but particularly at the top of the head; the hazard, however, would certainly be far less if they were applied to the nape of the neck, to the occiput, and to the forehead. I need not say that leeches must be repeated at intervals, as long as the local symptoms seem to demand them, on the one hand, and the strength of the patient will bear them, on the other.

Sinapisms.—When, however, notwithstanding local inflammation or irritation, you do not wish to apply even leeches or blisters, mustard poultices are particularly useful. When applied over the epigastrium, they will stop vomiting; they are very serviceable over the abdomen; and by many persons they are employed at last, or even in an early stage, to the feet, with the view of exciting distant irritation.

Mercury.—You will find, in many of these fevers, that it is of the greatest use, especially if there be a degree of inflammation, and great foulness of the tongue, to give mercury. If you give it in small, but repeated doses, it will answer the purposes that I formerly mentioned. It not only purges the patient, but by degrees it causes the tongue and the interior of the mouth to become moist, and when that is effected, the patient is almost sure to be better. It is necessary to remember that it should not take the place of local bleeding; that it will increase the good effect of the detraction of blood, and where that is not required, it will do alone; but where that is necessary, mercury must not take its place. It must be used as an auxiliary to, and not as a substitute for bleeding. I have over and over again in treating patients omitted the mercury, and I have seen the tongue grow darker and fouler, and then, upon having recourse to it again, I have seen the mouth resume its moisture, and the tongue become less foul. I have made the observation too often to doubt its accuracy. It is true there are many cases of fever that will do without mercury: I only say, that in the mass of cases where mercury is given, the success is greater than when it is omitted. We cannot draw any argument from a single case; it is only from a series of cases, treated in a particular way, that we can arrive at any legitimate deductions; and as far as I have seen, the moment the mouth becomes moist, provided proper detraction of blood be instituted, and all other suitable means, the local symptoms of inflammation generally decline, and patients recover more quickly than they otherwise would, and many recover who in all probability would not unless that practice were resorted to. Still, I must again repeat that a great number of cases of fever will do well without it; but where the symptoms are severe, you will find it a most useful medicine. You should not aim at any violent affection of the mouth; and if that do occur, the medicine should be omitted till the symptoms remit; but you should feel the patient's gums every day, to ascertain whether the

mercurial effect is kept up. There can be no doubt that mercury will take effect sometimes, not in consequence of having cured the disease, but by the disease becoming better through nature and the general means employed, and the mercury being no longer resisted. The proof of mercury doing good consists in this fact—that if you give it quickly to get the mouth sore, the sooner that object is effected, and, *cæteris paribus*, the sooner is the patient improved.

You will find, however, that calomel is frequently too active for the intestines, and blue pill, or hydrarg. c. creta, answers better; and after a time even these will purge, so that it is necessary to combine with them the exhibition of chalk mixture, or an infusion of catechu. It is well to give the infusion of catechu or kino, for it will enable the patient to bear the mercury; and you frequently find that hydrarg. c. creta is the only mercurial preparation that can be borne. Mercury, if pushed too far, increases the mischief; it induces great irritation of the alimentary canal. Every *good* remedy of course requires to be *properly* used.

Opium.—Small doses of opium, I need not say, will tend to check the diarrhœa, and frequently there can be no objection to five drops of the tinctura opii three or four times a day.

Antimony.—As to antimony, I do not think it an appropriate medicine, for it irritates the stomach, a circumstance which you do not want in fever; and there is so frequently a disposition to sickness, that it is very likely to excite vomiting. If you give mercury at the same time, the antimony has a great tendency to counteract its effect; it may cause the mercury to be rejected, and I confess that I have never seen material good done by it. In cases of decided inflammation it would be a good remedy, pushed to a large quantity; but when the case is attended with great irritation of the alimentary canal, as many attacks of fever are, and the patient has but little strength, I am sure that it is better not to give antimony, lest you make the patient's stomach so irritable that it will not bear food nor any thing else. It is only where there are very decided marks of inflammation that it might be given, and then I should infinitely more approve of mercury. Then, as to its sudorific effects, I have given the liq. antim. tartarizati, in very large doses, without producing sweating. The best mode to ensure sweating in fever is to clear out the bowels, to reduce the temperature of the surface, and to take away blood locally or generally, accordingly as it may be required. I have seen patients lying in a carpetted room, with the windows shut, having plenty of bed-clothes on, and perhaps a fire in the room into the bargain, and all this, I have been told, was to excite a gentle diaphoresis! The diaphoresis, however, never made its appearance; but by opening the windows, putting out the fire, removing the bed-clothes, taking up the carpets, purging the patient well, and removing any local inflammation that was present, it has come on immediately.

The idea of a few grains of antimonial powder, or a fraction of a grain of tartar emetic, once or twice a day, being important in fever, appears to me quite old-womanish. With the treatment I have mentioned, nothing else will be required, till great prostration of strength sets in.

Saline Draughts.—All cool drinks are of course proper, and a saline draught is as good as any thing which can be given for a drink. People will frequently take from half a pint to a pint a day. It is always to be remembered, however, that all acid matters, and even a saline draught, have a tendency to increase any irritation that may be present in the bowels, and diarrhœa is frequently kept up by a saline draught being continued. But if there be no purging, if the alimentary canal be tranquil, acidulated drinks are very useful. Upon the whole, however, a downright cool drink—plenty of cold water, is one of the best things that can be given.

Decline of Fever.—As the symptoms all decline, all the remedies must be diminished both as to force and to frequency, and there may be less abstemiousness practised. Nourishment may be gradually given, and one of the best articles, after slops are done with, is milk. It is very wrong to continue starving a patient after fever is gone. You will find when the fever is over, that the appetite becomes remarkably keen—keen in a degree that is never witnessed after any other acute disease. Patients, when the fever is over, are sure to say that they are very, *very* hungry. Other patients will ask you for full diet, but patients after fever ask from their very hearts; they seem to have a craving which impels them to ask you, whether they think you will be offended or not. You find patients more emaciated after fever than any other acute disease. When no evacuations have been practised by medical men, when the disease itself has not been attended by any great evacuations, you will still have a degree of emaciation which I think you will not witness after any other disease; and when the fever is over, and there is such a craving for food, it would be contrary to common sense to think it right to deny them. It seems to be a real call of nature; it seems that food is absolutely demanded. I always make a point of giving them light animal food, not pork and veal, and artificial trash, but plain mutton and good fresh beef, and watching its effects carefully.

Treatment of Fever attended by Debility.

Nourishment.—Suppose, however, that the disease does not run on in this mild form, or the inflammation become subdued as in any

inflammatory disease, but that signs of great debility come on; we must have recourse, even in the midst of fever, to good support. Milk should be given in as great abundance as the patient can take it, and, I think, strong beef-tea. Some imagine that, the digestive process being suspended in fever, animal broths cannot be digested; but, however that may be, I know that persons who take strong beef-tea—by which I mean a pound of meat chopped extremely small, and boiled in a quart of water until it is reduced to a pint—frequently do admirably well. Some persons will take two or three pints of it in a day—some require only one pint—and you find them considerably nourished by it. Many persons cannot take milk, but where it can be borne, it is excellent nourishment. I need not say that arrow-root, sago, and rice, may be taken with the milk. I believe that occasionally you will find that you must give more nourishment than you can get down by the mouth; and when there is such extreme prostration, it is useful to give strong clysters of beef-tea, in which an egg may be diffused. I have seen them given to a patient every four hours, apparently with the result of getting him through the disease. If the clysters be discharged again, and not retained sufficiently long to be beneficial, you will find it useful to put a drachm, or two drachms, of powdered catechu in each of the injections.

Wine.—However, more than all this is sometimes required, and we must give a patient wine. If you give wine as a general remedy for fever, I am certain that you will kill one-half your patients; but if you give it in the latter stages of the affection, or the disease be attended with great debility from the first, then you will frequently do much good by its exhibition. It is often indispensably necessary, but not (so far as I have seen) at the beginning of fever. A great number of cases that I see do well altogether without it, but I have seen cases, over and over again, where a glass or two of wine has stopped vomiting—has stopped a diarrhœa. It is where there is extreme debility—where there is irritation rather than inflammation, and you find that the pulse is feeble, almost fluttering, and by the look of the patient you see that he is sinking—that I have found it beneficial. I have always been accustomed to quote Sir John Pringle on the exhibition of wine, because I think that his directions, both as to the quantity required, and the time at which it is to be given, are the best which have been written. He says, "In our malignant fever," (he was physician to the army,) "when the pulse sunk, it always became very frequent;" (that is generally observed,) "and in proportion as it rose with the wine it turned slower. I have also had experience of the good effects of wine when the tongue has been both foul and dry." Here is an illustration of the propriety of not attending to one symptom, but to the whole. A dry and foul tongue frequently indicates inflammation; but if you find no inflammation present—on the contrary, great prostration of strength, with a fluttering pulse, an anxious countenance, and the patient unable to move himself, then you need not fear foulness and dryness of the tongue, but exhibit wine. "When wine is given," Sir John Pringle says, "in proportion as the patient grows stronger the pulse becomes slower." Wine will of course, in health, accelerate the pulse; but when a person is weak, and the pulse is quick in proportion to the weakness, and when this state does not arise from inflammation, but downright exhaustion, wine, instead of quickening the pulse, makes it slower. "The surest indication for wine," continues Sir J. Pringle, "is taken from the long continuance of the disease—the *languor* and dejection of strength—the slowness and faintness of the voice; but we can never be absolutely certain of its effects till we try them." This is also a point carefully to be attended to. You constantly meet with cases where you are in doubt as to the proper mode of treatment to adopt. This occurs to me every day, and will as long as I live. You are not certain whether the time has arrived at which you are to treat the case as inflammatory or not; and one is often made unhappy by this circumstance. Whenever a suspicion of this sort arises, it is best to combine both modes of treatment, to lessen any excitement, and to begin stimulating and tonic remedies with great caution. Whichever you find do good, you must increase, and, in fact, substitute it for the other plan. "I have seen," Sir John Pringle says, "in cases of this kind, strange instances of the power of instinct; for when wine was to do good, the sick swallowed it greedily, and asked for more—when it was to heat them, or raise the *delirium*, they either shewed an indifference or an aversion to it." It is of the greatest importance, in fever, to attend to the wishes of the patient, provided he be not delirious, for then he will talk at random; but if he be not delirious, or only partially so—if he be sufficiently collected to know his own feelings, and to give a clear account of those feelings, they should in general be attended to. "Sometimes (he says) the physician can have no better measure for the quantity requisite, than the appetite of his patient."

Half a pint of wine is sometimes required in twenty-four hours, one glass being given at a time; and generally a pint is the utmost that is requisite, though I have given a bottle with advantage. It is best not to give Rhenish wines, or thin claret, or any acidulated wine; for, if diarrhœa be present, it will increase the irritation of the alimentary canal.

It is also best not to give sweet wines, for they are apt to ferment and become acid in the stomach. Sherry, Madeira, and port, are the best that can be employed. If there be no irritation of the alimentary canal, but a torpid state of it, of course acidulous wines may sometimes, perhaps, be admissible. If the patient desire porter, or has been accustomed to it, you will find that very good; but you should not give a pint of porter at once: a wine-glass or two may be given every few hours, or perhaps only once in the twenty-four hours;—because a person desires porter, you are not necessarily to give a pint in the twenty-four hours. When there is mild delirium—not delirium ferox, but a muttering delirium—or when the pulse is rapid and weak, and when extreme debility has come on, then it is right to resort to this treatment, for it will frequently stop the delirium, or check it; but after it has stopped the delirium, if you go on with it, it will frequently bring the delirium back, and increase it, so that you have the patient worse than he was before.

Ammonia and Ether.—In this state of debility, many give ammonia and ether; but I cannot help thinking that wine, or good malt liquor, is the best article that can be exhibited, except the malt liquor bring on diarrhœa. It is much better to give those stimulants which most people like than to give ammonia and ether; they are natural stimulants, and they are much more grateful to the patient.

Care must be taken with all these things not to overload the stomach. The moment you find the head affected by them, or the stomach overloaded, it may be necessary to give an emetic. For the same reasons that I have already stated, I should also prefer giving wine to what are called *vegetable* stimuli—such as serpentaria. It is true I know nothing about them; I dare say they are good, but I have always been able to do without them.

Saline Treatment.—In this state of debility Dr. Stevens says that he has seen great benefit arise, far greater than from any thing else, from very small and repeated doses of the carbonate of soda, nitre, muriate of soda, or oxymuriate of potass, in remedying the deficiency of saline particles in the blood. He has a vast collection of testimony in favour of the saline treatment in yellow fever, where, after a certain period, ordinary treatment generally does more harm than good. In all such fevers, he contends that in the first instance it is the best practice to bleed the patient and give mercury; but after a certain period has arrived, the blood falls into the condition I formerly stated, and that condition is made worse by mercury, and, of course, by bleeding; but he says it is admirably remedied by small doses of neutral salts, particularly those I have mentioned, twenty or thirty grains every three or four hours, except of the oxymuriate of potass, of which about a third of this quantity is a dose. It appears that those who have adopted this plan have had the greatest success in the West Indies. He was telling me the other day, that, in yellow fever, many had agreed that they did their patients altogether more harm than good by treatment; that those, indeed, succeeded the best who gave nothing but saline draughts—saline draughts in large quantities. Neutral salts are now given in the large quantity I have stated, and with the very best effect, for they are solutions of a neutral salt. If any gentleman present have not seen the effect of neutral salts in reddening the blood out of the body, I will take an opportunity of shewing it in the course of the winter. By adding neutral salts to blood, however black it may be, you make it of the brightest scarlet; but if you add acids or alkalies to red blood, you make it black.

Quinine.—As to tonics, by far the best is quinine. If you judge it right to attempt supporting the patient by this means, it should be given in doses of three or four grains every three or four hours. I certainly fancy that I have saved patients in the last stage, when there has been no longer room for giving mercury, not only by supporting them well with good nourishment and wine, but by giving sulphate of quinine in considerable doses. But you must remember that in giving sulphate of quinine as a tonic, it may do harm by purging, and therefore you should be on your guard against this, by administering astringents at the same time. I frequently accompany it by catechu, and then the irritation is generally put a stop to.

Opium.—In this last stage some give opium in small doses, for the purpose of keeping up excitement. Dr. Wall, of Oxford, and Sir John Pringle, did so; but others give a dose once in the twenty-four hours, for the purpose of tranquillizing the system. I have no great experience of it as a stimulant; but I know that when a patient has long laboured under fever, it is quite safe to give him a dose of this kind, in order to procure him a good night's rest. I suspect that wine is a better stimulant than opium, although opium is certainly exceedingly useful in the latter stages of the complaint. Some highly praise musk. I need not say that morphia is sometimes greatly adulterated; it is said that opium is not always of a certain strength, and that morphia is; but from being adulterated, morphia is of the same uncertain strength as opium. Thus opium is particularly useful in procuring sleep, and in putting a stop to vomiting and purging. The muriate of morphia is an admirable, if not the best, form of opium.

The treatment of fever consists in subduing inflammation on the one hand, or sup-

porting the strength on the other; and you must always carefully look out for local symptoms, and check them.

The vomiting is frequently urgent, and very distressing to the patient; and although effervescing draughts will stop it, and hydrocyanic acid will stop it, yet if there be inflammation present, it is not in the nature of these remedies to stop it, and you must employ leeches, blisters, or sinapisms. So with regard to purging, opium will frequently put an end to it, and so will all astringents; but, as I have before said, it is best not to give astringents if there be inflammation present; leeches and blisters will then effectually subdue it, when nothing else will.

Yeast.—If the fæces be exceedingly fetid, you will find it very useful to give yeast, either by the mouth, or in the way of injection. An injection of yeast will frequently correct these very much, and some give yeast in porter. The injection of the chlorides would, I fancy, be useful, but I have no experience of it.

Necessity of attending to the state of the Bladder.—In fever it is always necessary every day to attend to the state of the bladder. It frequently happens in this disease that a great accumulation takes place in the bladder, and patients might go three or four days without making water. Great inconvenience may at last arise from this source, and therefore it is a point that should be carefully looked after every day.

There is only one other circumstance that it is necessary to attend to, and that is, to indulge the longings of the patient during the progress of the disease and afterwards, unless there be some very good reason against it. You see, therefore, that the treatment of fever is exactly like the symptoms. You saw that the symptoms of fever ran from those of the most violent inflammation—the most violent excitement of the system on the one hand, down to extreme prostration of strength on the other; and the treatment must vary in like manner. It may be requisite that the treatment should be of the most active anti-inflammatory kind on the one hand, or the most gentle anti-inflammatory treatment, with a moderate support of the system, on the other, or even of the most supporting and stimulating kind. Fever is not to be treated because it is fever, but according to the circumstances of each particular case. If you have twenty cases of fever, you are likely to find them more or less different from each other, and requiring more or less difference of treatment, according to the urgency of the symptoms. You will therefore remember that, in fever, you have to vary the treatment from that of an active inflammation to that which you employ in mortification, when the mortification is attended by inflammation.

MEDICAL VIRTUES OF CARBONIC ACID GAS.

To the Editor of the London Medical Gazette.

Sir,

Remarkable as are the properties of carbonic acid gas, I am not aware that it has ever been resorted to in this country as an external agent in the cure of disease. In various parts of Germany, however, where the gas exists naturally, and in a free state, I have seen it used in a great number of cases with such decided advantage, that I am anxious to call the attention of the profession to the very important effects which result from its judicious application.

The mineral waters of Germany and Hungary have enjoyed great celebrity. Many of them were famous in the time of the ancient Romans; and at Mohadia, in Banah Comitat, in Lower Hungary, on the frontiers of Turkey, there are still the remains of a very old Roman settlement, the favourite resort of the Emperor Adrian, who, it is said, used to repair thither to take the benefit of the waters. Justly as these two countries are esteemed for their mineral fountains, I might say that Hungary certainly (if not Germany), is equally so for its very abundant sources of carbonic acid gas; for to the inhabitants (and, shame to say, the country is little known to others), they are equally valuable, and are employed by them as much in the cure of disease. Carbonic acid gas, therefore, although little known here as an external remedy, is nothing new; the Germans, the Hungarians, and the Poles, have long been in the habit of employing it, and are well able to appreciate its value. They find that it may be applied to the surface of the body without risk or inconvenience, and that its effect may be modified according to the manner and extent of its application. They use it generally alone, as it issues from the earth; but it may be formed artificially, and applied by itself, heated or cold, diluted with common air or combined with other gases, or with steam. It acts as a general stimulant, and is found of essential service in those cases which require a tonic and invigorating practice—whenever it is an object to promote the

circulation of the blood on the surface, and excite perspiration; but it must be especially avoided when there is any active inflammation going on.

Having had opportunities of becoming personally acquainted with its effects, I should be to blame were I to conceal them, for they are beneficial chiefly in chronic complaints, and in a class of diseases which unfortunately occur too frequently in this country, and which depend on a condition which medical men are often obliged to see, but dare not name—viz. a scrofulous taint. I do not pretend that I have found a remedy for this condition, for it is a melancholy proof of the imperfect state of our art, that a strumous habit of body has ever prevailed against us, and is still likely to baffle all the skill we have; but it is some consolation to us to reflect, that if we cannot always overcome the evil, we may at least be able to diminish its force, deprive it of its terrors, and render its burden light. The rapid improvement to be observed in delicate females, and in persons of generally impaired constitutions, under the proper use of the carbonic acid gas, especially when assisted by attention to diet, air, and exercise, is incredible. This is particularly the case in chlorosis, amenorrhœa, hysteria, sterility, and a long catalogue of female complaints commonly called nervous, which, though sometimes depending on the imagination and affections of the mind, may often be referred to the condition of the sexual organs: in habitual constipation also, in dyspepsia, hypochondriasis, and even in melancholia, which are so common in derangements of the liver in persons who have led sedentary lives, indulged too freely in condiments and spirituous liquors, or who have resided in hot countries. The symptoms most complained of in all such cases depend chiefly on laxity of fibre, and want of tone; the secretions are vitiated, or in undue quantity, and there is a general deviation from the course of health, which constitutes disease of function rather than of structure. By the regular use of the carbonic acid gas bath, the general health improves—the patient no longer complains of listlessness, debility, or want of appetite—the pallid cheek resumes the glow of health—the countenance becomes enlivened, and alacrity is restored; in a word, wherever the object is to restore and keep up the balance of the circulation, and promote a healthy secretion, the remedy is of a most efficacious kind.

I may remark, that at the spots where nature has been so peculiarly bountiful of the gas in question, every facility is afforded for the treatment of disease; in the same neighbourhood there are generally chalybeate springs, various in degree as in kind, but containing for the most part salts of iron, saline matter, and fixed air. These waters are usually taken at the same time that the bath is employed, and contribute not a little to the restoration of health.

I was first led to the consideration of this subject from having witnessed the very great efficacy of the baths of Eger and Marienbad, in Bohemia, and of Sliatz, in Upper Hungary. I will state in few words the effects produced by the latter on my own person. The bath of Sliatz is contained within a square apartment, fourteen feet by eight; the water is four and a half feet deep, and its temperature about 90° of Fahrenheit. The gas is constantly bubbling up to the surface, and a person is placed just above the heads of the bathers with a flag, to prevent the gas accumulating. The sensation produced on entering the bath was agreeable; in less than five minutes my skin became of a florid red colour, and I perceived a tingling, or gentle prickling, over the whole exterior of the body; I perspired freely. In about half an hour my breathing became quick and hurried, the temporal arteries and carotids began to throb, and the circulation generally was so excited, that I thought it best to retire, lest I might pay too dearly for my curiosity. Having left the bath, my pulse soon became moderate, and my breathing natural. I had no headache or other unpleasant sensation, but felt in good spirits and refreshed; I was light and active, and my skin remained moist. The resident physician assured me he had seen the most beneficial results arise to delicate females, especially those of scrofulous habits; but that persons of inflammatory diathesis, or predisposed to irregular determinations of blood, would do well to use the bath with caution. I cannot dwell more on the subject in this place. The water contains a little iron, and a small portion of saline matters, but the gas is

pure carbonic acid. The following is the analysis according to Hähring:—

	Grains.
Sodæ Sulph.	3,500
Calcis Sulph.	6,500
Magnes. Sulph.	6,400
Calcis Muriat.	0,110
Sodæ Muriat.	0,200
Calcis Carbon.	0,600
Magnes Carbon.	1,080
Oxyd. Ferri	0,200
Silica	0,300
	18,890

In 100 measures of the water there are 56 of carbonic acid.

At Eger, in Bohemia, the gas rises out of the earth in a dry state; and is, both there and at Marienbad, in the neighbourhood, applied to the body generally or partially, alone or combined with steam, &c., according to circumstances; the patient being placed on a stool in a square chest, so arranged that the head may be effectually excluded (as may be seen at Mr. Green's steam-bath establishment, in Great Marlborough-Street.) The gas rushing in displaces the atmospheric air, and the naked body of the patient is exposed to the influence of carbonic acid gas. Dr. Heidler, who resided at Marienbad many years (and, I believe, is there still), says, "No other species of bath produces such sensible effects on the patients; it occasions *heat* and *excitement of the whole body*, but especially of the *belly*. It often begins to be felt at the extremities, and mounts gradually to the superior parts; it is attended with a sensation of tingling or prickling, like the crawling of ants on the skin; and sometimes the excitement is so great as to produce pain, especially in certain cases of gout, rheumatism, recent wounds, or old scars. On one occasion, a patient labouring under sciatica was obliged to leave the bath twice, on account of the pain. Such is, however, rare; and when there is no inflammation, the most violent gouty pains are immediately relieved. Another effect is, *to produce perspiration*. The action of the gas (thus indirectly applied) on the organs of respiration, is decidedly *irritating*. At Marienbad there is a small portion of sulphuretted hydrogen mixed with the carbonic acid; but at Cudova, in Silesia, and at Eger, the gas is pure carbonic acid. The effect of the gas at Marienbad on the human body, both during health and disease, is *to excite the vascular and nervous systems*. All the phenomena which occur during its use confirm this fact; and it is thus that its application is contra-indicated under inflammatory disease." Here, then, is the evidence of one who has been in the habit of applying this remedy in the practice of years. Professor Osann, of Berlin, in his valuable work on the waters of Germany, says, "The most remarkable effect of this kind of bath is observed in spasmodic diseases, in atonic cases, and especially in those in which the secretions and excretions are vitiated. The general effect of the gas is exciting, stimulating, and strengthening; a strong prickling, darting sensation, is produced on the skin, with increased heat, terminating in profuse perspiration. If it come in contact with the more sensitive organs, as the eyes, it irritates and inflames; and applied to foul, ill-disposed ulcers, it acts as an antiseptic, and improves their condition."

The professor believes that the gas acts, not only externally, but also by absorption; and he considers the stimulating effect increased if the gas be mixed with the vapour of water.

Now, sir, to come to the application of these remarks, which I have made as concise as possible, I would suggest that this remedy be had recourse to in this country in certain chronic cases, where a general stimulus is required. It is too valuable an agent to be lost sight of; its effects are so decided, and so important, that I am sure the profession only require to be reminded of them to be induced to give them a trial. We have not the gas in a natural state, as it Eger, it is true, but it may be formed artificially, and will be found to answer the same purpose. After what I have already said, and the evidence I have brought forward, I should be trifling with the good sense of the reader were I to make much further comment. I have translated the opinions of Drs. Heidler and Osann from their German works, as near as may be, in their own words. I intended to make this the subject of a pamphlet, and may hereafter endeavour to point out the effects of carbonic acid on the different states and conditions in which it has been tried. In the meantime, however, the country still labouring under so much excitement about cholera,

I have thought it better to trouble you with a paper on the subject.

I shall conclude these remarks by proposing that medical men take the subject of this paper into consideration, as I feel confident they will find themselves in possession of a valuable remedy in very many cases, though I dare say it may be misapplied or abused, like every thing else. It is attended with no expense, risk, or inconvenience—little apparatus is necessary. The gas may be applied partially or generally. Sometimes it is *poured upon* wounds, scrofulous joints, ill-conditioned sores, &c. It may be injected into the vagina, or the patient may squeeze it out of a bladder upon the external parts. Boxes are made so as to enclose an arm, a hand, or a foot, and the gas applied in that way; or the whole body may be exposed to its influence. The most convenient manner of doing it is, to let the patient sit naked in a chest, as stated above; the doors and lid are closed, taking care to have the head effectually excluded, a bag closed round the neck, and the room well ventilated. The neck of a glass retort may then be introduced into a hole at the upper part, and the gas formed at pleasure by any strong acid and pieces of marble or chalk—perhaps sulphuric acid is the best; and it is desirable to dilute it with an equal part, or a part and a half, of water. Portions of chalk may be added from time to time, at the stopper of the retort, and the mixture should be occasionally stirred or agitated, as the action of the ingredients is apt to be impeded by the formation of an insoluble crust of sulphate of lime; whereas, it should be briskly kept up. The gas will find its way to the bottom, displacing the common air, and the chest will soon fill. It would be well to have a moderate hole at the upper part of the chest's side, to facilitate the escape of the atmospheric air, and also to enable us to ascertain when the chest is fairly filled with carbonic acid, which may be done by introducing a lighted taper. The hole may then be closed, and the temperature raised by the vapour of spirit of wine, as in the application of the hot-air bath, now in common use. We have only to introduce *at the lower part* two perforated good sized tubes, one on each side; communicating, however, with a common chimney without, in which the spirit is burned. In the same way we might apply steam, in combination with carbonic acid, by attaching a tube to a steam-kettle, (or in a case of emergency, to the spout of a tea-kettle) and connecting it with the apparatus; and so we might go on, varying the process according to circumstances, having recourse to sulphur, mercury, or any thing else. In those cases in which the erect posture is inadmissible, the carbonic acid might be applied to the patient in bed, by employing a simple wooden frame, such as is placed over the body when the hot air-bath is used, *only so constructed as to take to pieces**; by which all risk of the effect of perspiration, &c. being counteracted by the cold air rushing in on the removal of the frame, is prevented; the frame is to be covered, also, with the blankets, just as under ordinary circumstances. In order to introduce the gas, it is best to be provided with a long flexible leathern tube, or a curved tin one, bent at right angles for about two inches at one end, which may be conveniently rested on one of the bars of the frame about the middle of the bed, *taking care to have the bed-clothes well secured*, so as not to subject the patient to any inconvenience by the escape of the gas: the neck of the retort may then be applied to the other end. This is all the apparatus required, and is nothing more than that of the common hot air-bath made subservient to a double purpose; but in hospitals and public institutions, where they have a bedstead so constructed that the heated air is admitted underneath the mattress, (which consists of merely cross open bars) it will be still more easy to introduce the carbonic acid, as the tube may be placed *under the mattress*, instead of above the patient, and then there will be no risk of disturbing the bedding. In private families, however, where the other apparatus is to be found, it will do extremely well; and by it, also, we may

* The upper part of the frame should not be made of hoop, as is commonly the case, but a semilunar piece should be cut out of a solid bit of wood for the neck, the head being entirely excluded. No intermediate hoops are necessary; but simply these laths or rods, lodged in grooves made in the two end-pieces, so that they may be removed one by one *without disturbing the bed-clothes*. The apparatus may be seen at Mr. Dennis's Warehouse, No. 11, Queen's-Buildings, Knightsbridge, where I have been in the habit of having it made.

raise the temperature as high as we please. It is only necessary to burn spirit of wine, in a dish or lamp, at the foot of the bed, as already mentioned; but for the better distribution of the heat, it is convenient to attach to the interior, at the end of the chimney, the point at which the heat enters, a perforated branch tube, one and a half inch in diameter, one arm going to each side, and reaching as high as the patient's hips.

The presence of a medical man, although not absolutely necessary, is desirable, and the patient should not be left; the head should be well excluded, the bed-clothes held close to the neck, and every precaution taken to prevent the carbonic acid from being breathed. I trust I may be understood I have treated the subject very generally and superficially, in order not to take advantage of your kind indulgence. You will readily excuse the length and imperfect state of these remarks should you hear that you have been the instrument of good to the afflicted, or that you have assisted in rescuing a fellow-creature from the grave.

I have the honour to be, sir,
Your obedient servant,
WILLIAM HOLT YATES, M.D.

Sloane-Street, Knightsbridge,
May 21st, 1832.

ESSAYS ON HYGEIA;

OR THE

Art of preserving Public and Private Health.

BY

JOSEPH ROGERSON, Esq. of Wigan; and
GEORGE ROGERSON, Esq. of Liverpool;
Surgeons.

ESSAY I.

Introduction.—Hygeia of the Atmosphere.

HYGEIA, (the *Hygiène* of French writers) by which we understand the art of preserving health, of preventing those diseases that living beings are disposed to, and of perfecting the living machine, or any of its individual parts,—teaches us a knowledge of the influence of all those agents which nature has destined or art made to act upon the organs of living bodies. The domain of hygeia is very extensive and important, interesting, alike, man individually, for the preservation and maintenance of his own health, comfort, and happiness; and the statesman or individuals holding public situations, a part of whose duty it is to ascertain the best means of securing those advantages which conduce to the salutary benefit of all, and of preventing or destroying every thing noxious, injurious, or destructive to public health, or to the salubrity of public places. In short, by hygeia we learn how to avoid what is hurtful to the body, and to use what is beneficial. The science of hygeia, then, should not be confined to the members solely of the medical profession, but its knowledge should be diffused among the mass of mankind; and, though opposed to popular treatises on medicine, as for the most part productive of evil rather than of good, we should consider a well-written treatise on the science of hygeia a boon conferred upon humanity.

Hygeia is public and private; the former treats of the health of the community at large, and the latter considers that of individuals in particular. The one relates to men as their bodies and organs are affected by things or agents in common, and the other to man personally, and his domestic arrangements. The objects of private hygeia, then, would chiefly consist in treating of the healthy qualities of food, drink, and clothing, as adapted for individuals; of apartments or private houses; of modes of living or habits; the arts and processes of cooking, baking, brewing, &c.

Public hygeia is civil, military, or naval; but the first of these divisions will more particularly at present engage our attention. Civil hygeia embraces an examination of the salutary construction and continued salubrity of towns, streets, highways, roads, houses, and public buildings of every description, as charitable establishments, markets, theatres, prisons, churches, &c.; the proper and healthy supply, state, and kind of nutriment and food, and the places in which they are prepared, as public bakeries, butcheries, fisheries, &c.; the clean condition of public sewers, fosses, and all places containing decayed vegetable or animal matter, and any kind of noxious effluvia; the state (and, where necessary, the removal) of burial

grounds, all putrefactions, all sources of infection, injurious manufactories—in short, every thing tending to injure the public health, or endanger the well-being of a town, or any portion of it. We should even consider what are called the bills of mortality and births as proper subjects of investigation in the science of civil hygeia.

Such are the subjects which obviously are of vital importance to every state, and ought to form not only part of its medicine, but of its political economy and political institutions. The apathy, however, which has been displayed with regard to them in this country, is very remarkable. Public situations, even of a medical nature, have been given away to others than medical men, and have not been considered by the people of England as strictly belonging to the province of the profession. But if the conduct of government were guided by dictates of wise legislation, the public health would be regarded as an object of paramount importance, and every possible measure would be adopted for obtaining and securing it. Councils of hygeia would be appointed, and officers be found in every town, whose reports, observations, and experience, being published, would advance the knowledge of hygeia, and reduce it to a regular system. By these means the health of large communities would be secured and improved, whilst corporations or other public bodies would be taught scientific principles on which they might with certainty act in the establishment of public improvements. In England, the study of this science has been much neglected, and to some it is not known even by name, simply because it has not been made the interest of any particular class of men to devote themselves to its pursuits.

Of all the agents of civil hygeia, the *atmosphere* is one which attracts in a most especial manner the attention of the hygienic inquirer, and exerts a more extended influence over mankind than any other common agent. The hygeia of the air will, therefore, with the greatest propriety, form the subject of this first essay; but it is better to defer entering into a copious examination of its nature, properties, and salubrity, until the causes and sources of its pollution have been considered, mentioning only in a general manner what may be necessary to the clear elucidation of the subject.

The lungs are the principal organs of which atmospheric air is the natural excitant, and its effect is to change the black or venous blood into red or arterial, so as to make it fit for all the purposes of life. The hygeia of air, then, consists in its capability and fitness to produce those changes on the blood in the lungs, preserving in a proper state the structures exposed to its agency; so that whatever airs, gases, vapours, or floating particles of matter, exert a contrary effect, or do not produce these physiological changes, are improper for respiration, and injurious to the blood, respiratory organs, and health. This we should lay down as an established maxim or law of the hygeia of the respiratory organs. The lungs are composed of three tissues,—the mucous, or internal; the cellular, or middle; and the serous, or outer; but it is the structure of the mucous membrane which suffers primarily and mostly from noxious, injurious, or improper gases, or mixtures of them. The influence or agency of these improper gases is not, however, confined solely to that portion of the mucous membrane lining the lungs, but is felt in any part of its tract throughout the whole of the respiratory organs and their outlets. The mucous membrane of the nose, that of the frontal sinuses, that of the eye or conjunctiva, and, we have even known, that of the ear to be affected; but that of the windpipe, and particularly of its higher portion, and epiglottis, are very sensible to the impressions of improper gases. If the gas be very powerful, it will seldom penetrate into the lungs until after repeated attempts, for the irritable epiglottis resists its passage, contracts spasmodically, and excites all the parts connected in respiration to join in stopping its ingress. It is thus that arise pain, or tickling, coughing, and interrupted or short breathing, or catches of the breath, exciting a sensation of what the common people forcibly express by "taking away the breath." After the epiglottis, however, has become accustomed to the stimulus of improper air, even the most noxious gases will be distributed over the air cells of the lungs, and there produce all their injurious effects. Generally speaking, these kinds of concentrated gases create greater mischief by

their action on the blood, either by preventing that fluid being exposed to proper air, or by their irrespirable properties, than by any effect on the mucous membrane itself. They, however, injure both. When the irrespirable gases are mixed with various proportions of common air, they then become less and less injurious in proportion to the quantity of gas in mixture; but, unless very much diluted, they still are injurious to health, and lay the foundation of diseases, by producing chronic affections of some part of the mucous membrane of the respiratory apparatus. In protracted cases of exposure, or with the inhalation of some gases, the mucous membrane lining the stomach and alimentary canal will suffer; inflammation of its structure will be brought on, and diarrhœa frequently result. Custom or habit, however, diminishes the degree of deleterious effects of many improper gases upon the respiratory organs of some individuals, who habitually inhale them,—like the criminal mentioned by Sanctorius, who felt sick on being removed from an impure dungeon, and did not recover until he was restored to the place whose impure atmosphere he had long breathed. The less powerful gases and vapours are breathed with scarcely any perceptible inconvenience, but they are insidious enemies to public health, and become the fruitful sources of pulmonary and tracheal chronic inflammations, and their ultimately fatal consequences.

The effects of the atmosphere are manifested on the respiratory organs, both by its chemical and physical qualities, which will be only alluded to here briefly, for the sake of better understanding the effects of a vitiated condition of the air. Analytic chemistry has demonstrated, that the air is not a simple body, but is composed of two principal gases, azote and oxygen. In a certain mixture of these two gases, living beings are destined to breathe, and their lungs are made expressly for its agency; but, separately, these gases are either more or less injurious to respiration and health. Azote will not of itself, under any form or condition, support life or combustion, but will speedily destroy both. Oxygen is always respirable, and only proves injurious and fatal from being too powerful and concentrated a stimulus; but animals immersed in it will live a considerable time longer than they would in the same quantity of common atmospheric air. Their respiration becomes hurried and laboured, and they die before the whole of the oxygen is consumed, though another animal of the same or any other species will breathe and live for some certain time in this residuary air. It is on this constituent part of the atmosphere that the aerial laboratory of life, the lungs, act by taking the oxygen, and substituting the same proportion of carbonic acid gas and vapour. The chemical proportion of these gases, as constituting common air, is found to be 79 of azote and 21 of oxygen in 100 of atmosphere, and so rigidly is this proportion observed by nature, that it exists every where and always, throughout the whole mass of air which has hitherto been examined. It is preserved the same in one country as in another—on water as on dry land—on the sea as on the shore—in low plains as in lofty places. On the top of Mont Blanc, the air observed these proportions; La Perouse supplies us with similar testimony; and Gay Lussac, the celebrated French chemist, in his aerial voyage, analysed air collected 7250 yards above the level of the sea, and found it to be the same. This uniform proportion of the elements of the air constitutes an atmosphere salutary, respirable, and necessary for the support of life; shews decidedly its hygienic proportion, and that this should be invariably preserved, for we are born to breathe it. But one part of it, oxygen, may be increased in some cases, to some little extent, with advantage; the other azote never can be augmented with impunity. Individuals or bodies of men predisposed to pulmonary and heart diseases, or those who labour under diseases affecting the lungs—as thickening of the pulmonary mucous membrane, or hypertrophy of the heart—will derive advantage from air more strongly oxygenized; but it must be in an atmosphere created artificially, for it will not be discovered in nature's air, let it be moist or dry, warm or cold, high or low.

Besides these two chemical constituents, there prevails very generally, amidst the aerial mass, carbonic acid gas and the vapour of water; but they do not necessarily form an integral part of our atmosphere, for La Perouse could not detect the slightest trace of carbonic acid gas in the air which he analyzed. Even where it does abound, it exists in

a very minute proportion, and is very much diluted, so as to be harmless; but in greater quantities it is an improper agent for the lungs, is injurious to public health, and, in its concentrated state, is destructive of life. When the moisture of the atmosphere is excessive, it distresses the lungs and acts unfavourably on the body, through the medium of the skin.

Such is the chemical hygeia of the air, and whatever disturbs or destroys it must be more or less an improper, unphysiological, or unnatural agent. But let us rapidly pass on to its physical hygeia, which comprehends its fluidity, weight, elasticity, calorie, temperature, light, electricity, and motion—as breezes, winds, &c.

The weight of the atmosphere has considerable effect on our health, well-being, and comfort; but this property of air acts more directly and potently on the body through the medium of the skin than through that of the lungs. Its pressure is, however, influential on the lungs, as well as on every other part of living mechanism. The pressure of the air is sufficient to support, at the level of the sea, a column of water thirty-two feet high, or a column of mercury of twenty-nine inches; and it is when supported near this degree of atmospheric pressure, that a healthy man best exists and breathes. The exact specific gravity of air, compared with that of distilled water, is a nice problem, and is not as yet undeniably ascertained; but since the pressure of the atmosphere upon any given surface is equal to a column of water of thirty-two feet, or twenty-nine inches of mercury, it will follow that its weight bearing upon a moderate sized man will be about 28,000 lbs. or 33,320 lbs. The weight, however, varies according to circumstances: it gradually decreases as we ascend above the level of the sea, and increases as we descend below it; calorie decreases as well its absolute weight pressing on us, as its specific gravity and density, and vapours also decrease it.

This changeable variety in degrees of atmospheric pressure is far from being a matter of indifference to public health and personal comfort. When the air is pressing on us too lightly, we experience an universal uneasiness and lassitude; have our respiration affected, breathing more frequent and laborious, and the circulation is accelerated. This happens generally on those days when the air is vulgarly supposed to be heavy and dull, but when it is in reality lighter, though our feelings are heavy and dull. In higher regions of the atmosphere, as on the top of very elevated mountains, it is also light, producing the same sensations; and Gay Lussac thus experienced it in his aerial voyage. On days when the air is heavy, or, in other words, when the weather is popularly said to be light, a contrary effect is produced, and we feel elevated and happy. The extremes of light or heavy pressure are very injurious; in too light pressures the balance of the circulation would be destroyed, bleedings would occur, and respiration would be too quick to be long maintained. The various classes of animals will with impunity endure different degrees of atmospheric pressure, according to the constructions of their bodies and the mechanism of their respiratory organs. Fishes and birds will thus bear it better than man, but vegetables are sooner injured than animals, and will die under a pressure where animals would breathe and live. The pressure, then, of the atmosphere is an important hygienic agent, and, in the salubrious selection of towns, should always be attended to: in the selection of a residence for individuals predisposed to chest diseases, it is of primary importance.

MR. ORTON ON CONTAGION.

To the Editor of the London Medical Gazette.

Sir,

There is a mode of propagation of the epidemic cholera which seems to have been overlooked by authors, but it appears to me highly probable that it is the means by which it is chiefly disseminated, namely, the inhalation of the effluvia arising from the evacuations. The intestinal discharges of cholera are usually much less offensive than common fæces, but they have been observed to possess a peculiar faint odour, making a strong impression on the olfactory organs, and "hanging long about the nostrils." Mr. Jameson (an anti-contagionist) states, that on the ex-

amination of a body after death from cholera, in Bengal, the three medical officers present at it were all sensible of a peculiar smell, and all were, for a day or two afterwards, affected with vomiting, diarrhœa, and other symptoms of disorder of the digestive organs. Mr. Chapman states, in the Madras Reports, that on visiting a solitary case of cholera in his corps, he felt a slight nausea, apparently arising from inhaling the effluvia of the evacuations; that he remained four hours with the patient, and next morning suffered a violent attack of cholera. Here, then, is evidence of the power of these effluvia to produce the disease, and hence may be derived very sufficient answers to some of the principal arguments of the anti-contagionists.

It is now well known that the fully developed form of cholera is usually preceded, at least in Europe, by mere diarrhœa, often of many days' continuance, and in many attended with various slight affections more or less approaching to cholera. It is equally certain that, in a large proportion of every population, the disease proceeds thus far and no farther; being either arrested by the efforts of nature or art in this first or premonitory stage, or, as is most probable in the generality of cases, the system being insusceptible of the full effect of the morbific cause. They are, however, part and parcel of the disease, and it may be fairly presumed that the body, under their influence, is capable of communicating it to others, as well as under its more perfect forms. We have, therefore, during the prevalence of the epidemic at any place, and probably before decided cases of it have appeared or have been declared, numbers of persons going about unsuspected among us, scattering the seeds of the disease by their breath, by their perspiration, but chiefly by their evacuations, incessantly renewed in situations where the effluvia arising from them are inhaled in a concentrated form, for a considerable time, by numerous persons in health, who never come into contact with the affected.

The circumstances of the recent outbreak of the epidemic in Paris, which have been considered so fatal to the doctrine of contagion, (or at least the universal origin of the disease from that cause, variously *assisted* by others,) are thus rendered perfectly compatible with it. The probability is, that during the six weeks that the cholera prevailed in London before it reached Paris, many persons passed from the former to the latter capital affected with the choleroid diarrhœa; but let us suppose only one, near the end of March, making the journey. He sleeps, of course, at Calais, infects the atmosphere of at least one public privy there, and perhaps transgresses the prohibition stuck up on the Boulevards, "ni faire ni deposer ses ordures,"—for necessity has no law,—and passes on. The disease immediately afterwards appears there. The same individual visits various public cabinets in different parts of Paris, and very likely, among others, that pretty little pavilion which forms so conspicuous an object in the square of the Palais Royal, and reflects so much credit on the good taste of our neighbours; possibly he even leaves at night a dose of the poison in the Rue St. Honoré, or some other equally frequented place, where it is spread about by the broom of the scavenger in the morning. Thus the morbific exhalations arising from a single person are taken into the lungs—into the very blood of thousands of others, in the course of twenty-four hours. In a few the requisite susceptibility exists, and the disease immediately appears in its confirmed shape in various parts of the city, yet not one can tell where he received the infection. "De non apparentibus et non existentibus eadem est ratio," therefore the disease is not contagious!—The very rapid diffusion of the cholera over Paris has probably arisen in a great measure from the practice of using public privies, and, among the lower orders, of obeying the calls of nature in the open air.

It is said that the disease cannot depend upon contagion, because it has pervaded entire camps, and has committed most extensive ravages in bodies of troops in India, in the course of two or three days from its first appearance in them; but, by means of the common privies, every man of a corps may be exposed to a full dose of the infection arising from a single individual in a day or two. Thus, likewise, we may explain the fact, that the hospital attendants in India have not in general suffered more than other persons. This observation has arisen chiefly from experience among the military, where that mode of propagation always exists in a high degree, besides great and constant personal intercourse; but even among the

civil population it is scarcely less evident, for nearly the whole of them perform their evacuations *sub dio*, assembling in great numbers in particular places, where they sit for a considerable time, often luxuriously smoking their hookas during the process. Where such modes of universal exposure exist, it must be in vain to attempt generally to trace the conveyance of the contagion from person to person. It appears to me that it may be traced, on the larger scale of its passage from place to place, ever since the year 1817, in as satisfactory a manner as the experience of other contagions, or common sense, would authorize us to expect.

Various instances are recorded where the disease has appeared to be conveyed short distances by the wind, as, for example, to vessels sailing up the Hoogly, before they had had any communication with the shore; which may well be believed when it blows from the filthy purlieus of a populous town, particularly about the banks of a river or the sea, which in India are great places of resort for the above-mentioned purposes. In like manner we may account for the suddenly extensive appearance of the epidemic in Gateshead. The wind changed on the 25th December, (as is proved by Dr. Craigie,) precisely coincident with the beginning of that great outbreak, and crossed the river loaded with contagion from every jakes and dead-wall about the northern bank, where the disease had been prevailing for some time before; and great susceptibility existing from starvation, drunkenness, and filth, the necessary effect was such as we have seen. And thus the atmosphere of a dirty street may be so infected as to render it dangerous to pass through it. The wife of a medical gentleman residing at Kew came to London, and walked through St. Giles's when the epidemic was prevailing there. In the course of the following night (after returning home) she was attacked with it, and died; and immediately afterwards her mother, residing with her, was also attacked and carried off. These facts are both strong and well attested, for I had them from the gentleman's own mouth.

One of the clearest series of facts in proof of contagion on record is that detailed in the Medical Gazette of the 5th May, regarding the introduction of the epidemic into Hawick. A traveller from Newcastle, during the prevalence of the disease there, is attacked and dies at an inn at Morpeth, where the disease is not prevailing. Another person, after staying a day at that inn, during the illness of the former, returns to Hawick, is immediately afterwards attacked and dies, and numerous other cases arise there after direct intercourse, in a continued chain, which it would be repugnant to common sense to attribute to any cause but contagion. But there is a slight break in the evidence, which might afford a handle to sceptics. It appears that the traveller from Hawick never saw the cholera patient at the inn at Morpeth, though he staid under the same roof with him, (see the Cholera Gazette, April 7.) It is, however, highly probable that he visited the privy where the cholera evacuations were deposited, either by the patient himself or his attendants. And thus, probably, an infinite number of other and greater hiatus in the contagionist evidence, have occurred.

It must be allowed that the argument insisted on by Dr. Johnson, Dr. Gilkrest, and others, of the occurrence of numerous cases of cholera, in various parts of England and Scotland, last summer and autumn, some of them similar in character to the severer forms of the epidemic, where no suspicion of imported contagion could be entertained, presents a difficulty to the theory of the universal origin of the malignant disease from that cause. But the evidence of absolute identity of any of these cases with the epidemic, and of difference from common cholera, is incomplete, and they exhibit strong features of dissimilarity to the epidemic in their infinitely less fatality and limited number, the disease shewing no disposition to spread generally throughout the community as the malignant cholera so constantly does, on its first appearance at a place. If all the causes of the epidemic existed so generally in England and Scotland at those times, and have since only gone on increasing in intensity until they produced the effects we have seen, the line of demarcation between these precursory cases and the epidemic, should have been infinitely less distinct than it is. The disease should have appeared as generally over the country as did those cases, and in such a gradual, undefined manner, that it would have been impossible to say when it arose at any place, or where it existed at any time, and where it did not. Instead of which,

what are the facts? It began at a point, the very place where it was most expected; it has been confined to partial continuous tracts of country; its origin at each place, not only in Britain, but wherever it has been, has usually been most distinctly marked; or if from concealment, or other causes, its existence has been at first doubtful, in a very few days that doubt has always been dispelled. The great prevalence of common cholera which preceded the epidemic, argues the existence of a state of the atmosphere favourable to the spreading of the latter disease, but by no means an occult or peculiar state, of itself capable of producing it. I have endeavoured to shew that this state consists essentially in a disturbance of the electricity of the atmosphere, and that the injurious effects are owing to the respiration of air reduced to the negatively electric state, either by changes in the atmosphere itself, or by the putrefaction of substances on the earth, the latter case constituting malaria; and I still trust that these inferences will some time be fully brought to the tests of observation and experiment. On the peninsula of India, in like manner, the epidemic was preceded by the extraordinary prevalence of common cholera, the returns of the Madras army shewing nearly double the usual number of cases of that disease in 1817*, when the epidemic was prevailing in Bengal, but before it reached any part of that army. But in the following year, the gradual march of the malignant disease over the peninsula, and its arrival at each successive station, were almost as distinctly marked as those of an invading army could have been.

It is most probable that the question of contagion in cholera will remain for posterity to decide on it. At present there is too much party spirit and prejudice, and adherence to opinions perhaps too hastily declared, to permit either party to submit to evidence. It is to be feared that there will be no lack of farther experience for its elucidation; but what facts can ever appear, sufficiently strong and constant to come up to the full measure of proof required by some of the leading controversialists? For example, Dr. J. Johnson adduces the instance of the second case in one of the Borough hospitals, occurring in a nurse who had returned to her duty in the ward where the first had died, thirty-six hours after his death; and observes, that this would have been held up as a case of contagion if the nurse had been present during the first patient's life. But is it not abundantly probable that sources of infection remained after that short lapse of time about the bedding, clothing, bed-pans, privies, &c.? Dr. Gilkrest, a most uncompromising anti-contagionist, maintains that there is only one fact fully ascertained regarding the cholera—that it is not contagious. And Dr. Craigie, though he admits that the Vicar of Newburn had been employed in his duty among the numerous sick there for several days before his attack, yet "ventures to assert, without fear of refutation, that the intercourse with the sick in that case went for nothing." It is no wonder that we so often hear the assertion from that party, that they are unable to find any proofs of contagion in cholera, when they seem to look for stronger proofs than most of the acknowledged contagious diseases afford.

If we would still reject the obvious and almost perfect explanation of the phenomena which contagion, as the primary and sole essential cause, affords, supported as it is by a vast mass of evidence that the disease actually exhibits that property, what other have we to replace it but forced hypotheses, without a single constant fact, or a fact at all, to give them countenance—such as the emanation of a morbific matter—a new principle in nature—from every variety of soil or substance of which the earth is composed, and even from the ocean itself, yet perfectly identical under all circumstances, and arising independent of any cause or law which the united inquiry of mankind for so many years has been able to detect?

If, however, the universal origin of the disease from contagion should be fully proved, the question of the utility or propriety of quarantine, and other precautionary means against contagion, would still remain. It appears pretty evidently that the strict measures of separation, cleanliness, &c. practised in Edinburgh, have *retarded* the progress of the disease there; but it is probable that they will only have had the effect of extending its prevalence through a longer period of time than usual, with-

* The number, however, was still small for a tropical climate, being 280 in 80,000 men.

out lessening its amount. Experience seems to prove that the virus of cholera is of a highly diffusible nature, and I have just endeavoured to shew, that it has extraordinary facilities for universal dispersion. Moreover, like those of plague and small-pox, it appears to lie dormant for considerable periods of time, under circumstances unfavourable to its propagation, and to reappear when these circumstances are reversed. There is therefore too much reason to fear, that all measures of separation and insulation will prove finally inefficient—that any practicable measures of that kind will never prevent all the susceptible persons in a large city, for example, being sooner or later exposed to the infection, and either carried off, or acquiring that insusceptibility which so generally follows an attack of the disease.

The last-mentioned fact, like almost every thing else regarding this disorder, is disputed; but a fresh instance in proof of it is stated as having occurred at Paris, where many Poles were living during the epidemic, but entirely exempt from it, having been in their native country last summer when it was ravaged by the disease. An instance recorded at page 133 of the Bengal Report, which has often been adduced against contagion, proves nothing of that kind, but clearly shews the immunity conferred on a body of people by their having undergone the visitation of the epidemic. A detachment of ninety men were attacked with the cholera on the 11th May, 1818, encamped on their march to join the Saugor, or left division of the field army in Bengal, within six miles of that force. Twenty men were attacked the first day; on the following day they joined the great camp; and, "before the end of a week, there was not a single man of the detachment but was sent to the hospital labouring under cholera, *or other modifications of bowel complaints*. The men of this party mixed promiscuously with those of the Saugor troops, and yet of the latter not one individual got the disease." The explanation is to be found at pages 20 and 182 of the same work, where it appears that that force got through their attack in the preceding month.—I am, sir,

Your obedient servant,

REGINALD ORTON,
Surgeon, Army Medical Staff.

18, Vincent-Square, Westminster.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

On the Anatomy of the Thymus Gland. By SIR ASTLEY COOPER, Bart. F.R.S. &c. &c.

WE have to introduce to our readers another splendid work—the production of Sir Astley Cooper. Only a few months ago, we were called upon to notice a treatise by the same author on the structure and diseases of the testis; and previous to that, with an equally short interval between them, appeared his book on the anatomy, development, and diseases of the female breast. Such a rapid succession of important works evinces a zeal which calls for our most cordial praise. Young men may be alleged to write in the laudable desire of advancing themselves; and an occasional paper, contributed to some society or journal, suits very well with the reputation of a physician or surgeon actively engaged in practice; but there is nothing in the present instance that looks like selfish ambition. The author's pleasures are obviously centered in these scientific pursuits; and we feel happy that the medical profession can offer such an example of devotion to its best interests, as we witness in the course pursued by Sir Astley Cooper. How often do we see a man making his fortune by the practice of his profession, and then starting off from it into selfish, if not vulgar enjoyment! Our author, in his mature years, does not leave his profession, but is to be found following it in a manner which allows leisure for the pursuit of the science, and for collecting and arranging the many useful remarks which he has made in the active period of his life.

The subject of the present work is one which has been long known as having baffled the attempts of every anatomist in every age who has endeavoured to throw light upon it. And, accordingly, the thymus gland has for many years ceased to obtain much attention from physiologists. We may conceive, therefore, how great must be the satisfaction of Sir Astley Cooper in having succeeded, as we confidently believe he has, in developing for the first time the true intimate structure of this organ; for it might be shewn that our ignorance has

been equally great respecting its anatomical characters as regarding its functions. Neither are we without a theory on the uses which the thymus gland may serve in the economy, founded on the newly-discovered views of its structure; but it is obvious that the author is far more anxious to afford a clear and satisfactory demonstration of its organization than to dwell at much length upon the probable functions which it performs. The remarkable completeness of the illustrations which he has given, chiefly excites our admiration. The work is accompanied with no less than sixty-two distinct views of the gland, executed in lithography, the drawings having been made from preparations formed by the author's own hand. Sir Astley, we may inform our readers, has invited, in his preface, "those who are zealous in the science of anatomy" to view these specimens; and having been gratified with a sight of them, it is our duty to express how faithful are the representations of them contained in the volume before us. There are as many more specimens in the collection as he has had drawn; and nothing can excel the neatness and beauty with which they have been prepared, as well as the clearness with which they exhibit all the appearances that he has described.

Owing to the smallness of size, the delicacy of texture, and the soft and pulpy nature of the thymus gland in the human subject, Sir Astley despaired of discovering its minute structure by commencing with it; and he had recourse, therefore, to the same gland in the fœtal calf. We regret that our limits prevent us from noticing fully the processes by which he succeeded in obtaining his results. We must content ourselves with extracting the following passage, somewhat abridged, in which the most interesting circumstances relating to the gland in the human fœtus appear to be sufficiently clearly stated for our readers to comprehend them.

"When this membraneous covering is removed, the substance of the gland is exposed, which is found to be of the conglomerate kind, being formed of numerous lobes, which are connected together by a second covering of reticular tissue uniting the lobes to each other, and combining its parts by entering minutely into its interstices. The lobes of this gland differ in magnitude, but not one of them appears to be larger than a pea, and they vary from that of the head of a pin to the size above mentioned. When the form of the thymus is strictly investigated, the lobes are found to be disposed in a serpentine direction around a cavity hereafter to be described.....To proceed with the investigation of the structure of this gland, remove a very thin superficial slice of each lobe, or of several of these, and numerous little cavities will be seen, which may be set open after the organ has been hardened in spirits of wine, and these are the secretory cavities or cells producing the fluid which issues so abundantly. The lobes being further examined, beside their cells, are found to contain a small pouch at their bases, which leads into a reservoir, so that the secretion which escapes from the lobes finds a ready entrance into the cavity of the gland, from which it may be absorbed.

"If a pipe be introduced into the gland, and alcohol be injected, and the organ immersed in strong spirits, or a solution of alum, a large cavity will be filled, which I shall call the reservoir of the thymus. This reservoir forms a general communication between the different lobes.....When opened, after having been injected and hardened, its internal surface appears to be lined by a smooth membrane; but if it be at once dissected in water, this lining membrane is found to be of the mucous kind, for it is rather villous than smooth, and instead of having a few red vessels, when filled with a vermilion injection, it is found to be highly vascular, and the arteries which are distributed to it, may be seen meandering upon its surface and minutely dividing, so as to redden every part of it.....When the reservoir is floated in water, a number of small openings appear upon its internal surface, and if a probe be introduced into these, it passes into the pouch at the roots of the lobes, so that by these apertures the secreted fluid escapes into the reservoir. These orifices are not so numerous as the lobes themselves, the reason for which is, that each pouch communicates with more than one lobe.

"The boundaries or walls of the gland are full of secretory cavities or cells, which are extremely minute; they

communicate with each other, and open into the pouch of the lobes, and from the pouch into the reservoir."

Having been often struck with the remarkable abundance of secretion which the cells above described contained, the author looked naturally with interest to the vessels destined to absorb it, or convey it into the system. The universal problem has hitherto been to discover the *duct* of the thymus gland; and Ruysch thought that he had made this discovery in his young days, when, in his enthusiastic manner, he informs us, "*Persarum mihi videbar rege beatior.*" Our author has found out (we know not, however, whether he will measure his happiness according to the same standard as his predecessor, who was like him celebrated in his day for his admirable injections, and for his skill in tracing the minute anatomy of the glandular structures) that it is not a duct, but a series of absorbing vessels coming in profusion from all parts of the gland, and uniting to form certain large trunks, which carry the fluid into the veins. The following is his description of these vessels as they are found in the fœtal calf:—

"Such are the blood-vessels of the thymus, and I am next to trace the *absorbent vessels* and their glands. On the spinal surface of the thymus, numerous absorbent glands are found, and if these be injected many absorbents are discovered. But upon the posterior surface of the cornua and cervical portion, two large vessels proceed on each cornua and the side of the trachea towards the junction of the jugular veins with the superior cava. They are sufficiently large to admit a pipe employed to throw in coarse injection; and I can readily inject them with wax, dissect, and preserve them, so as to make very interesting preparations of them. They pass nearly straight upon the spinal surface of the cornua, converging a little as they proceed towards the sternum, and terminate in the jugular veins at their junction with the superior cava by one or more orifices on each side.....

"The vessels I am now describing, although of large size, are transparent, and possess valves, and, above all, if quicksilver be thrown into the absorbent glands of the thymus, small vessels are filled from them which open suddenly into a tube of considerable diameter, forming the two vessels I have mentioned; and further, to shew that they partake more of the nature of absorbent vessels than the structure of an excretory duct, they cannot be injected but in their course towards the veins from the valves which they contain. Around the thoracic portion, numerous absorbent glands are found, which send vessels into the veins at the junction of the jugulars with the superior cava. These vessels I consider and shall name *absorbent ducts* of the gland, and they are the carriers of the fluid (hereafter to be described,) from the thymus into the veins of the lower part of the neck."

He has not been equally successful in displaying these vessels in the human subject; and the difficulty of demonstrating them in so small a part may be easily conceived.

"The absorbent vessels I have only once been able to inject in the human subject, proceeding from an absorbent gland of the thymus. Absorbent glands are found at the upper part of the sternum in the mediastinum; also a small gland between the thoracic portions, and some at the junction of the thymus with the jugular and subclavian veins, where the principal trunks of the absorbent vessels at all periods of life terminate. Here the advantage of comparative anatomy is evinced, in the readiness with which the absorbent vessels, their glands, and the absorbent ducts, can be shewn in the fœtal calf."

To understand the function of the gland, the next step was to inquire into the nature of the secretion so abundantly found within it. Having submitted this fluid, which he obtained from the thymus gland of the calf in sufficient quantities, to examination, the following are the results shortly stated by him in summing up:—

"It appears by the analysis which I have given from Dr. Dowler, that this gland secretes a fluid which contains albumen and fibrin, and the microscope readily discovers white particles in it, and that, in short, it secretes all the component parts of the blood, viz. albumen, fibrin and particles, excepting that the particles, like those of chyle, are white instead of being red. I will therefore put the following query:

"As the thymus secretes all the parts of the blood, viz. albumen, fibrin and particles, is it not probable that the

gland is designed to prepare a fluid well fitted for the fœtal growth and nourishment from the blood of the mother before the birth of the fœtus, and consequently before chyle is formed from food, and this process continues for a short time after birth, the quantity of fluid secreted from the thymus gradually declining as that of chylification becomes perfectly established?"

We have thus given as accurate an idea of the contents of the work as can be done without the plates: these, of course, must be seen to be duly appreciated.

Reports of Medical Cases. By Dr. Bright. Vol. II. Part II. Price 9*l.* 9*s.*

One of the most useful portions of a medical periodical is that which is devoted to the analysis of sterling works, and especially where these are too expensive to become extensively circulated. This particularly applies to the Reports of Dr. Bright, many of the numerous valuable remarks contained in which, owing to the high price of the volumes, would become comparatively but little known were they not transcribed into other pages. We have already given a tolerably full digest of the two first volumes, and now proceed to the consideration of the third, or rather the concluding part of the second—for so it is called, though equal in size, and bound up separately from the others.

One of the most satisfactory accompaniments of Dr. Bright's observations consists in the conviction of their fidelity, with which the simplicity of his details, and the constant reference to hospital cases, unavoidably inspire the reader, so that his work may safely be pronounced one of the most important additions to clinical medicine which has been made in modern times. Without farther preamble, then, we proceed to analyze his observations on diseases of irritation.

Under this head our author includes many of the diseases usually called "Nervous," whether merely functional or involving structural changes. Hysteria—chorea—paralysis agitans—neuralgia—tetanus—and hydrophobia, are the chief maladies which make up the list.

HYSTERIA.

This, which may be called the disease of imitation, is considered under several heads, according to the form which it assumes, and the first of these which we find in Dr. Bright's arrangement is—

Hysteria imitating inflammatory action, a mask under which it frequently presents itself to us. The inflammation which our author has most frequently seen hysteria imitate is peritonitis, and the resemblance is sometimes so close as to require considerable attention in order to discriminate between the two: the distinction, however, is for the most part sufficiently appreciable in some incongruity of symptoms, particularly a tenderness beyond any thing which the pulse or tongue would lead us to expect; a hurry of breathing disproportionate to the other symptoms, and a sudden shifting of the site of pain; or the abrupt cessation, and equally capricious return of the complaints; lastly, the presence of other hysterical indications. Dr. Bright states (and from what we have ourselves witnessed we doubt not that most of our readers will be able to call to mind similar events) that he has seen hysteria repeatedly treated as hepatitis or pleuritis. The mammæ—the joints, particularly the knees—the spine—the soles of the feet—and, in fact, almost every region of the body, may become the seat of obstinate painful affections, which, by their subsequent history, evince that they had been but modifications of hysteria. As illustrations of the inflammatory disguise of the disease, we subjoin the following cases:—

Hysteria mistaken for Peritonitis and Hepatitis.

"I had a long interrupted attendance, in the years 1828 and 1829, on a young lady, the daughter of a medical man, who had been supposed by her father to labour under repeated attacks of hepatitis and peritonitis, being frequently seized with pain and acute tenderness of the abdomen, and more particularly of the right side; the pulse quick, the mouth clammy, and the nights sleepless, with occasional delirium. For this she was often bled to a large extent, and was put under the action of mercury. The attacks were sudden, and used to subside almost as rapidly as they came on, leaving her generally much exhausted by the treatment; she was, during the whole time, subject to dysmenorrhœa and leucorrhœa, and had so great a debility in her lower extremities that it amounted almost to paraplegia, and she was obliged to be carried up and down stairs. From the frequent bleedings, her aspect had become blanched and puffy. It was quite obvious that though the active remedies gave present relief, they always left her in a worse state than they found her; and she

had become so nervous that she could not bear the motion of a carriage, and every attempt to put her forwards was always followed by a fresh attack. Under these circumstances, astringent lotions for the leucorrhœa, and the zinc and gentian persisted in for several weeks, together with domestic circumstances demanding some mental exertion, restored her completely."

Hysteria supposed to be Peritoneal and Pleuritic Inflammation.

"Sarah Mitchell, aged about 22, was admitted under my care, complaining of palpitation at the heart, pain in the loins, and leucorrhœa, confined bowels and irregular catamenia. It appeared that she had now been out of health about three months; that the first attack was in the head, with a sense of fulness and great dimness of sight. She was freely bled, and getting better from this, was seized with pain in the abdomen and tenderness, for which she was bled and leeched. Pain then came in the back, followed by severe pains in the chest and violent palpitation of the heart, for which she was bled; on the whole she had been bled six times, besides having been leeched and cupped. I immediately ordered her fifteen grains of aloes and myrrh pill every night, and half an ounce of the compound steel mixture, and the alum and zinc wash to be injected into the vagina. While she remained under my care I had opportunities of witnessing several relapses of pain and tenderness, both in the abdomen and back, and for two days the headache was intense. She persisted, however, in her remedies, except that on one occasion cupping-glasses were applied to the nape of her neck, in consequence of the continuance of severe headache.—She left the hospital quite well.

Hysteria with spasmodic action may be regarded as the most common form of the disease, and the complications it exhibits are so multifarious as almost to defy enumeration: we shall, therefore, confine ourselves to a few cases illustrative of some of its rarer or more curious manifestations.

Hysteric Spasm with Coma.

"Rebecca Vincent, aged 17, was brought to the hospital, October 15, 1825, in a state of perfect insensibility to all external objects; countenance rather flushed, eyelids closed, and mouth obstinately shut, with no convulsive movements of any kind. Her hands were clenched, and upon forcibly extending the fingers they again immediately closed. The eye was not red, but the pupil appeared fixed when the eyelid was raised; breathing easy, but slow; pulse 106, weak; feet cold.

She was cupped from the neck, a blister was applied to her head, and sinapisms to the feet, and powerful cathartic injections were administered; before the following morning she was so much recovered as to be able to give some account of herself, and it then appeared that she had experienced one or two hysteric attacks. She complained of tenderness of the abdomen and intense headache; this was afterwards succeeded by urgent vomiting; and as these were relieved by leeches and sinapisms, she complained of pain in the left side of the abdomen; cathartics and a blister removed this; and in a few days she left the hospital well."

Hysteric Hiccup.

"I was requested to see a young lady in the City, who had lain for a fortnight, suffering day and night from a convulsive effort, something between a hiccup and an attempt to vomit: this was incessant; I heard it as I entered the house-door, though she was in the bed-room above stairs.—She was weak and exhausted, with a clammy perspiration and a dry tongue: it was impossible not to feel anxiety for the result. A blister to the nape of the neck, cold to the forehead, frequent doses of sulphuric æther, and purgatives of cathartic extract and the compound galbanum pill, tranquillized the whole in the course of the night, and in two days she was quite well."

Hysteria, with Spasmodic Exclamation.

"Mary Gosling, aged 18 years, was admitted, under my care, into Guy's Hospital, July 8th, 1829. It appeared that the catamenia had come on regularly at the age of eleven years, and she had enjoyed good health, upon the whole, till about ten months previous to the time of her admission; at which period, having suffered a good deal from anxiety and over-work, she became the subject of deep involuntary sighing, recurring very frequently; but this, within the last six weeks, had become much worse, and her present condition was very distressing. She was unceasingly uttering a sound, like 'Heigh-ho! Heigh-ho!' at regular intervals of three seconds, so that the sound was repeated twenty times in every minute, unless it was at times changed for the single work 'Heigh!' which was then repeated thirty times in the same period: this sound was, however, to a certain degree and for a short time under her control, so that she could check it, with much apparent exertion, for a few moments, while she uttered a short sentence; but it was immediately resumed when she ceased to speak; and if she attempted to put two or three sentences together, they were interrupted by this spasmodic and almost involuntary sound. She appeared to be much exhausted by the per-

petual exertion; and I immediately ordered her head to be shaved and kept damp with a cold wash, and occasionally to be sprinkled with water; and a blister to be applied between her shoulders. Fifteen grains of colocynth and calomel were immediately administered, and the camphor mixture with the sulphuric æther was given every six hours.

"9th.—The purgative had been assisted by some senna, and had acted very freely, bringing away a large quantity of hardened fæces, and the general result of the remedies had been most favourable; for towards four o'clock this morning the spasms had gradually subsided, and at the time I saw her she was perfectly tranquil. To insure a continuance of these favourable symptoms, I repeated the purgative two or three days following, and she was completely restored. She afterwards became the subject of cynanche tonsillaris; and when convalescent, had one or two slight relapses of her hysteric symptoms, but they were removed by similar remedies; and by the use of the shower-bath the tone of her system was established, so that she left the hospital quite well."

Hysteric Dyspnœa.

"I was passing through the wards of Guy's Hospital one day during the last winter, when one of the surgeons requested me to look at a female patient, who had formidable disease of the mamma: she had been seized with alarming dyspnœa; her respiration was performed with a most unusual effort, but was not so much hurried as laborious, and she complained of a constriction across the chest, which was altogether unconquerable. Pulse very quick: it had been believed by some that she suffered an attack of pneumonia; but there was no cough, and the breathing was rather with effort than with pain or difficulty. Her feet were quite cold; the pulse weak. She was in a state which might have resulted from sudden effusion into the chest, or the bursting of an aneurism. This was hysteria; and assafœtida was its cure."

Hysteric Dysphagia.

"Amongst the symptoms in hysteria, difficulty of deglutition is by no means uncommon. I once saw a female who was sent to the hospital as being the subject of stricture of the œsophagus. It was stated that the difficulty of swallowing had been upon her for several weeks, and was increasing. The surgeon under whose care she entered was immediately struck with her age (which was less advanced than that in which we usually find serious strictures of the œsophagus) and her appearance, which did not bear the marks of organic disease. However, he thought it right to examine the œsophagus by means of a probang; and no sooner was the instrument introduced than the patient went into an hysteric fit, which was followed immediately by hysteria in several females in the ward. The disease, fortunately for the young woman, proved to be nothing but an hysteric constriction, and was soon completely removed.

Hysteria imitating paralysis.—Dr. Bright remarks that it is not very common to find this modification of the disease uncombined with spasm, and that it is generally the presence of this latter circumstance which presents the real obstacle to the performance of certain voluntary motions: for example, the inability to empty the bladder, so frequently met with in hysteria, is oftener dependent upon the contraction of the sphincter than on paralysis of the viscus itself. Sometimes, however, the disease assumes the true characters of palsy, of which the following case may be taken as an illustration.

Hysteric Paraplegia.

"Ann Moss, aged 24, was admitted into Guy's Hospital, under my care, October 25, 1826. She was rather a delicate young woman, and had so completely lost the use of her lower extremities that she was unable to walk a step without support. It appeared that this had been her condition, with very little change, for the last seven years. Her catamenia had been obstructed about the time this complaint first came on, and for the last two years had never made their appearance. I examined the spine very carefully, but could discover nothing like disease. She occasionally complained of pain in her temples, and her bowels were constipated. The medicines I gave her were directed entirely to strengthen her general condition by bringing the stomach and bowels into better action; and I employed counter irritation both by the ointment of tartrate of antimony and by repeated blisters to the loins. For a long time very little improvement took place; the catamenia returned in sparing quantity, and attended by great dysmenorrhœa; but at length, when she had been seven or eight months under treatment, she became rapidly well, and left the house walking as if she had never been ill. So great was the change, that I received two very complimentary and grateful letters from the clergyman of the country village where she lived, for my successful treatment of his parishioner.—This disease, formidable and hopeless as it had appeared to those who had taken another view of its nature, was but a modification of hysteric affection."

Hysteria with mental affection is a very frequent modification of the disease, sometimes assuming the character of delirium, and at others bordering upon, if not actually passing into, the maniacal condition. Another aspect sometimes assumed by hysteria is that of hypochondriasis, of which numerous striking examples are on record. These occasionally last for many months, or even years, together, and are very little under the controul of medicine.

The several conditions above sketched, (for our space admits not of filling up the details,) serve to shew the principal varieties of this Protean malady. The uterus appears to be the centre towards which all these forms of suffering converge, and anatomical demonstration, as well as pathological inference, point to the nervous relations of this viscus as explanatory of its multifarious sympathies. In the treatment of such complaints, therefore, the first business of the practitioner is to act upon any indications which the state of the uterine system may afford him. Upon the whole, and more particularly in the simple forms of hysteria, purgatives, of a moderately stimulating description, and acting more particularly on the great intestines, are extremely useful: witness colocynth, aloes, and the fœtid gums. Tonics, particularly iron in its different forms, are very often of service, but it is, we think, true, as Dr. Bright remarks, that these are by no means inconsistent with the simultaneous employment of local depletions, particularly when the head appears to suffer from an undue determination of blood towards it. Cold, especially when suddenly applied to the face and head, is another important adjuvant in numerous cases; and to these are to be added, blisters over the seat of the pain where this exists; and these failing, the application of the belladonna plaister. The diffusible stimuli are also mentioned by Dr. Bright, particularly ammonia. We do not observe valerian among the list of remedies: it is one, especially in the form of the ammoniated tincture, from which great advantage has appeared to us in many cases to accrue.

Our next notice shall be of *Chorea*.

MEDICAL GAZETTE.

Saturday, May 19, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

THE HOMŒOPATHIC SYSTEM.

Is the reader a *homœopath* or an *allopath?* He may be neither;—perhaps he is an *antipath?* We will not be so ungracious as to verily believe that he does not understand our meaning: but yet for the sake of fairly introducing the subject into our pages, he will allow us to take it for granted that he does not well know what he is,—though that he is one or other of the three is beyond a doubt, that is, if he be but a practitioner at all.

For above two years last past we have been fully in the mind to enlist the attention of our readers to Hahnemann's system of cure; and often, we must confess, have we wondered that among our numerous and able correspondents none have seemed anxious to open the debate. Here has been an almost perfectly sealed book for British readers—here has been ground hitherto almost unbroken in this country, but promising to the explorer thereof a mine of the most curious interest. The homœopathic system, it must however be generally acknowledged, is something more than a merely curious and speculative subject: it is not the creature of a day, but has grown up steadily, and in spite of much deliberate resistance, during the better part of nearly half a century: its founder still lives, an aged man—on the frail tenure of whose remaining days the system by no means hangs: it has its disciples widely spread over the continent, its text-books, and its periodical journals in the principal languages of Europe: it can appeal to its records of innumerable, and, it is said, undeniable cures: it has divided into two

great sects, *for* and *against*, all the eminent physicians of Germany, Italy, and France; and, in fine, it has been honoured with a special commission of inquiry issued from the imperial court of Austria It is far from being our intention to calculate with its adepts and admirers in how long a time homœopathy shall have affected a revolution in the art of medicine; nor shall we venture all at once to advocate or cry down any of its doctrines without a fair hearing; but even the few remarks which we have just penned may suffice, we conceive, to justify the sort of notice and indulgence which we think it not improper to bestow upon the subject.

Dr. Samuel Hahnemann is now in his seventy-seventh year, in the healthy and vigorous exercise of all his faculties, and resident in the ducal town of Koethen, in the German States. Some of the chief incidents of his varied life may not be unworthy of a short recital. He was born at Meissen, in Saxony, in the year 1755. At the age of twenty, with but twenty ducats in his pocket, he went to Leipsic to obtain his education, and while there supported himself principally by the translation of English works on medicine. Two years after he attended the hospitals at Vienna, and gained the friendship of Dr. de Quarin, physician in chief to the Hospital of Leopold. His stay, however, at Vienna was but short; for he was obliged, by reason of his straitened finances, to accept the place of medical attendant and librarian in the family of the Governor of Transylvania; and there he remained for some time, until he was enabled to repair to the university of Erlangen, where he took his doctor's degree.

After graduating, he does not seem to have settled in the practice of his profession for several years. His inquiring mind led him chiefly to the pursuits of chemistry and mineralogy: with the state of practical medicine he professed to be disgusted; and rather, as it is said, than take it upon his conscience to live by an art so contradictory and empirical, he preferred to earn a livelihood by his old work of translation, and by contributing to the scientific journals of Germany. Nor was even this period of his life unattended by some fame: his researches on poisoning with arsenic, and the tests which he proposed for the medico-legal investigation of that subject, are still spoken of, and there is also a preparation of mercury which bears his name.

It was in the year 1790 that the first germ of his doctrine was developed. While translating the Materia Medica of Cullen, the future homœopath was so little satisfied with the account which he found in that work of the febrifuge properties of the Peruvian bark, and the gratuitous hypotheses by which they were attempted to be explained, that he resolved to make experiments for himself, and on himself, with that substance. He now discovered, to his great surprise, that the proper action of cinchona on a healthy man is the production of an intermittent fever, very analogous to that which it most constantly cures; besides which it gives rise to a number of other symptoms never before noticed, as occasioned by its medicinal agency. This led him naturally to inquire, whether the virtue of the bark did not mainly reside in its effect of producing in the human system a disease like that which it removes; and whether other pathogenetic substances might not owe their virtues to the exercise of a similar power. Experiments in abundance followed, and Hahnemann and his friends thought themselves amply compensated by the result. Their second step, however, was with the reputed *specifics*;—the analogy between mercurial action and the symptoms of syphilis, and between

scabies and the cutaneous eruptions produced by sulphur, were manifestly too obvious to be overlooked; and the general conclusions at which they arrived were these,—that our acquaintance with materia medica was only in its infancy; that all that was known about the efficacy of medicinal agents was the most prominent symptoms which they produced, and by which they were classed into their several varieties of emetics, purgatives, sudorifics, diuretics, and so forth, without sufficiently considering whether the said symptoms were not rather the effects of the reaction of the system, than the direct consequences of those ill-understood medicinal substances.

Hahnemann now duly respected himself as the founder of a system. Like the Count de St. Simon, whom his valet awaked every morning with an admonition to be mindful of the great things which he had to perform that day, our great homœopath began to learn for what high destiny he was born; and the time soon came when he was prompted to declare, that his "was the great gift of God to man."

In employing his newly-discovered principle in the treatment of his patients (for he was now induced even to court practice for the more complete development of his doctrines), he adopted two peculiarities which are, perhaps, the most remarkable belonging to the homœopathic system. In the first place, he directed all his attention to *symptoms*, and them alone he contended with, neglecting altogether the received notions of proximate causes, and those accredited groups of ailments to which the current names of maladies were given. The second peculiarity regards the *doses* of his remedies; and the extreme minuteness of these has almost made them proverbial. Two motives seem to have led Hahnemann to this singular scale of administering medicine. He considered in those his first essays, that he was about to exhibit a substance which would most probably in the first instance aggravate the complaint which he was about to remedy; and then he reflected that he was about to produce a morbid action in a body already strongly predisposed. Both these motives, however, only led him a certain length in attaining that minuteness which is at present practised by the homœopaths: the principle of it is professedly derived from what Hahnemann regards as his greatest and most original discovery —the almost incredible increase of virtue which is imparted to a medicinal substance by adequate trituration, agitation, and friction *.

It is easy to conceive what ridicule was immediately heaped upon this practice of tiny dosing: one facetious opponent compared it to setting a flea to draw a waggon fit for the draught of a team of oxen; another demanding to know whether, if an ounce of salts were thrown into the Lake of Geneva, it would not be enough to physic all the Calvinists in Switzerland? and so with the rest of the reasoners of this stamp. But, luckily for the homœopaths, it was perceived that a child could detect the flimsiness of those loose analogies, and this mode of opposition was, in consequence, soon pretty generally abandoned.

But to return to the personal history. It would fill far more room than we could spare to follow Hahnemann in his various movements from place to place. From Georgenthal (where he cured, with great eclat, a literary man who had been driven mad by an epigram of Kotzebue's) we may mention that he

* The ingenious method by which he contrived to attain those almost infinitessimal doses may be briefly mentioned. A drop of a given vegetable juice, added to and mixed intimately with 99 of alcohol, gives a preparation of which every drop contains 1-100th of a drop of the juice. One of these latter, again, mixed with 99 of alcohol, carries the division to the 1-10,000th, and so on. Sugar of milk, instead of alcohol, is used for dry substances; and the same minuteness is arrived at by taking the grain for unity.

went to practise in Brunswick, in the year 1794; whence he removed to Kœnigslutter, where he first began to feel the effects of that jealousy and ill-treatment with which he had so long afterwards to contend.

The interests of pharmacy were undoubtedly at stake, wherever Hahnemann's principles were successful: but this could never justify the persecution with which he was but too frequently afflicted. He was obliged to change his residence from Kœnigslutter to Hamburgh, to Eilenburgh, and to Torgau, successively; and in this latter town, at length, he was enabled for a time to pursue his researches in peace.

It may probably be surmised that there must have been something essentially savouring of quackery in the early proceedings of Hahnemann: but there appears to be no real grounds for such a surmise. There is not, nor, so far as we can understand, ever was there, any quackery about the homœopathic doctrines—no secret or mystery in any part of them — no devices for mere money getting: those doctrines are, moreover, any thing but a resource or refuge for ignorance; on the contrary, it is allowed that they require, for their successful employment, an unusually large stock of knowledge in physiology, pathology, and all the subsidiary branches of medicine, including particularly an extraordinary degree of acquaintance with the substances constituting the materia medica; and, withal, a store of experience such as few of the ordinarily-educated would have patience to acquire. So far was Hahnemann from ever dreaming of making a secret of his doctrines, or any part of them, that he availed himself of every opportunity of making them public. In Hufeland's Journal he published several of his earliest cases and cures; and acquiring confidence as he proceeded, he was at length unfortunately betrayed into a tone of indignation and reprisal on his opponents, for which his friends have never since ceased to be sorry.

The occurrence of scarlatina, in a severely epidemic form, in the year 1800, gave occasion for some new observations in furtherance of Hahnemann's doctrines. Belladonna was the remedy which his system pointed out, and which was actually found to be the most efficacious — at least, in his hands. But the founder of homœopathy did not stop here: he bethought him to try the effects of belladonna upon the healthy as a preservative against scarlatina; and experience is asserted to have completely verified the results which he expected. The homœopaths affirm, that no point has been more clearly established than this, their opponents having failed (whenever they did fail) simply from not properly following the directions of Dr. Hahnemann, in the due preparation and minuteness of the doses. They moreover state, that the law is general with respect to the preservative efficacy of the articles in the homœopathic materia medica,—the vaccine virus itself being no more than one of the said articles.

At length the principles and practice of the homœopathic system were given to the world in a collected form: the *Organon fur die Heilkunst* was first published in 1810, has already gone through four German editions, and is translated into the French and Italian languages.

Hahnemann now went once more to live in Leipsic, and is said to have soon gathered round him crowds of pupils and patients. It is related that he made some most valuable converts belonging to the faculty, by curing them of diseases when all hope of recovery on the *allopath* system was at an end. Dr. Necher, afterwards of Naples, was one of them; the Drs. Aegidi and Petersen were also cured.

In 1811 the publication of his *Reine Arzneimittellehre* was commenced,—his Pure Materia Medica,—the most voluminous of his works, running to six volumes, and the last of which came out in 1821. In this production he was most materially assisted by some of his friends and many of his most zealous pupils. It is described as "a rich arsenal, from which homœopathy may arm herself against every known disease: it contains at present nearly 80,000 combinations of symptoms, with the corresponding substances which shall produce their counterparts; and it goes on every day to be still further enriched, and to such an extent, as to leave it utterly impossible to assign any limits to the future developments of homœopathy*."

But what has been the Hahnemannian treatment in *chronic* cases all this time? It is admitted by the homœopath himself, that up to so late a period as 1816, those cases were his greatest stumbling block; and that he looked upon them as a class of affections in the highest degree *rebellious* against all principles drawn from his experience. We cannot enter here into a detail of all the facts and inferences by which he arrived at his present singular theory of chronic diseases, but we may mention simply the jet of it, which is, that the far greater part of those diseases are generated by a principle which he denominates the *psoric virus*, of which the *psora* itself is the simplest development. The treatment by which he has followed up this theory is said to be eminently successful, though many of his *antipsoric* remedies have certainly been hitherto deemed as little better than inert, in ordinary medical parlance,—such as silex, chalk, charcoal, sepia, and certain harmless vegetable powders. In 1828 the first volume of his work on Chronic Diseases was published, and the fourth in 1830.

For the last twelve years Hahnemann has been residing principally at Koethen, under the distinguished patronage of the reigning Duke: there he enjoys an extensive practice, with leisure to put a finishing hand to those works which have occupied the better portion of his long life.

Our biographical sketch we find has carried us to a greater length than we expected: we are in consequence obliged to defer some general remarks, which we meant to have offered on the Hahnemannian system itself. So much, however, may not be thought amiss by way of introduction.

* Bibliotheque Homœopathique, publiée à Genève, par un Societé des Médecins, No. 1, Avril—Mai, 1832. An interesting first number, to which we are indebted for several of the facts given above.

ROYAL INSTITUTION.

Friday, 11th May, 1832.

After Mr. Cowper had delivered a valuable lecture on the *Recent Improvements of the Loom for Weaving Silk*,—but an account of which it would be difficult to give without reference to diagrams and complicated models,—our attention was next called to that most interesting subject,

The Magneto-Electric Spark.

Since Mr. Faraday's recent discoveries and public explanations of his views regarding the magneto-electric induction, Dr. Ritchie has been engaged in going over the same ground, and has succeeded in contriving a mode by which the phenomenon of the electric spark proceeding from a permanent magnet, may be made visible in the most satisfactory way. The doctor prefaced his account of his plan by some general remarks on the object to be effected. The spark was to be produced on the separation of the wires which borrowed their influence from the magnet: and for this purpose he employed a deep and acutely bent horse-shoe magnet, between the extremities of which were suspended two tubes; a wire was immersed in each tube, and their connexion maintained by a conducting medium; the wires ultimately were connected to the magnet by the folds of a thin copper riband. An explosive mixture of oxygen and hydrogen was then introduced from a bladder and stopcock, and by the motion of a lever like a pump handle, the separation of the wires was suddenly effected, and the spark made sensible to all by

the explosion of the gases. The spark itself, independent of the explosion, was abundantly visible upon approaching the table.

Mr. Faraday now came forward, and after expressing his high gratification on finding his investigations thus so ably followed out, informed the audience that he had succeeded in devising an easy method by which the spark from the natural magnet could be rendered clearly visible. From a small bar of iron, about nine inches long, were raised the connecting wires at right angles, and which, by another perpendicular bend, were made slightly to overlap each other, the undermost terminating in a disk about the size of a penny piece. By a moderate but rapid percussion of the bar against the natural magnet, the disk and overlapping wire were separated by their elasticity, and a brilliant violet-coloured spark made its appearance. Nothing could be more satisfactory than this simplest of all experiments, and the crowded theatre expressed their delight by repeated rounds of applause. This spark, it should be added, was clearly visible in the strongest light. All the world, no doubt, will now be made familiar with the production of fire from the magnet in all its forms,—the temporary, the artificially permanent, and the natural; nor can limits be set to the speculations of the uses to which the knowledge of this fact may be applied. What would not Wollaston have given to be the author of this discovery; or to have lived to see the day in which it was made!

On the table, in the library, among other rarities, were two splendid specimens of petrifaction of the gum-tree from the New Hebrides: they were very large and very perfect; and the glossy streak of the fibre gave them the freshest look possible.

The subject announced for last night's conversazione was — Mr. Faraday on the Crispation of Fluids upon Vibrating Surfaces.

OFFICIAL PAPER ON CHOLERA.

Council Office, Central Board of Health,
9th May, 1832.

Precautionary Hints to Persons residing in Places suffering, or likely to suffer, from Cholera; with Concise Directions for the Treatment of those threatened with or actually attacked by the Disease, in Situations where Medical Advice cannot be immediately obtained.

Heads of families living in the country, and benevolent individuals wishing to afford remedial assistance in this destructive malady, ought to provide themselves with the following articles, viz.;—

	lbs.	oz.
Tincture of opium (laudanum)	0	2
——— of catechu	0	4
——— of assafœtida	0	4
Aromatic spirit of ammonia	0	4
Compound spirit of lavender	0	2
Oil of peppermint	0	0½
Castor oil	2	0
Ipecacuanha in powder.....	0	2
Mustard in do. (best Durham)	10	0
Compound chalk powder ...	0	4
Sulphate of quinine	0	1

Pills, No. I.

Six Dozen { Calomel, 2½ grains; Opium, ¼ grain; Cayenne Pepper, 2 grs. } in each pill.

No. II.

Three Dozen { Calomel; Comp. Ext. Colocynth } of ea. 2½ grs. in each pill.

No. III.

Three Dozen { Blue pill, 2 grains...; Rhubarb, 2 grains... } in each pill.

Powders, No. IV.

		lb.	oz.
Calc. Magnesia, 2 pts.; Rhub. in powder, 2 pts.; Ginger in ditto, 1 part, carefully mixed	...	0	1

No. V.

Calomel, 1 grain; James's powder, 2 grs.; Nitre in powder, 5 grs.	...	0	0½

Liniment, No. VI.

Comp. Soap Liniment, with opium, 8 parts; Tinct. Cantharides, 1 pt.	...	0	3

Mustard Poultice, No. VII.

The mustard poultice is made by mixing equal parts of mustard powder and crumb of bread into a paste with hot water; or by mixing equal parts of mustard powder and thick porridge.

Bags or stockings to hold heated bran or salt.
Stomach and feet warmers.
Enema syringe.
A graduated glass measure (1 oz.)
A set of scales and weights (grain.)

The above supply is calculated for the number likely to be attacked in a population of 500; and in price, as estimated by a London chemist, will not exceed 3l. 3s.

Precautions.

1. *The Clothing* should be warm. Woollen stockings ought to be worn, and flannel next the skin—at least over the belly and loins.

2. *Diet.*—Avoid, above all things, overloading the stomach. Indigestion, however produced, disposes the body to this disease.

If in easy circumstances, take for dinner a moderate quantity of roast meat in preference to boiled, with stale bread or good potatoe, two glasses of wine with water, or an equivalent of weak brandy or whisky and water, or of sound porter or ale. Eat garden stuff and fruit sparingly, and avoid fat luscious meats. In short, whilst under apprehension of cholera, use a dry, nutritive diet, sparing rather than abundant; observe great caution as to eating suppers, for cholera most frequently attacks about midnight, or very early in the morning.

In case of costiveness, take one or two of the pills, No. 3, going to bed; or one or two of the pills, No. 2, in the morning, should no effect be produced by No. 3; but avoid salts, senna, and all cold, drastic purgatives.

3. *Exercise.*—Moderate exercise in the open air, in fine weather, is conducive to health; but the greatest care should be observed by all, more especially by the weakly and the aged, not to carry that exercise to fatigue or profuse perspiration, nor to sit down with wet feet or wet clothes.

Treatment of the Premonitory Symptoms of Cholera.

4. In a very large majority of cases, the attack of cholera is preceded by a looseness of bowels of longer or shorter duration, say twenty-four hours. It is in this stage that remedial assistance is most efficient, and that life may be saved with the most certainty, by checking the disease in its commencement. When, therefore, the bowels become relaxed without an obvious cause, where cholera is prevailing at the time, the following measures should be adopted without loss of time:—

5. In the case of adults, previously healthy, let blood be taken from the arm to eight or ten ounces, or by ten or twelve leeches to the pit of the stomach, or by cupping.

Should the loose motions be of a darker colour than natural, give two pills of form No. 2, and four hours after a table-spoonful of castor oil, floating on half a wine-glass-full of gin and water, brandy and water, or cold coffee, with ten drops of laudanum if there be griping pains. Confine the patient strictly to bed, and give the following draught at night:—

Cinnamon or peppermint water, half an ounce*; laudanum, twenty-five drops.

6. When the purging is of the ordinary, bilious, and fæculent kind, with griping and flatulence, give ten drops of laudanum and forty of tincture of catechu in the same vehicle, every hour, for five or six hours; or twenty grains of the compound chalk powder every second or third hour, should relief not be obtained sooner.

A warm bath for half an hour, followed by rubbing with flannel or flesh brushes; warm fomentations to the belly by means of bladders half filled with hot water, or flannels soaked in hot spiced wine, or in hot spirit and water, will afford much relief.

7. When there are cramps, a dessert-spoonful or two of the liniment No. 6 should be assiduously rubbed on the part affected.

8. If there be nausea or sickness, without acute pain at the pit of the stomach, give an emetic of twenty-five or thirty grains of ipecacuanha in half a pint of warm water.

9. When giddiness and pain at the pit of the stomach are present, bleed as above, and give a tea-spoonful of the aperient powder No. 4.

10. Let the diet in all these premonitory stages consist of light farinaceous preparations: — sago, tapioca, panada; chicken broth and tepid drinks, to promote perspiration.

11. Should debility, with chills and sweats, remain, give two grains of sulphate of quinine three times a day for two or three days. This medicine will often be found to check the relaxation of the bowels.

First Stage of the Attack—Treatment.

12. When the motions have lost the appearance of fæculent matter, and have put on that of rice-water or chicken-broth, with vomiting of similar liquids, spasms, intense thirst, irregular, slow, and weak pulse, give an emetic of half-a-pint of a solution of common salt, as strong as it can be made, with a tea-spoonful of mustard powder. Place a mustard poultice, No. 7, over the whole stomach, belly, and front of the short ribs, having previously rubbed the parts with the liniment. Give one of the pills, No. 1, every alternate half-hour, and in the intervals two table-spoonsful of weak brandy or whisky and water; cold if preferred. Let the patient drink cold water or iced water if it can be had, allowing no more than two or three table-spoonsful at a time, or bits of ice the size of a nut may be given to be swallowed whole, to allay the burning sensation at the pit of the stomach. Let bags or stockings, filled with heated bran or sand, be placed along the patient's spine or sides, and feet warmers applied to his feet. Let him be kept still, if possible, wrapt in warm blankets, but not oppressed with heat or coverings, particularly over the chest and neck.

Second Stage of the Attack.

13. If, notwithstanding these measures, the patient should appear to be sinking, the pulse becoming weaker, the skin colder, the breathing more laborious, the individual appearing less anxious about his own situation, then, in addition to the steady application of the measures already recommended, let an injection be thrown up the rectum, consisting of two or three pints of water, as warm as the hand can conveniently bear, with a

* Peppermint water may be made by rubbing down five drops of oil of peppermint with half a tea spoonful of sugar, adding a table-spoonful of water by degrees.

small wine-glassful of brandy or whisky, to be repeated, if thought necessary, at intervals of an hour.

Third Stage.

14. When the pulse at the wrist has ceased, or become almost imperceptible, with coldness of the extremities, and perhaps blueness of the surface, particularly of the lips, hands, and feet; irregular breathing, loss of voice, suppression of urine, ghastly countenance, without delirum; although under these awful circumstances there is but little room for hope, our exertions should not cease.

15. At this stage of the attack the vomiting and purging will generally have ceased, or at least be much diminished; the belly will be drawn in, and pain, sinking, and death-like oppression will be felt about the heart.

16. Let the hot water injection be repeated, with two or three drachms of the tincture of assafœtida, and retained for some minutes by means of a napkin.

17. Let mustard poultices be applied to the inside of the thighs and calves of the legs, in addition to that on the belly, which may be removed to the sides of the chest or back; let the limbs be diligently rubbed with warm cloths; let small quantities of light cordials be given at intervals—such as a tea-spoonful of compound tincture of cinnamon, or of aromatic spirit of ammonia, in a table-spoonful of water—snd let the treatment ordered for the second stage be continued until the pulse becomes distinctly perceptible at the wrist *.

Stage of Re-action, or Fever.

18. When the pulse has begun to rise, and the heat and natural colour begin to return to the surface, keep the patient perfectly quiet, but let him be carefully watched, for a sudden sinking of the powers of life not unfrequently occurs at this period of the disease. Opiates of all kinds must now be withheld; and wine, brandy, and other stimulants, used very sparingly, and withdrawn altogether as soon as the pulse and heat are steadily re-established; when mild tepid drinks are to be substituted, and the powder No. 5 given every hour, instead of the medicines hitherto used, should the bowels be torpid.

19. Under this treatment a warm copious sweat often breaks out, or a more healthy discharge takes place from the bowels, or some urine is passed, which, of all others, is the most favourable sign. When such is the case, the patient, with proper care, will often pass into a state of convalescence, without further difficulty or danger.

20. It often happens, however, notwithstanding all our care, that the re-establishment of the pulse and heat are closely followed by symptoms of fever, or by some degree of stupor, or by great oppression of breathing, or by distention and tenderness of the belly; all of which indicate danger.

21. The moment such symptoms appear, bleed from the arm, or from the part most affected, by leeches or cupping, to ten, twelve, or sixteen ounces, according to the effect produced by the bleeding. Reduce the temperature of the patient's room, give cool drinks, and apply cold wet cloths, or pounded ice in bladders, to the head; and give the powders No. 5, as already ordered.

22. When convalescence has begun, observe the strictest care as to diet. At this period a full meal has, in numerous instances, brought on a relapse. Indeed, animal food, even in small quantity, under these critical circumstances, has often been attended with dangerous consequences to those just recovering from cholera. To such, even the mildest articles of food should be given in much smaller quantities, and at shorter intervals, than to those in health; and their ordinary diet and habits should be very cautiously resumed.

W. Pym, Chairman.

* The following plan of treatment, proposed by Dr. Stevens, and acted upon under his direction, has excited some notice, and is stated to have been attended with very considerable success in all stages of the disease:—

Supercarbonate of Soda, half a drachm.
Muriate of Soda (Common Salt) 20 grains.
Chlorate of Potass, 7 grains.

To be given in half a tumbler of water every hour, until the patient begins to recover from the collapse.

Dry heat, frictions, mustard poultices, and injections of hot salt and water, were used at the same time.

MR. MARK ON CHOLERA.

We have received a very elaborate paper on cholera from Mr. E. R. Mark, of Newcastle. We cannot make room for the details, but we shall state the results. Mr. Mark strongly protests against the use of opium in the manner usually recommended, it being "as decidedly contra-indicated as brandy in inflammation of the brain." The patient is directed to be placed in a warm bed as soon as any indisposition is felt, and if there be much sickness, an emetic of hot water and salt is to be administered; after which, the peculiarity of Mr. Mark's practice consists in giving doses of opium not exceeding the sixth part of a grain, but repeated every hour. "I have given (says he) opium as above for thirty-six successive hours without the slightest stupor or drowsiness." The opium is combined with two grains of calomel in each pill, the intention being to affect the system with the mercury. The purging having been arrested for about twenty-four hours, rhubarb and magnesia, with ginger, is given, as a laxative. Sinapisms were also used.

Mr. Mark states, that of ninety-five patients. treated on the above principles, only six were carried off by the disease, though "slight" and "doubtful" cases are excluded.

LONDON UNIVERSITY.

We have received the account of the annual distribution of prizes, which took place on the 15th instant; but having declined to insert similar announcements from other medical schools, except as advertisements, we are compelled to omit it. The same remark applies to the list of questions with which we have been favoured.

DEATH OF CUVIER.

We lament to announce the death of Baron Cuvier, which took place at Paris on the evening of the 17th instant. On the 16th he had an apoplectic seizure, accompanied by paralysis, for which he was bled, and regained his consciousness to such an extent as to have been aware of his situation. He expressed his regret at being compelled to leave several works unfinished, particularly his Comparative Anatomy, about which he was occupied at the time of his attack. Cuvier was the son of a protestant clergyman, and was born in 1769, a year prolific in great men, among whom few have left a name greater in science than this distinguished naturalist. We shall probably take a future opportunity of giving some farther details regarding his biography.

DEATH OF DR. DUNCAN.

Dr. Duncan, Professor of Materia Medica in the University of Edinburgh, died on Sunday last. We believe his disease was pulmonary consumption, and that it was his intention to have tried the effect of an Italian climate, had he survived till the autumn. Arrangements had been made for Dr. Christison to lecture in his place, and this gentleman is of course a candidate for the vacant professorship; others are no doubt in the field, but we have not heard who they are.

NEW EPIDEMIC IN FRANCE.

A disease of a very formidable nature, and consisting of profuse sweating, with miliary eruption and extreme debility, has manifested itself in some parts of France; and, notwithstanding the presence of cholera, has excited so much attention, that a medical commission has been dispatched from Paris to the infected districts to examine into its nature.

CESSATION OF CHOLERA IN LONDON.

The disappearance of cholera in the metropolis has, we rejoice to say, been officially announced, and clean bills of health were yesterday (Friday, May 18) granted at the port of London.—There have been altogether 2619 cases, and 1381 deaths.

REPORT OF CHOLERA, UP TO FRIDAY, MAY 18, 1832.

New cases in Great Britain (exclusive of London) since our last report ...	364
Deaths.....	170
Total number of cases throughout Great Britain (inclusive of London) since the commencement ...	12613
Deaths...	5134

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

May 1832.	Thermometer.		Barometer.	
Thursday . 10	from 33 to	53	30·24 to	30·33
Friday. . . 11	33	57	30·30	30·22
Saturday . 12	33	53	30·04	29·76
Sunday . . 13	35	53	29·68	29·67
Monday . . 14	31	53	29·75	29·71
Tuesday . 15	34	57	29·78	29·74
Wednesday 16	29	57	29·79	29 87

Prevailing winds N.W. and N.E. Alternately clear and cloudy; rain at times on the 11th, 13th, and 15th. Rain fallen, ·5 of an inch.

On the afternoon of the 15th, from three to four, this neighbourhood was visited by thunder and lightning, accompanied with a shower of hail. All who describe the peal of thunder heard at 20 minutes after three, give it as their opinion that it was by far the loudest ever heard; several have felt slightly the effects of the electric fluid, which preceded the thunder about half a second. An oak tree, about a quarter of a mile eastward of this place, was shattered to pieces, and the ground thrown up at a little distance from its foot. At three o'clock the hailstones fell so thickly, that in less than three minutes the ground was completely covered; the hailstones were remarkably soft, resembling rather lumps of snow, of an irregular form, and measuring, in the broadest part, half an inch. During the storm, the thermometer fell from 57 to 43.

Charles Henry Adams.

LITERARY INTELLIGENCE.

An edition of Dr. W. Philip's work on Acute and Chronic Diseases, with notes, has just been published in America, by Professor Miller, of Baltimore.

NOTICE.

We have received Dr. Alexander Thomson's handbills, addressed to the English residents in Paris, on the subject of cholera; they are unfit for publication in our journal.

ERRATUM.

In our last number, page 191, in the signature to the communication on Scirrhus of the Brain, *for* "Jolly," *read* "Solly."

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 26, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Part I.—Lecture XXXIII.

Remittent Fever.

I now enter, gentlemen, upon the subject of remittent fever, which resembles both intermittent and continued fever, and partakes of the character of each. Essentially it is the same as intermittent fever. It arises from the same cause, I believe — malaria; but either through a modification of the malaria, or the influence of such causes as predispose to, or excite, continued fever, we have only remissions;—not intermissions, but remissions. The disease is really continued, although remitting, having a relaxation of severity at different hours.

This disease occurs in hot climates particularly, and in cold climates during hot seasons; it therefore occurs when and where there is the greatest abundance of vegetable matter, and all circumstances that are the most favourable for decomposition and the production of *aguish* fevers; and it also, you see, occurs when there is the greatest prevalence of other causes which are likely to excite *continued* fever—excitement and relaxation of the body, and disturbance chiefly of the abdominal organs.

Duration.—When remittent fever is acute, it seldom, I believe, lasts more than six weeks. There is continual fever day and night, and every day, but the patient is much less hot at one time than at another, so that, although he has constant feverishness, he feels himself far less ill at different intervals.

Remittent Fever marked by paroxysms of Ague.—Sometimes, besides the continual fever, there is, in addition, a regular paroxysm of ague. I have seen a person hot every day and every night constantly, except perhaps every other day, when he had a cold fit, and then the heat would become more intense, and sweating occur; yet when the sweating was over there was no intermission, but the patient was hot again.

The usual form is continued feverishness, with a great aggravation of it at particular periods; or, if you choose to put it in another way, the symptoms of continued fever, but a relaxation of these symptoms at particular periods. But besides, I have seen, although I do not recollect it being described—I have seen continual fever, with a paroxysm of ague coming on at regular intervals in the midst of it.

Marked by Chilliness or Sweats.—Sometimes, instead of remissions, or an aggravation of the feverish symptoms, you will have during this continual fever the remittent nature of it shewn only by occasional chilliness. I have frequently discovered that continued fever, as it appeared, was remittent fever, by noticing, that although there was continual feverishness, heat, and thirst, yet the patient in the midst of it was frequently very chilly; and on other occasions by observing that, in the midst of the heat, there was from time to time profuse sweats. The nature of the disease was pointed out to me either by the occurrence, in this continued fever, of chilliness—even rigors—from time to time, or the occurrence of sweating from time to time. The correctness of this opinion has been proved, by giving the patient quinine, and getting him instantly well. I think, so far as my own observation goes, that remittent fever shews itself sometimes in these two forms.

Intermittent fever will become remittent, if the causes of continued fever be superadded after intermittent fever has began. Supposing a patient has ague, and goes through great fatigue or want of rest, or is

exposed to wet or cold, he may be thrown into continued fever. These circumstances alone would not cause ague, but ague existing, the disease becomes remittent fever. The causes of continued fever are applied to a constitution labouring under intermittent fever, and then you have remittent fever. Frequently, however, the disease is produced from the very first.

Chronic Remittent Fever.—Dr. Macculloch imagines that intermittent fever is often a chronic affection—that it is often mistaken for hypochondriasis—and that the paroxysms are not observed, because they occur in the night. Now I am persuaded that he is correct; I am persuaded that many persons have long-continued remittent fever—frequently for many, many months, and, its true nature not being detected, it is thought to be some indescribable complaint. I have now under my care a patient who appears to be in this state—a man who is frequently very hot, complains of heaviness in the head, is affected in his mind, and has great depression of spirits. I believe the observation of Dr. Macculloch to be right. This man was taking five grains of sulphate of quinine three times a-day, without any effect; but on increasing the dose to ten grains, I found him better. I have seen many such cases, and they long puzzled me—oppression of the head, heat occasionally in the night, and great depression of spirits.

Intense Remittent Fever.—The acute disease frequently occurs in a dreadfully violent form, and it is as dreadful in rapidity as in violence. It is in hot climates that this particularly occurs. This form of the disease is so violent, and so acute, that there is at first delirium and violent vomiting, with an absolutely roasting heat at the stomach, great pain, tenderness, and agonizing distress, at the epigastrium.

Besides these local symptoms of the head and the epigastrium, there are all the signs of a violent inflammatory fever; and all these symptoms, both local and general, are soon followed by the most awful debility, and by all the signs of typhus.

The fever of the East Indies is a bilious remittent fever—a violent and acute remittent fever, with great bilious symptoms, and at Bengal it arises from the overflowing of the Ganges, and the consequent fall of the waters in November and December, acted on by a high temperature. After the Ganges has overflowed, the waters fall in November and December, and, through the heat, such emanations are produced that they cause this fever.

After these violent symptoms of delirium as to the head, and pain and vomiting as to the epigastrium, there is, after a certain time, a remission and a sweat, which occur about twelve or fourteen hours after the attack; and the pulse, from being 130, may fall down to 90, and the mind is then restored and the delirium ceases. The remission, however, is but short; the symptoms soon reappear, and then they are succeeded by the awful debility to which I have referred. If the disease prove fatal, there is very soon no remission at all; the gastric symptoms become more violent than before, the tongue becomes black, the pulse small, and the patient sinks. If it be fatal, it generally proves so, we are told, (of course I am only speaking from information which I have derived from authors), between the third and the seventh day, but occasionally it has been known to last fifteen or even twenty days.

The fever of the East Indies is a violent bilious remittent fever, accompanied by great derangement of the liver and stomach, and it occurs in various other parts of the East than Bengal. This disease has appeared with some other symptoms in the Mediterranean, in North America, and in the West Indies. In the Mediterranean it has appeared at Rochfort, Bourdeaux, and at Lisbon; in the south of Spain, at Cadiz, Gibraltar, and Minorca. There is here, in addition to the symptoms I have already enumerated, yellowness of the skin, and a vomiting of black matter. The disease, thus varied, has been called *yellow fever*, from the yellowness of the skin.

In America it is said that generally thirty-six or even seventy-two hours elapse before the remission takes place.

Black Vomit.—This black vomit, which is as much characteristic of yellow fever as the yellowness of the skin, is thought to be bile a little changed from blood—imperfectly formed bile, or even blood only a little changed; indeed, some suppose that, of the two, it would be more proper to call it blood than bile. It is imperfectly formed bile, or blood but slightly changed; indeed it is said that it has been found on the surface of the stomach when there was none in the duodenum, as though it had not come from the liver.

This black vomit is said to be by no means acrid. Dr. Physie, of America, says it is tasteless, and that he dropped some into his eye, and the organ did not become irritated; that, indeed, he found an abundance of it in the stomach, when a different fluid was observed by him both in the gall-bladder and the gall ducts. It is decidedly of the appearance of charred blood—of blood that has undergone certain changes, and is poured forth from a mucous membrane. Another gentleman has carried his experiments with the black vomit farther. Dr. Ffirth tells us that he procured two ounces from a patient, (whether dead or alive I do not know,) and that he drank it undiluted, and found it harmless. Before this, he had drunk a large draught of it, mixed with water, with

impunity; but, in its genuine state, he took two ounces, and said he was none the worse for it. He wished to try it in a solid form, and therefore made an extract into pills, and he says that he found as little inconvenience from them as he had previously done from his black draughts. The fluid and concretion of melanosis is also generally inert.

Cause of the Yellowness of the Skin.—In regard to the yellowness of the skin, it is ascribed by some to bile pervading the system, but there are very great doubts as to whether that is its origin. The bile in the disease is very abundant, and may be regurgitated by violent vomiting into the veins, through the compression of the parts; and, indeed, bile is abundant in the urine. There is a great collection of bile in this disease, and it may find its way into the veins, and more or less will go into the circulation, but there is no obstruction. At any rate it is certain that, if it be bile, it is not obstruction that produces regurgitation of it, for the fæces are well tinged with bile. One reason for saying that it is not bile is, that it will occur in patches. It is not more visible in the eyes and nails than in other parts of the body, and it is not diffused pretty equally throughout the body, as we observe in jaundice, but it occurs in patches, as ecchymoses would do; and it would rather appear to owe its origin to the blood being changed—to the serum, with a few red particles, escaping more or less into the cellular membrane, the blood having undergone a change. It is altogether analogous to ecchymosis when nearly gone, which does not arise from bile, but from an effusion of blood that is subsequently in a great measure absorbed, the portion which remains giving a yellow colour. Sir Gilbert Blane says that this yellowness has spread from bed to bed without any increase of the malignity of the disease; that when yellow fever has prevailed in the West Indies, he has observed it spread from bed to bed, and bearing no proportion to the malignity of the disease; that it appeared in two vessels, the Alcides and the Royal Oak, when there was only a slight indisposition. He also says, that, unlike jaundice, it begins usually about the parotids; there is usually fulness and flushing of the face and neck, but particularly about the parotids: here there is the most blood, and here it is that the yellowness appears in the most marked manner.

Influence of Temperature. — This yellow fever (or *causos icterodes,* as some call it) is apparently another form of bilious remittent fever, and it occurs only in countries and in seasons where the heat is such as would destroy or arrest the plague; and it occurs, too, in hot seasons, such as would usually put an end to contagious typhus fever, when that is prevailing. Yellow fever, or bilious remittent fever, are always extinguished by cold weather; as soon as the cold sets in, they cease. It is said that the blacks, in America, are usually less violently affected than the whites; but that if they go into a cold country, and reside there for a time, and then return to America, they suffer equally with the whites. Persons who are unaccustomed to a high temperature, suffer, it appears, the most from this disease. Blacks coming from hot countries, or having descended from parents born in hot climates, suffer less; but if they reside in cold climates their constitution partakes in some degree of that of the white, and then they suffer far more than others of their black brethren.

Cold countries, and the cold seasons of hot climates, are free from this disease. It generally occurs only in the tropics, and elsewhere when the heat is as high as in the torrid zone; but very intense heat in cold climates has been known to occasion it. If the temperature have accidentally been as high as in a hot country, then even a cold climate has been subject to it. Sir John Pringle, for example, says that he has sometimes seen yellowness of the skin in Flanders. Dr. Brocklesby mentions seeing it in 1758 in the Isle of Wight. Dr. Home saw it in 1743 at Worms, in Germany. All those places which are subject to it have a quantity of wet dead stagnant vegetable matter. It prevails in the West India Islands, in Charlestown, Norfolk, Providence, Philadelphia, New York, Baltimore, and Boston. Those places that have the largest quantity of vegetable matter for decomposition when the hot weather comes, and have the most moisture, are most subject to this yellow fever. It occurred at Lisbon till the earthquake happened. It occurs also at Cadiz, Bourdeaux, Rochefort, Seville, and Gibraltar. Now the latter place is situated high enough; but although high in reference to the sea, the town is low in reference to the mountains. The rain, it is said, that runs off 4000 acres streams towards the town, bringing with it from thence a quantity of vegetable matter, as well as fragments of vegetable refuse from the markets. Even straw has been said to afford the source of this disease. Sir John Pringle says that the straw was very injurious to the army. The rocky soil here increases the mischief, as it causes the water to remain.

Notwithstanding, however, it would appear that yellow fever is the product of malaria: there has been a difference of opinion entertained on this subject at different times. A Mr. Vines, a planter and physicker in the West Indies in 1647, writing from Barbadoes, ascribes the yellow fever to the Lord's heavy

wrath, and not to any thing in the climate. The island was not cleared for a considerable time after he wrote. The disease prevailed then to a greater extent than it does now. A Spanish officer named Armesta, who ascribed the disease to local situation and atmospheric causes, was actually at one time arrested, and obliged to retract his views, as being "false, dangerous, and seditious opinions." So violent was the opinion entertained at one time, that it had nothing to do with local situation, that 1500 copies of this officer's book were burned. In 1800, the government had a different opinion, and ascribed it to something in the atmosphere, which really is the case—that is to say, to miasmata.

Contagion.—A great question has arisen in the western hemisphere, and in the Mediterranean, as to whether yellow fever is contagious or not. Dr. Ffirth, of Philadelphia, who has tasted the black vomit in the way I have stated, says that he has frequently inoculated himself with it, and also with the serum, saliva, &c. of patients labouring under yellow fever, but that no prejudicial effect was produced. It is said that it is never propagated a mile from Philadelphia, and that therefore it must owe its origin to local causes. Dr. Rush, who was a celebrated American physician, and a violent contagionist, contended that this was a contagious disease. He afterwards retracted his opinions, and begs the forgiveness of the friends of science and humanity if the publication of his former opinions had had the effect of increasing the miseries attendant upon the disease; and, indeed, he says, such is the pain he feels in recollecting that he ever entertained or propagated the opinion of its being contagious, that it will always lie heavy at his heart, and deprive him of the pleasure that he might otherwise have derived from a review of his attempts to fulfil the duties of his public station, although, he says, he was misled by Dr. Lining, and other writers. "I am aware," he says, "of the influence which changes in opinion have upon a medical man's reputation, but still I would cheerfully make a sacrifice of that kind, could it avert the evils which are connected with the belief of its being a contagious disease." The mode in which a false belief in contagion does harm, is by preventing the healthy from attending upon the sick, by stopping commercial intercourse, and by allowing the patients to remain in the unhealthy spot which really causes the disease.

Dr. Stevens's hypothesis.—Now it is impossible for us living here to know any thing about these matters, for there is as much evidence on the one side as on the other. In the East Indies it is never thought to be contagious, but in the West Indies it is so considered by a large number of persons. The fever in Gibraltar gives rise to a diversity of opinion, and there are as many protestations as to its being contagious as there are as to its arising from malaria. Dr. Stevens thinks that he has discovered the reason of all these discrepancies of opinion. He says there are three distinct yellow fevers in the western hemisphere. In the first place, there is one in which there is no contagion at all, which is a climate fever, and arises simply from excessive heat; that it occurs to new comers, who are operated upon violently by the intensity of the temperature; that great bilious symptoms arise, but that there is nothing contagious in it. He contends, next, that there is the remittent fever, the yellow fever, arising from the local cause of malaria and heat together, which is seen by every body, and acknowledged to exist by most. Besides that, he says there is a yellow typhus fever—a contagious fever, which he has been able to trace to the negroes in America who have come from Africa: it is a contagious disease, attended with yellowness of the skin, and brought there by the Africans. He says that all these three fevers are attended with yellowness of the skin. What his observations are, or how numerous they may be, from which he has drawn these conclusions, I cannot tell. He says that the symptoms of these are all different at the first, but that after a time they are all the same; and that although the general treatment of the whole is the same, yet that in their minutiæ they all differ; they all require a modification of treatment. I cannot say more upon this subject—I give no opinion upon it, but I should think it likely that it is the case; that there are two or three different kinds of fever, but the intensity of the heat occasions them all to be characterized by great yellowness of the skin, some by a depraved state of the blood poured forth under the skin, and others by jaundice. I have no doubt but that Dr. Stevens's facts will be numerous, and that many of his opinions will be found to be well established. I believe that the greatest authorities in America are satisfied that the yellow fever, as it prevails there, is, for the most part, not contagious; but that it is sometimes contagious, one would think appears very certain.

I made a note of a conversation which I had with Dr. Stevens the other night, for the purpose of communicating it to you. He mentioned one instance of yellow fever occurring in a family situated a great distance across the sea from any place where the disease had prevailed. The captain of a vessel was in a port where yellow fever prevailed, and being continually among the people, his clothes and his linen were impregnated with it, and he left the place. I am not sure whether he had had the disease

himself, but he had been in the midst of those who laboured under it. When he left he cheated the quarantine, and went to another place, where no fever had prevailed, with his linen unwashed. According to the quarantine regulations, his linen should have been washed; but I understand that, under under those regulations, washing is very dear, and that every thing is charged double. He, therefore, to save this double expense, contrived to take some of his dirty linen with him, satisfied that the disease was not contagious. The linen was taken out by his family in this place where yellow fever had not prevailed, and in a very short time one died, and another died, of the sisters and maids who washed it for the captain, and an old woman, who lived in another part and came to help them, was also seized with the disease, and it spread from house to house all through the town, and produced very great devastation. Dr. Stevens says, that he knows it was the contagious form; that, in fact, it was what he calls *African typhus.* He took a voyage for the purpose of inquiring into the circumstances. The sisters said that it arose from the dirty linen; and it appeared that he was able to trace it with the greatest accuracy. He also mentioned his opinion that cholera was contagious, but he is satisfied that the contagion is not efficient at first; that you may be exposed to a person labouring under African typhus, as he calls it, and that you may be exposed to a person labouring under cholera, without any fear of catching it, while you are in his first emanations; but that if you be exposed to these after they have passed from him many days, then you stand a chance of getting the disease; that what proceeds from the body is not at first efficient, but undergoes a change, which enables it to produce the disease; that it is not a perfect poison when it is first produced. He reasons in this way: that many persons who have visited cholera patients have escaped, but they have suffered as soon as they have been exposed to fomites, in which the secretion was contained. How that may be, I do not know. He also says that he has ascertained another circumstance, that, before the disease breaks out, the blood will shew the disease. He says that this may also be remarked in scarlet fever. A medical man found that he was ill, and he was then bled. The blood was shewn to Dr. Stevens, who found that it would not turn red with the usual neutral salts. He stated that he was satisfied the gentleman was about to have a contagious disease, and, in three or four days after this, scarlet fever occurred. He says that he has seen yellow fever in the blood before the disease appeared; and that, in all instances where the disease subsequently appeared, he found the blood was of such a character that it would not redden on the application of a mixture of neutral salts. Of course, all these things are exceedingly crude at present; but it is the opinion of many eminent chemists, that we are upon the eve of some important discoveries with regard to the nature of all these matters; that great light will be thrown upon the animal changes which take place pathologically in the various fluids of the system, and on the nature of many diseases, by the treatment which Dr. Stevens has recommended. I feel satisfied with the truth of much that has been said, and I feel satisfied that a strong and new light will be thrown upon a number of these matters, respecting which we are all at present in the dark.

The latent period of this yellow fever is thought to be from two to ten days.

Treatment.—With regard to the treatment, the best writers agree that there should be copious venesection in the first instance. Whether it be the fever of the western hemisphere, or the mere bilious remittent of the east, cold affusion is always serviceable, or cold ablution. Great attention should be paid to any local inflammation that may occur, and local bleeding may be demanded. As to emetics, they cannot be employed if there be any tenderness of the epigastrium on pressure. Mercury is found useful in the first instance, but not afterwards. When you have symptoms of violent inflammation it is very useful, but still a person may die with his mouth sore, if it be given too late, or a proper degree of bleeding is not practised. Of course moderate purging is necessary; but as soon as the remission occurs, sulphate of quinine should be poured in in great abundance; and Dr. Stevens says, that when the symptoms of typhus fever come on, after venesection, and mercury, and cold affusion have been freely had recourse to, no time should be lost in pouring in neutral salts. He says he has great evidence to shew, that where these fevers have proved fatal to a frightful extent, the mortality has been reduced almost to nothing by practitioners having listened to his advice. By giving neutral salts, as the carbonate of soda, the oxymuriate of potass, and common salt, every hour during the disease, the beneficial effect has been infinitely beyond what any one could have anticipated. I know that this gentleman states nothing as a fact but what is strictly true. If he merely give an opinion, of course, like other men, he may be wrong; but upon whatever he states as a fact, you may place the greatest reliance; and if he say that a certain number of cases have been cured out of the whole number, you may depend upon it that they have been cured.

Sol-lunar Influence.—Before I conclude the subject of these fevers in hot countries, I must mention that in the East Indies

the sun and moon appear to have a great influence upon them. Dr. Balfour has written a work upon what he has called the sol-lunar influence, and he proves that at new and full moon bilious remittent fever is most easily taken; that at new and full moon relapse is most common; that at new and full moon the intensity of the disease is greatest; and that those suffering from pernicious fever, which has destroyed the constitution, are particularly affected at that period. This may appear incredible or whimsical, but I have no doubt of the fact. I have seen medical men who have practised in the East Indies, and they say it is well enough for us not to believe it, but they have suffered enough to know that the matter is perfectly true. I do not suppose that it is to be ascribed to the direct influence of the sun and moon upon the body, but that it arises from the operation of these upon the tides. The new and full moon, with respect to the elevation of the water, may have very great effect; and that, again, may have an effect, by causing more or less malaria to be disengaged, so that the atmosphere is more impregnated with it at those periods than at others. I believe that there can be no doubt whatever on the subject, that at new and full moon in the East Indies fevers are most easily taken, relapse is most common, fevers are most intense, and those who have suffered most from these fevers feel a strange sensation about them, just such as we continually find here when the east wind blows upon a person who has previously laboured under ague.

I forgot to say that many epidemics described in ancient books as plagues and pestilences, were really remittent fevers.

ON THE STRUCTURE OF THE HUMAN PLACENTA, AND ITS CONNEXION WITH THE UTERUS.

By Robert Lee, M.D. F.R.S &c.
Physician to the British Lying-in Hospital.

(From the Philosophical Transactions.)

In the year 1780 Mr. John Hunter presented a paper to the Royal Society, in which he laid claim to the discovery of the true structure of the placenta and its communication with the vessels of the uterus. The following is the history of the appearances which he observed in the dissection of a woman who had died undelivered near the full term of utero-gestation, and from which appearances his conclusions were drawn respecting the natural structure of these parts. The veins and arteries of the uterus having been injected, an incision was made through the parietes, at the anterior part where the placenta adhered to the internal surface. Between the uterus and placenta lay an irregular mass of injected matter, and from this mass regular pieces of the wax passed obliquely between it and the uterus, which broke off, leaving part attached to that mass; and on attentively examining the portions towards the uterus, they plainly appeared to be a continuation of the veins passing from it to this substance, which proved to be the placenta. Other vessels, about the size of a crow-quill, were seen passing in the same manner, although not so obliquely. These also broke on separating the placenta and uterus, leaving a small portion on the surface of the placenta; and on examination they were discovered to be continuations of the arteries of the uterus. The veins were next traced into the substance of what appeared placenta; but these soon lost the regularity of vessels, by terminating at once upon the surface of the placenta, in a very fine spongy substance, the interstices of which were filled with yellow injected matter. He then examined the arteries; and tracing them in the same manner towards the placenta, found that, having made a twisted or close spinal turn upon themselves, they were lost on its surface.

On cutting into the placenta, he discovered in many places of its substance yellow injection, and in others red, and in many others these two colours mixed. The substance of the placenta, now filled with injection, had nothing of the vascular appearance nor that of extravasation, but had a regularity in its form which showed it to be naturally of a cellular structure, fitted to be a reservoir for blood.

From these appearances Mr. Hunter infers, "that the arteries which are not immediately employed in conveying nourishment to the uterus go on towards the placenta, and proceeding obliquely between it and the uterus, pass through the decidua without ramifying. Just before entering the placenta, after making two or three close spiral turns upon themselves, they open at once into its spongy substance, without any diminution of size, and without passing behind the surface, as above described.

"The veins of the uterus appropriated to bring back the blood from the placenta, commence from this spongy substance by such wide beginnings as are more than equal to the size of the veins themselves. These veins pass obliquely through the decidua to the uterus, enter its substance obliquely, and immediately communicate with the proper veins of the uterus. This structure of parts points at once to the nature of the blood's motion in the placenta. The blood detached from the common circulation of the mother moves through the placenta of the fœtus, and is then returned back into the course of the circulation of the mother, to pass on to the heart*."

Dr. William Hunter's description of the vascular connexion between the uterus and placenta coincides with that of his brother; "for it seems incontestible (he observes) that the human placenta, like that of the quadruped, is composed of two distinct parts, though blended together; viz. an umbilical, which may be considered as a part of the fœtus, and an uterine, which belongs to the mother; that each of these parts has its peculiar system of arteries and veins, and its peculiar circulation, receiving blood by its arteries and returning it by its veins; that the circulation through these two parts of the placenta differs in the following manner:—in the umbilical portion the arteries terminate in the veins by a continuity of canal, whereas in the uterine portion there are intermediate cells, into which the arteries terminate, and from which the veins begin†."

It is a singular fact, that these celebrated anatomists should both have asserted their claims to the merit of what they supposed to be the discovery of the true structure of the human placenta, and its connexion with the uterus, and that their controversy on this subject should have loosened those bonds of affection which had united them together from their earlier years‡.

Noortwych, Rœderer, and Haller, had previously investigated this subject, by injecting the blood-vessels of the gravid uterus: their researches, however, did not determine, in a satisfactory manner, that a vascular connexion exists between the uterus and cells in the placenta. The opinions of the Hunters were generally acquiesced in at the time they were promulgated, and their accuracy has not been called in question by any anatomist of reputation in this country for the last forty years.

In the communication which I have now the honour of presenting to the Royal Society, I propose to describe certain appearances which I have observed in the examination of six gravid uteri, and many placentæ expelled in natural labour, which seem to demonstrate that a cellular structure does not exist in the placenta, and that there is no connexion between this organ and the uterus by great arteries and veins.

If an incision be made through the parietes of the gravid uterus, where the placenta does not adhere, the membrana decidua will be observed lining the internal surface, and numerous minute blood-vessels and fibres passing from the inner membrane of the uterus to the decidua. At the circumference of the placenta, the membrana decidua separates from the chorion and amnion to pass between the uterus and placenta, and thus forms a complete membranous septum, which is interposed betwixt these organs. The chorion and amnion cover the fœtal surface of the placenta; and between these two membranes and the decidua lie the ramifications of the umbilical vein, and arteries subdivided to an almost indefinite extent, and connected together by white slender filaments running in various directions. The placenta thus consists solely of a congeries of the umbilical vessels, covered on the fœtal surface by the chorion and amnion, and on the uterine surface by the deciduous membrane, and enclosed between these membranes; it adheres to the fundus, or some part of the uterus, by innumerable flocculent fibres and vessels.

On detaching the placenta carefully from the uterus, the deciduous membrane is found to adhere so closely to the umbilical vessels which it covers, that it is impossible to remove it without tearing these vessels. With the fibres uniting the placental decidua to the uterus, are mingled numerous

* Observations on certain Parts of the Animal Œconomy, by John Hunter, 1786, page 127.

† Anatomical Description of the Gravid Uterus and its Contents, by the late W. Hunter, M.D. London, 1794, page 48.

‡ Their letters are preserved in the Archives of the Royal Society.

small blood-vessels, proceeding from the inner membrane of the uterus to the decidua; and these vessels, though more numerous at the connexion of the placenta with the uterus, exist universally throughout the whole extent of the membrane. There is no vestige of the passage of any great blood-vessel, either artery or vein, through the intervening decidua, from the uterus to the placenta; nor has the appearance of the orifice of a vessel been discovered, even with the help of a magnifier, on the uterine surface of the placenta. This surface of the placenta, deprived of the deciduous membrane, presents a mass of floating vessels, its texture being extremely soft and easily torn; and no cells are discernible in its structure, by the minutest examination.

At that part of the surface of the uterus to which the placenta has been adherent, there are observable a great number of openings leading obliquely through the inner membrane of the uterus, and large enough to admit the point of the little finger: their edges are perfectly smooth, and present not the slightest appearance of having been lacerated by the removal of the placenta. In some places they have a semilunar or elliptical form, and in others they resemble a double valvular aperture. Over these openings in the inner membrane of the uterus, the placenta, covered by deciduous membrane, is directly applied, and closes them in such a manner that the maternal blood, as it flows into the uterine sinuses, cannot possibly escape either into the cavity of the uterus or into the substance of the placenta. The above appearances on the inner surface of the uterus have been accurately represented by Rœderer.

When air is forcibly thrown either into the spermatic arteries or veins, the whole inner membrane of the uterus is raised by it; but none of the air passes across the deciduous membrane into the placenta, nor does it escape from the semilunar openings in the inner membrane of the uterus, until the attachment in the deciduous membrane to the uterus is destroyed. There are no openings in the deciduous membrane corresponding with these valvular apertures now described, in the internal membrane of the uterus.

If a placenta be examined which has recently been separated from the uterus in natural labour, without any artificial force having been employed, its surface will be found uniformly smooth, and covered with the deciduous membrane; which could not be the case did any large vessels connect it with the uterus. The placenta, in a great majority of cases, is also detached from the uterus after labour, with the least imaginable force; which would be impossible, if a union by large blood-vessels, possessing the ordinary strength of arteries and veins, actually existed. Besides, a vascular connexion of such a kind would be likely to give rise, in every case, to dangerous hæmorrhage subsequent to parturition, a circumstance not in accordance with daily experience.

Noortwych, Rœderer, Haller, Dr. W. and Mr. J. Hunter, and Dr. Donald Monro, do not appear to have examined the gravid uterus and its contents in the natural state of the parts, but after fluids had been forcibly injected into the hypogastric and spermatic arteries. The laceration of the deciduous membrane covering the orifices of the uterine sinuses followed this artificial process, as well as the formation of deposits of injection in the vascular structure of the placenta, giving rise to the deceptive appearance of cells. That this took place in the examinations made by Rœderer * and Monro †, does not admit of dispute; and the following facts render it more than probable that the Hunters were also misled, by the effects of artificial distention of the placenta, from the extravasation of the fluids forced into the uterine vessels.

In the course of last autumn, the preparations of the gravid uterus in the Hunterian Museum at Glasgow, were examined at my request by Dr. Nimmo; and in none of them does it appear certain that any great blood-vessels pass from the uterus into cells in the placenta; but in many the deposits of injection, causing the appearance of cells, were observed evidently to be the result of extravasation. No preparation in the collection seems to have been expressly made for the purpose of proving or disproving the fact that the deciduous membrane passes over the uterine surface of the placenta; but in reference

* Icones Uteri humani, Observationibus illustratæ. J. G. Rœderer, 1759.

† Essays and Observations, Physical and Literary, read before a Society in Edinburgh, 1754. Vol. i.

to preparation R. R. No. 139, it is observed by Dr. Nimmo that no vascular openings are visible in the membrane interposed between the uterus and placenta.

No. 178 " is a small section of the uterus, with the veins injected green, and broken off where they were entering the placenta." The surface of the injected matter is smooth; the edges of the openings defined, and quite unlike ruptured vessels; their form in general elliptical, seeming as if they were holes cut in the side of a convolution.

No. 125. " A portion of uterus and placenta, the latter injected from uterine vessels." There is an opening which seems to be natural, corresponding to one of those in the uterus; but the majority of those whereby the injection has passed into the placenta, seem to be mere lacerations.

No. 101. " A section of uterus with veins injected black, and the injected matter protruding by irregular plugs into the cavity of the uterus." The holes are semilunar and elliptical, with defined edges, and nothing resembling the continuation of vascular tubes to be seen.

R. R. 121, is described in the printed catalogue as follows: " A small portion of placenta and uterus, where the cells of the placenta have been injected from the veins of the uterus. The veins are seen very large, entering the substance of the placenta."

Dr. Nimmo makes the following observations on this specimen: " This preparation seems to be most in point. I would describe it differently. The cellular substance of the placenta has certainly been filled from the uterine vessels. These, however, instead of passing directly into the placenta, are distinctly seen applying their open mouths to the membrane of the placenta, where the injection in some instances stops. The membrane is thinner here than where no vessels are applied, consisting, so to describe it, of one layer, while a second layer covers all other parts. Where the injection is passed into the substance of the placenta, it has evidently been forced to the side between the layers, and found some weak point, whereby it has entered into and been diffused throughout the cellular texture of the placenta*."

In the Museum of the Royal College of Surgeons of London, there is a preparation of the uterus with the placenta adhering to the inner surface, which is supposed to have been put up by Mr. Hunter himself nearly fifty years ago. The vessels both of the uterus and placenta have been filled with injection, and the parietes of the uterus, placenta, and membranes, have all been divided by a vertical section into two nearly equal portions. By permission of the Board of Curators, I have been enabled to examine one of these portions, and to have a drawing of it made. In the interstices of the muscular fibres I observed the veins of the uterus, which ran in great numbers towards the part where the placenta adhered. They were of an oval form, their long axes being in the long axis of the uterus. The muscular fibres ran longitudinally from the fundus to the os uteri.

The deciduous membrane was every where covered with minute, tortuous blood-vessels, proceeding from the inner surface of the uterus, and filled with injection. There was no appearance of vessels of any magnitude passing between the inner surface of the uterus and placenta; but flattened portions of injection were observed in this situation, having in many parts the form of thin layers, which had obviously escaped from the orifices of the uterine veins. Elsewhere the injection had lacerated the deciduous membrane, and formed deposits in the vascular part of the placenta.

The facts which have now been stated warrant, I think, the conclusion, that the human placenta does not consist of two parts, maternal and fœtal; that no cells exist in its substance; and that there is no communication between the uterus and placenta by large arteries and veins. The whole of the blood sent to the uterus by the spermatic and hypogastric arteries, except the small portion supplied to its parietes and to the membrana decidua by the inner membrane of the uterus, flows into the uterine veins or sinuses, and after circulating through them, is returned into the general circulation of the mother by the spermatic and hypogastric veins,

* My friend Samuel Broughton, Esq. F.R.S., during a recent visit to the Hunterian Museum at Glasgow, examined the preparations of the placenta and uterus at my request, and authorizes me to say that his observations fully confirm the accuracy of Dr. Nimmo's statements.

without entering the substance of the placenta. The deciduous membrane being interposed between the umbilical vessels and the uterus, whatever changes take place in the fœtal blood must result from the indirect exposure of this fluid, as it circulates through the placenta, to the maternal blood flowing in the great uterine sinuses.

Since the preceding paper was forwarded to the Secretary of the Royal Society, the following valuable communication has been received by the author from Mr. Owen, to whom portions of the gravid uterus and placenta were submitted for minute examination:—

"Lincoln's Inn Fields,
17th November.

"My dear Sir,—During the time you were examining the Hunterian preparation of the uterus and placenta in the Museum of the Royal College of Surgeons, your observations on the obscurity produced by the extravasated injection led me to think of some less objectionable mode of demonstrating the vascular communication between the uterus and placenta, if it existed; or of proving more satisfactorily than the appearances you pointed out in that preparation seemed to do, that there was no such communication.

"You have since afforded me the means, through the kindness of Mr. Alex. Shaw, of examining, in the manner I wished, the anatomical relations between the placenta and uterus. This has been done by dissecting the parts under water before disturbing them, either by throwing forcibly foreign matter into the vessels, or by separating the placenta from the uterus, to observe the appearances presented by the opposed surfaces,—a proceeding which if done in the air is liable to the objection of the possibility of having torn the vessels which were passing across, and the coats of which are acknowledged, by those who maintain the existence of such vessels, to be extremely delicate.

"The mode, therefore, which was adopted to avoid these objections, was to fix under water, in an apparatus used for dissecting mollusca, &c. a section of the uterus and placenta, and, commencing the dissection from the outside, to remove successively and with care the layers of fibres, and trace the veins as they pass deeper and deeper in the substance of the uterus in their course to the deciduous membrane; in which situation, as the thinnest pellicle of membrane is rendered distinct by being supported in the ambient fluid, I naturally hoped in this way to see the coats of the veins continued into the deciduous membrane and placenta, and to be able to preserve the appearance in a preparation, if it actually existed in nature. But in every instance, the vein, having reached the inner surface of the uterus, terminated in an open mouth on that aspect; the peripheral portion of the coat of the vein, or that next the uterus, ending in a well-defined and smooth semicircular margin, the central part adhering to, and being apparently continuous with, the decidua.

"In the course of this dissection, I observed that where the veins of different planes communicated with each other, the central portion of the parietes of the superficial vein invariably projected in a semilunar form into the deeper-seated one; and where (as was frequently the case, and especially at the point of termination on the inner surface) two or even three veins communicated with a deeper-seated one at the same point, these semilunar edges decusssated each other, so as to allow only a very small part of the deep-seated vein to be seen. I need not observe to you how admirably this structure is adapted to ensure the effect of arresting the current of blood through these passages, upon the contraction of the fibres with which they are every where surrounded.

"On another portion of the same uterus and placenta, (which were removed from a woman who died at about the fifth month of utero-gestation,) I commenced the examination under water by turning the placenta and deciduous membrane from the inner surface of the uterus. In this way, the small tortuous arteries that enter the deciduous membrane were readily distinguishable, though not filled with injected matter; and as it was an object to avoid unnecessary force in the process of separation, they were cut through, though they are easily torn from the decidua. But with respect to the veins, they invariably presented the same appearances as were noticed in the first dissection, terminating in open semicircular orifices, which are closed by the apposition of the deciduous membrane and placenta. This membrane is, however, certainly thinner opposite these

orifices than elsewhere; and in some places appeared to be wanting, or, adhering to the vein, was torn up with it; but in these cases the minute vessels of the placenta only appeared, and never any indication of a vascular trunk or cell commensurate with the size of the vein whose terminal aperture had been lifted up from the part.

"The preparation which accompanies this letter shows the termination of a vein on the inner surface of the uterus, and an artery of the decidua cut through, with the corresponding appearances on the surface of the placenta; also the valvular mode in which the veins communicate together in the substance of the uterus.

"I remain, yours very truly,
"RICHARD OWEN."

SINGULAR CASE OF FUNGUS CEREBRI, FROM A GUN-SHOT WOUND.

To the Editor of the London Medical Gazette.

SIR,

I AM not aware that any example of fungus cerebri is on record, in which the protrusion took place from the base of the skull, and came through the wound, from so distant a source as in a case which I have lately met with. If you think it deserving of a place in the Gazette, it is at your service.

I am, sir,
Your obedient servant,
CÆSAR HAWKINS.

31, Halfmoon-Street,
May 17, 1832.

On the 2d of May, W. P., a fine boy of eleven years of age, being in a room where there were several pistols, took down one of them and looked into it, in order to ascertain whether it was loaded; in doing which it went off, and the contents entered the face just below the right eye, making a small wound, and remaining in the head. There was considerable hæmorrhage from the wound, as well as from the nose and mouth, and from the right ear. When I saw him he was faint and cold, and in a complete state of collapse, but sensible to pain when he was touched, and he had vomited slightly. The right eye seemed to be uninjured, and the left was observed to be a good deal turned inwards to the nose, but the pupil acted readily. From the probable course of the ball, and the quantity of venous hæmorrhage from the ear, it appeared to me that the temporal bone was probably fractured, so as to rupture the lateral sinus; and the extreme degree of collapse rendered it not impossible that the ball might have gone through the bone into the cerebrum. His alarming state induced me to avoid any violence with a view to the extraction of the bullet, lest any internal hæmorrhage which might be going on should be increased: he was left, therefore, perfectly quiet, directed to be kept quite cool, and to take nothing but cold liquids.

During the remainder of this and the next day he continued in a state of restlessness, turning from one side to the other, but sensible to pain, and capable, when roused, of answering, and giving a clear account of the manner in which the accident happened. He vomited occasionally the contents of the stomach, mixed with a good deal of blood, which appeared to come from the wound, as the bleeding from the ear still continued. The circulation had risen a little, but he never required depletion, the pulse being for the most part not more than from 80 to 90, and weak. The face and eyelids were swollen, but without much tension.

On the third day he was perfectly quiet and rational, the swelling began to subside, and suppuration took place freely from the wound and from the ear. A probe passed from the wound towards the ear, to a depth of about four inches, through a quantity of splinters of bone, and at the bottom struck what appeared to be the bullet; and a probe passed also through the meatus, in the midst of similar splinters of broken bone; so that, from both examinations, about one inch inwards and forwards from the ear seemed to be the situation of the bullet. It was clear, therefore, that part at least of the temporal bone was fractured, yet he heard perfectly on this side, so that the labyrinth must still have been uninjured.

During the next three days he went on very well, without any symptoms attributable to injury of the brain. The

swelling of the face and fauces, which had produced some difficulty in swallowing and speaking, wholly subsided, and several pieces of bone, and a small portion of lead, were taken away through the wound and through the ear. On the 9th, however, he again became restless and irritable, and started much in his sleep; he complained of pain in the back and abdomen: the whole of the muscles of the trunk were in a state of spasm, so that even if his head were moved the whole body was turned with it, though, at the same time, the muscles of the legs and arms, and the lower jaw, were perfectly under his control. He afterwards became hurried in his manner, then delirious, singing and talking incessantly, and attempting to get out of bed, and at last was quite unconscious of what was passing around him. At the time these symptoms of irritation commenced, the discharge from the wound and from the ear became dark-coloured and bloody, and some blood was discharged with pieces of bone. On the evening of the 9th a protrusion was observed, apparently of cerebral matter, through the wound in the face, which increased to the size of a walnut, but was afterwards pushed off. He died on the 10th, just eight days from the accident.

It was found that the bullet had passed through the junction of the malar and maxillary bones, breaking up completely the floor of the orbit; thence it passed to the inside of the lower jaw, through the external pterygoid muscle, having so far injured the joint that both the glenoid cavity and the condyle of the jaw were denuded of cartilage and periosteum, and a little piece of the inside of the condyle was broken off. The bullet had then broken up the junction of the sphænoid and temporal bones, in the fossa between the sella tunica, the petrous portion of the temporal bone, and the lesser wings of the sphænoid. The glenoid cavity and the meatus were of course loose, and a fracture extended across the petrous portion of the temporal bone, a sharp point of bone having pricked the dura mater and entered the lateral sinus. The inner part of the petrous portion was thus detached, but the carotid artery and the jugular vein were uninjured; the ball itself, which was much flattened, being situated just below this inner part of the temporal bone, where it had been perceived by the probe.

Through the wide opening thus made in the sphænoid and temporal bones the brain protruded in considerable quantity into the cavity made by the bullet, and was in the usual vascular and pulpy state of fungus of the brain: the substance of the cerebrum around this softened part was vascular, and of the yellow colour generally found in such cases. A small layer of coagulated blood was perceptible on the surface of the right hemisphere, and upon the tentorium.

The veins of the brain were full of blood, but there were little signs of inflammation, except around the junction of the optic nerves, which were slightly covered with lymph.

It may seem doubtful whether the dura mater and the brain had been directly wounded by the bullet at the time of the accident; but, as the symptoms were at first those of concussion only, and the fatal symptoms occurred suddenly, after the boy had been gradually getting better, I think it more probable that the blow had produced contusion only in the first instance, giving rise to the small effusion of blood, which has been mentioned, and that the dura mater being bruised, and deprived of its support, had sloughed, so as to allow of sudden protrusion of the brain at the time the delirium and other signs of irritation were observed. And it seems probable, that, but for this protrusion, the boy might have got well, as he had recovered from the concussion, and had no inflammation, the pulse being soft and natural, and only 80, just before the protrusion took place, and the wound itself being healthy, and allowing plenty of room for the extraction of the remainder of the bones and of the bullet.

Fracture of the bones of the cranium, allowing of the formation of fungus cerebri, has been observed in every part of the upper surface and sides of the head, even so low down as to admit of the escape of brain from the side of the middle lobe through the ear; but I am not aware of any instance in which fungus took place from the base of the brain, as far inwards as in this instance, by the side of the sella tunica; and the case seems on this account curious. It shows how necessary the support of the

cranium is to prevent fungus cerebri, as it took place through a wound of four inches in depth; and it shows also how distinct the symptoms arising from protrusion of the brain are from those of inflammation, although, no doubt, the fungus cerebri is often accompanied by inflammatory symptoms.

Another point deserving notice is the preservation of the sense of hearing, although not only the tympanum was empty, and the external ear completely separated, but the whole temporal bone so shattered that the fractured portions left not enough entire to include the whole labyrinth, but must actually have broken across one at least of the semicircular canals.

INJURY OF THE GREAT ISCHIATIC NERVE.

To the Editor of the London Medical Gazette.

Liverpool, May 14, 1832.

Sir,

I beg to enclose the particulars of a case of severe injury of the great ischiatic nerve, which has caused a good deal of speculation here as to the consequences, and of course has excited some interest. It is a case of rare occurrence, and may be considered singular, inasmuch as the patient had scarcely one bad symptom. I shall feel obliged by your giving it a place in the next number of the Medical Gazette.

Yours, very respectfully,
J. M. Banner.

I was requested on the 22d of March last to visit Robert Lunn, a coachman, aged 45, who had met with a severe accident. On my arrival at the house, I found Dr. Brandreth in attendance. We were informed that Lunn had been cleaning the drawing-room window; that his foot had slipt from its ledge; that he had fallen a distance of seven or eight yards, and had alighted on the spikes of the area railing. He is a man of full habit of body, and had hitherto been remarkably healthy.

On examination, we found two punctured wounds; a triangular one, an inch in extent each way, situated in the back part of the right thigh, about three inches below the tuberosity of the ischium; the other on the right side of the anus, the sphincter being partially divided. Both wounds were of considerable depth; there was hanging from that in the thigh a number of filaments of the ischiatic nerve, much lacerated. The patient was very restless, and complained of great pain in the thigh, with numbness which was in the calf of the leg, and more complete in the heel and sole of the foot. Dr. Brandreth and myself determined on the removal of the lacerated portion of nerve. We thought there would be much less danger in so doing than in returning it into the wound. Accordingly, it was cut away with a pair of scissors; the edges of the wound were brought together with adhesive plaister, and slight pressure made with compress and bandage. The filaments of nerve removed measured three and a half inches; they were much separated, except at the part that was divided by the scissors; here it measured in circumference near the half of an inch.

The patient was visited again in four hours; he complained of increased pain, shooting from the wound in the thigh to the loins, and down the front of the leg to the foot, with occasional spasmodic twitchings; there was an increase of numbness in the calf, heel, and sole of the foot; and his friends observed an unusual irritation in his manner. The pulse was now full and quick; (soon after the accident it was low, from loss of blood;) he had lost a considerable quantity of venous blood from both wounds, particularly from that near the rectum. Eighteen ounces of blood were taken from the arm; an aperient draught was given; and cold lotion applied over the bandages.

23d.—I found him with all the symptoms of the preceding night; he had not slept; the pain was great; the spasmodic affection and numbness continued; he had had slight rigors; the tongue was furred; the pulse continued full; there was a dark-coloured discharge from the wounds, the edges of which were puffy; and there was considerable inflammation and swelling of the surrounding skin. The blood taken yesterday was buffy, and slightly cupped. He was ordered to poultice the wounds with linseed-meal; V.S. ad ℥xiv. After the bleeding he felt faint, and much relieved; he slept occasionally during the

day; when in sleep there was twitching of the hands and distortion of the face; the bowels had been well acted on.

24th.—He had passed a restless night; complained of a sensation of burning at the root of the toes; the numbness in the calf of the leg was less, that in the heel and sole of the foot continued the same; the pain in front of the leg was much abated; he had been frequently affected during the night with spasmodic twitchings in the thigh and calf of the leg; there was a dark-coloured matter discharged from the wounds; sloughing had taken place between the rectum and wound, so as to lay the gut open three inches in extent; he was suffering from considerable irritative fever; the bowels were constipated. V.S. ad ℥xvj.

℞ Mag. Sulph. ʒiss. Inf. Rosæ, ℥iss. M. ft. Haust. 4tâ quâque horâ sumendus.

A strong opiate embrocation was ordered to be applied to the foot occasionally.

25th.—He had passed a better night; he had had very few returns of spasm since the last bleeding; the numbness continued much as at the last report; the pulse was not so full or frequent as on the preceding day; irritative fever much abated; bowels had been acted on; the wounds looked less inflamed; the discharge was of a lighter colour; the wound into the rectum was now dressed to the bottom with lint.

In the evening of this day I found him complaining of a return of pain in the leg, extending to the foot; the sole of the foot was very sore to the touch; he had a sensation of cold water trickling down the back of the leg, and felt slight numbness on the top of the foot; there was increased pain at the root of the toes, and very acute pain at the edges of the foot; he complained also of pains shooting from the wound to the bottom of the back; the pulse had risen.

V.S. ad ℥xvj.

After this bleeding he felt immediate relief. An anodyne draught was ordered for bed-time.

26th.—He had passed a restless night; he had felt an inclination for stool, but was prevented by the spasmodic affection in the calf of the leg; the sensation of pain and heat in the toes continued; pulse was quick and full; tongue slightly furred; the wounds were healing; the blood was buffy.

Continued the Draughts, with the Sulphate of Magnesia. V.S. ad ℥xij.

28th.—There has been a cessation from pain and spasm since the last bleeding; the wounds were looking clean, and dressed with a solution of the nitrate of silver; the blood was slightly buffy; pulse regular; the numbness in the calf of the leg and top of the foot nearly gone; that in the heel and sole of the foot continued much the same.

30th.—There had been little alteration since the night of the 26th, until this morning, when he complained of acute pain at the sides of the foot, so great, that on passing the finger lightly over them, he called out: his wife states, that during sleep he opened and shut his hands often, and that there was a frequent spasmodic action of the muscles of the face, as if in pain. He stated, however, that he did not feel any pain unless the foot was touched; in other respects he was much better: he was gradually regaining sensation in the sole of the foot; it was nearly perfect in the calf and top of the foot: a strong opiate embrocation was ordered for the foot.

31st.—The sensitiveness of the foot was much lessened.

April 2d.—He has progressively improved, and there has been very little deviation until to-day; he complains of languor and thirst; the pulse has fallen from 82 to 48 beats in the minute; his spirits are depressed; in consequence of this alteration, I improved his diet, and allowed a little ale during the day: he also took the following three times a day:—

℞ Inf. Cinchonæ ℥iss.
Sp. Amm. C. ʒss. M. ft. haust.

10th.—He has not had any unpleasant symptom since the last note; the pulse has become regular, at 74; he now leaves his bed, and is able to walk without much difficulty; he states that his feelings in the affected leg are very different to those in the sound one; it feels much heavier, and not so firm; that, in walking, if he places his foot upon a raised stone, or uneven ground, it causes great pain to shoot from the

foot to the knee; the sense of feeling is nearly restored to every part, with the exception of the sides of the heel; here it remains very slight; the wounds are healing fast.

30th.—The wounds are quite healed, and he has resumed his duties as a coachman: he has not a proper use of the limb; it still feels heavier and less firm than its fellow; the sense of feeling is returning to the heel; in the other parts it is quite perfect.

May 13th. — Lunn visited me this morning; he is each day gaining a more perfect use of the leg; the sensation is much more perfect in the heel; in other respects he remains quite well.

May we not infer that at least one-half the diameter of the ischiatic nerve must have been divided, and that three inches and a half in length of those filaments which form the tibial nerve, must have been cut away? It will be observed, from the foregoing symptoms, that the parts to which the tibial nerve goes were numb from the time of the accident, and that numbness came on the top of the foot on the third day: this led me at first to suppose that the filaments which were removed might have composed, in part, both the tibial and fibular nerves; this latter symptom, however, may have been caused by pressure on the nerve, from effusion by inflammation, near the wound, which at that time was considerable.

I was induced to adopt the antiphlogistic treatment from a supposition that there was inflammation of the nerve existing, by the symptoms of spasmodic twitchings and excruciating pains that were felt soon after the injury. These symptoms, however, may have been merely sympathetic; my greatest fear was in their terminating in tetanus, and consequently was anxious to reduce all symptoms of inflammation.

Is it not possible to account for the eturning sensation (which began to take place in so short a time as the third day after the accident) from the anastomosis which takes place between the tibial and fibular nerves; and by continuing sensation to the parts supplied by the tibial nerve, by means of the connecting influence, which might thus supply that nerve with its former power?

Or can the return of sensation be accounted for by the cutaneous branches of the ischiatic?

PARALYSIS OF THE FACE.

To the Editor of the London Medical Gazette.

Sir,

Examples of partial paralysis of the face are not, according to my observation, of very frequent occurrence. The subjoined case, which is of that description, furnishes additional pathologic evidence of the functions of the "respiratory nerve of the face," as established by the experiments and observations of Sir C. Bell. The seat and cause of disease were easy of detection, and the symptoms yielded to obvious treatment. If the case appear to you worthy of being laid before the readers of the Gazette, I will thank you to insert it when convenient.

I am, sir,
Your obedient servant,
R. Arrowsmith, M.D.

Coventry, April 15, 1832.

January 9, 1832.—W. Hyde, æt. 12, states, that six weeks since an abscess formed within the right eye, attended with great pain, loss of hearing, and fever. About a fortnight previously he had fallen into the water, and thus taken cold. The pain in the internal ear was relieved by the discharge of matter, but the cheek and eye of that side have since remained in the state now to be described. That side of the face generally, but the mouth in particular, is drawn, as in a paralytic person, very much towards the left side; he cannot whistle, nor can he smell through the right nostril. In laughing, the right side of the face remains completely tranquil. Mastication is performed equally well on either side, but on the right the food collects between the teeth and the cheek, whence he dislodges it by pressing the outside of the cheek with the fingers. Sensation is perfect. He cannot close the right eye; if requested to do so the eye is rolled upwards, so that the cornea is nearly covered by the upper eyelid. The eye appears larger, in consequence of the dragging downwards of the lower eyelid. The conjunctiva is inflamed, and the eye affected with lachrymation. At night, when in bed, he closes the eye by drawing the eyelids together, and immediately turns his face upon the pil-

low. Vision is perfect. The attempts to frown produce squinting, the pupil of the right eye being directed somewhat upwards.

There is now no discharge from the ear, but occasional pain and a "sawing noise." He has headache; sleeps well, but dreams a good deal. The pulse is natural, the tongue clean, the bowels open, and appetite good.

He was directed to apply four leeches every other day close to the ear, a blister behind it, to take calomel and jalap every other night, and to abstain from animal food and stimulating fluids.

12th. — No change, excepting that there is less pain in the ear.

16th.—The eye is less inflamed, and he can close it somewhat more. In laughing, the right side of the mouth is much drawn towards the left. He cannot whistle, nor can he smell through the right nostril, because, he says, there is "a stoppage in the head." There is no pain in the ear. The leeches and powders to be continued, and the blister kept open with savine ointment.

20th.—He can close the eye still more, and whistle imperfectly, the mouth being less drawn towards the left side. The deafness continues. He can neither frown nor elevate the eyebrows.

February 3d.—He can draw up the angle of the mouth on the right side a little, and can whistle better, and quite easily, if he hold the right angle of the mouth. He can also nearly close the eye.

22d.—The ear is free from pain and discharge; the hearing not quite perfect. He can close the eye almost completely. The face is not at all distorted; he can whistle easily, and smell perfectly through the right nostril. The blister continues to discharge.

March 15th.—The hearing is quite restored. He is free from all remains of the affection.

To the Editor of the London Medical Gazette.

Sir,

I am induced to send you the following case of partial paralysis, from its similarity in many of its symptoms, and in its result, to the case which Dr. Arrowsmith has drawn up for insertion in your journal; and if you consider it worthy of being laid before the public, I shall be obliged by your giving it a place in the Gazette.

I am, sir,
Your obedient servant,
Chas. B. Nankivell,
One of the Surgeons to the Coventry Benevolent Dispensary.

Coventry, May 15, 1832.

William Lucas, æt. 44, states that he always enjoyed good health until the latter end of March, when he became affected with headache, and a singing noise in the left ear; the bowels were at the same time rather constipated. Soon after the accession of these symptoms, he experienced a slight impediment in his speech; he could not close the left eyelid, the external canthus of which seemed lower than that of the opposite lid, and the tears ran down the left cheek. On the 11th of April, when he applied at the Dispensary, there was complete paralysis of the whole of the left side of the face. It was quite immoveable by the action of its own muscles, and the lips were only acted upon by the muscles of the opposite side, and by the right half of the orbicularis oris, which had drawn the mouth upwards, and to the right side, so that the lips were nearly in a line with the right meatus auditorius externus. The tongue could be protruded straight forwards. In laughing, and in attempting to whistle, the left portion of the lips did not touch each other, and he had considerable difficulty in the pronunciation of labial letters. Sensation is unimpaired. The left meatus auditorius is deficient in ceruminous secretion, but the sense of hearing is unaffected. The tongue is rather furred; the bowels costive; skin hot and imperspirable; urine high-coloured; pulse full, and rather hard. He was bled freely from the arm, took calomel and colocynth, with cathartic mixture, was placed upon low diet, and ordered to refrain from all employment.

April 13th.—Feels much relieved; no improvement in the state of the paralyzed muscles; pulse softer; febrile symptoms abated. Some degree of pain still remaining in the head.

Six leeches were directed to be applied below the left ear, and a blister to the nucha. Purgative medicine to be continued.

16th.—Pain in the head less; some improvement in the parts affected. The

lips can be drawn nearer together, and the external corner of the mouth and of the eyelid is not so much depressed. Still some thirst and heat of skin ; pulse soft and compressible.

℞ Mag. Sulphatis, ℥iss. Liq. Ammon. Acet. ℥iij. Antimon. Tartar. gr. ij. Aq. Menth. Sativæ, ℥v. M. Cap. Coch. iij. 4tâ quâque horâ. Sumantur Pil. ij. Hyd. Sub. et Ext. Col. c. omni nocte.

18th.—Considerable improvement in all the symptoms.

Emp. Lyttæ pone aurem sinistrum imponendum. Perstet in usu Pil. et Misturæ.

From this period he progressively recovered the use of the affected parts, and now continues in every respect convalescent.

May 11, 1832.

INJECTION OF SALINE SOLUTIONS INTO THE VEINS,

Adopted with success in Malignant Cholera.

[We have been favoured with the following curious and interesting document, addressed to the Central Board of Health.]

Sir,—I conceive it to be my duty to let you know, for the information of the Central Board of Health, that the great desideratum of restoring the natural current in the veins and arteries, of improving the colour of the blood, and recovering the function of the lungs in cholera asphyxia, may be accomplished by injecting a weak saline solution into the veins of the patient. To Dr. Thomas Latta, of this place, is due the merit of first having recourse to this practice. He has tried it in six cases ; three of which I have seen, and assisted to treat. The most wonderful and satisfactory effect is the immediate consequence of the injection. To produce the effect referred to, a large quantity must be injected, from *five to ten pounds* in an adult, and repeated at longer or shorter intervals, as the state of the pulse, and other symptoms, may indicate. Whenever the pulse fails, more fluid ought to be thrown in to produce an effect upon it, without regard to quantity. In one of the cases I have referred to, 120 *ounces* were injected at once, and repeated to the amount of 330 ounces in 12 hours. In another, 376 ounces were thrown into the veins between Sunday, at 11 o'clock A.M. and this day, (Tuesday) at 4 P.M. ; that is, in the course of 53 hours, upwards of 31 pounds !

The solution that was used consisted of two drachms of muriate, and two scruples of carbonate of soda, to sixty ounces of water. It was at the temperature of 108 or 110°.

The apparatus employed for injecting was merely one of Reid's common syringes, (the fluid being put into a vessel rather deep and narrow) with a small pipe fitted, that it might easily be introduced into an incision in the veins of the usual size that is made in bleeding. It may, however, be well to keep in mind that in the event of the operation being frequently repeated, it may be advisable to inject by different veins.

I forbear at present to enter further into the particulars, nor have we had sufficient experience to speak decisively on the subject. I may, however, mention that the idea of having recourse to this remedy in cholera occurred to Dr. Latta, from being convinced (which I am also) that the evacuations upwards and downwards are in reality the serum of the blood ; that it is the duty of the physician to replace it as speedily as possible by injecting a fluid, as similar to the serum as can be formed artificially, directly into the veins, which has been done here with wonderful, and so far as we can yet judge, excellent effect. An immediate return of the pulse, an improvement in the respiration and in the voice, an evolution of heat, an improvement in the appearance of the patient, with a feeling of comfort, are the immediate effects. The quantity necessary to be injected will probably be found to depend upon the quantity of serum lost, the object of the practice being to place the patient in nearly his ordinary state as to the quantity of blood circulating in the vessels.

I have, &c.

(Signed) Robert Lewins, M.D.

Fellow of the Royal College of Physicians, and Member of the Leith Board of Health.

Leith, 6, Quality-Street,
May 15, 1832.

To W. Maclean, Esq.
Secretary to the Central Board of Health.

Sir,—I did myself the honour to address a letter to you lately on the effect of injecting a saline solution into the veins of a patient labouring under cholera. We have not frequent opportunities now of trying this, which I denominate, admirable remedy, as the disease is decidedly less frequent here; but I have seen it employed in two other cases, in the course of the last two days, with the same excellent effect. Sixty ounces are generally thrown in at once, and repeated at the end of three or four hours. In a case to-day, where I saw fifty-eight ounces injected, (being the third time of performing the operation) the patient's pulse at the commencement was 180, very small, and very feeble. She was excessively restless, with a feeling of great weakness and tormenting thirst. Before twelve ounces were injected, the pulse began to improve; it became fuller and slower, and it continued to improve until, after fifty-eight ounces had been injected, it was down to 110. Before I left the patient (a woman) her condition was altogether amazingly ameliorated. There was a fine glow, and a slight perspiration on her face; the veins on the back of her hand were well filled; the restlessness was removed; and the feeling of excessive weakness gone, and the thirst ceased. The pulse was under 100, and free, full, and soft! Verily, sir, this is an astonishing method of medication, and I predict will lead to wonderful changes and improvements in the practice of medicine. I have addressed you upon the subject as the organ, from your high official station, of most speedily and effectually disseminating a knowledge of the extraordinary facts referred to. It will, of course, give me great pleasure to enter further into particulars upon any particular point on which you may require information in reference to the cases that have come under my observation.

I have, &c.

(Signed) Robert Lewins.

6, Quality-Street, Leith,
May 18, 1832.

W. Maclean, Esq.
Secretary to the Central Board of Health.

In the hands of a man of ordinary dexterity, the common injecting apparatus alluded to in my last will be found to answer the purpose perfectly well; but if the practice I recommend is, as I hope it will be, generally adopted, it will, I conceive, be expedient to advise that a regular and perfect transfusion apparatus be used, at all events, to warn those who inject to beware of allowing air to get into the vein. The tubes, of course, must be filled with fluid, as well as the pipe in the vein, before commencing, and considerably more fluid than it is intended to use ought to be in the vessel from which it is pumped.

R. L.

CHOLERA WITH ABORTION,

Black Discharge from Uterus becoming Florid under the Saline Treatment.

To the Editor of the London Medical Gazette.

Sir,

I am induced thus publicly to communicate a recent case of cholera successfully treated by the *saline practice*, and under circumstances which may, perhaps, render its recital not altogether uninteresting to the profession.

May 12th.—I was consulted in the afternoon by Mrs. L., æt. 42, the mother of twelve healthy children, the eldest twenty-three and the youngest three years old. Supposes herself about three months advanced in utero gestation; complains of diarrhœa of two or three days' standing, with great prostration, and cramps in her lower extremities. Pulse infrequent, and feeble.

Ordered ℞ Mist. Cretæ, f. ℥iv.; Tinct. Opii, ♏xl. capiat 4tam partem statim et repetatur post singulas dejectiones.

In the evening her daughter came to say her mother had taken all the mixture without experiencing any benefit; indeed, to her former symptoms were superadded vomiting of a violent character.

Ordered the mixture to be repeated, with the addition of Tinct. Catechu, f. ℥ss.

13th, 6 A.M.—Mixture all used, but the patient is considerably worse. Has passed a very restless night; countenance much sunk; voice peculiar, and little more than a whisper; stools fæculent; a bilious fluid was also ejected

from the stomach, and complains of a bitter taste in the mouth.

Ordered Pulv. Cretæ, C. c. Opio, ʒj.; divide in Pulv. iv. to be taken in the same manner as the mixtures were directed.

Noon.—The powders have all been taken, without the slightest improvement in any one symptom. Stools liquid, and now, for the first time, of a rice-water colour. The fluid which she vomits, however, is still bilious; tongue flabby, coated, and cold; pulse scarcely perceptible at the wrist; complains much of headache.

℞ Liq. Opii Sedat. ʒj.; Ammon. Carb. ʒj.; Syrupi Aurantii, f. ʒvi.; Aquæ, ℥v. misce, capiat Coch. ii. secundâ quâque horâ cum Acid. Tart. gr. xv. in statu effervescentiæ.

10 P.M.—No better. Some discharge from the uterus of *black blood*, with bearing-down sensations, as if about to abort.

14th, 7 A.M.—Miscarried in the night; appears to have been very correct in her calculation as to her period of gestation. Purging and vomiting not at all relieved; extremities cold; pulse gone, and appears to be fast approaching to a state of dissolution. Ordered a Seidlitz powder to be taken directly, and repeated at pleasure; also one of the following powders to be taken in twenty minutes after the Seidlitz, and repeated every hour:—

℞ Sodæ Carb. ʒss.; Sodæ Mur. ℈j.; Potassæ Oxymur. gr. vii. misce.

Noon.—Vomiting has ceased; purging less frequent; pulse begins to be perceptible, but small, slow, and tremulous; some return of heat on the surface. One Seidlitz powder has been taken; also four of the other powders; all of which have been retained. These were ordered to be continued.

10 P.M.—Nine of the above powders have now been used, and retained. Considerable re-action has taken place; voice and countenance much improved; pulse 80, and begins to be more full. *Lochia beginning to be florid in appearance*, and of the usual quantity. Only one dejection since my last visit. Powders ordered to be continued every hour. From this time she rapidly improved, and is now out of danger.

It is worthy of remark, that the eldest daughter and the husband of this woman have also been attacked with diarrhœa and cramps in the extremities, for which Pulv. Cretæ C. c Opio was given, without affording the slightest relief. After persisting in their use for some time, and as the symptoms were evidently becoming worse, recourse was had to the above saline remedies, and speedy recovery was the almost immediate result.

You will observe, sir, I was very tardy in putting these patients under the saline treatment; but I must beg to observe that this did not arise from a conviction on my part that they were not cases of cholera, for, perhaps, a better marked case than the first was never witnessed; but I was anxious to try whether, while bile continued to pass into the duodenum, the diarrhœa and vomiting could not be arrested by any other means than the saline medicines, as recommended by Dr. Stevens, which I had seen used with so much success in the prison at Cold-Bath Fields. I think, however, you will agree with me, that I gave chalk and opium (the usual remedies) a very fair trial in the above cases. I am the more particular in pointing out this, as my belief *now* is that the non-purgative alkaline salts are, as Dr. S. asserts, more useful in relieving the sickness at the stomach, and checking the diarrhœa, than common astringent or absorbent medicines. I may also observe, that these cases shew that the rice-water evacuations are not invariably present in the early stage of the Indian cholera; and from what I have seen of this malignant disease, though I believe that no treatment will be successful in every case of collapse, yet my thorough conviction is, that a much greater number of patients will be saved by the saline treatment than by any other practice that has yet been tried.

I have been the more induced to communicate the above facts, as I observe that others (even the Central Board of Health) are still recommending the use of medicines which have been long used in cholera, fairly tried, and found to be not only useless but actually injurious.

I am, sir,
Your obedient servant,
HENRY WHITMORE.

Cold-Bath Square, May 21, 1832.

I may add, that on last Sunday night,

just before midnight, I was called on to attend another female, who resides in the same parish, and at no great distance from the above patients. This woman had been suffering for two or three days from vomiting and purging, but when I saw her, for the first time, she was in a state of collapse. She was also exceedingly emaciated from previous bad living. She was immediately put under the saline treatment; re-action soon came on, with general amendment in all the symptoms. She has since continued to improve; the kidneys are again acting, and I have now great reason to hope that she may recover. She is, however, in a very low state, not merely from the effect of the disease, but also from previous ill health. She is suffering also from great mental anguish, for the loss of two children, one of whom had died on Saturday and the other on Sunday, the same day that she was herself attacked. I did not see either of these children, but both of them were reported as having died from confirmed cholera.

Thursday evening, 9 o'clock.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Critical and Experimental Essay on the Circulation of the Blood; especially as observed in the Minute and Capillary Vessels of the Batrachia and of Fishes. By MARSHALL HALL, M.D. F.R.S.E. M.R.I. M.Z S. &c. &c.

DR. MARSHALL HALL, dissatisfied with the investigations of preceding inquirers on the subject of the minuter portions of the circulating apparatus, lately armed himself with one of Dolland's achromatic microscopes, and investigated the transparant parts of various animals with great care, and we must also say with novel and interesting results. Among the principal objects which engaged our author's attention, may be enumerated the minute anatomy of the capillaries in the extremities and in the lungs of some of the batrachia—the powers which circulate the blood—the influence which the brain and spinal marrow have upon the circulation—the influence of other organs on the heart and circulation. We have one or two additional points of inquiry, or description; and among the latter is an account of the caudal heart of the eel, which Dr. Hall has also minutely represented in an engraving, which exhibits this curious provision with extreme fidelity. The delineations, indeed, of the different parts, as they appear through the microscope, are excellent: we speak as eye witnesses, having seen most of them displayed in this manner. The physiologist who fails to make himself acquainted with this essay, will remain in ignorance of some of the most interesting observations which have hitherto been made on the subject to which they relate. Some idea of their importance may be gathered from the subjoined epitome of the matters discussed:—

1. The distinction between the ultimate minute arteries, the true capillaries, and the first roots and minute trunks of the veins;

2. The successive divisions of the minute arteries, the continual conjunctions and redivisions of the capillaries, and the successive conjunctions and occasional anastomoses of the veins;

3. The characteristic rapid flow of the blood along the arteries, and its retarded flow along the capillaries and veins;

4. The singular differences in the form and distribution of these vessels in the systemic and pulmonary systems; especially,

5. The more abrupt divisions of the arteries, the more crowded number of the capillaries, and the abrupt formation of the veins, in the latter;

6. The extensive power of the heart in the circulation, the irritability of the arteries, the want of evidence of irritability in the true capillaries, and the effect of the respiratory and other muscular motions upon the course of the blood along the veins;

7. The doubt whether the true capillaries be real vessels or mere canals;

8. The temporary independence of the action of the heart and of the minute and capillary circulation, of the brain and the medulla oblongata and spinalis;

9. The power of the heart to continue the circulation in the minute and capillary vessels, after its entire removal from the body, in opposition to the opinion of Legallois;

10. The independence of the circulation of respiration and of that part of the medulla on which respiration depends, in opposition to the opinion of M. Flourens;

11. The independence of the capillary circulation of a part, upon that part of the spinal marrow from which it derives its nerves, in opposition to the opinion of Legallois and the original opinion of M. Flourens;

12. The extraordinary difference of removing the brain and medullæ, at once, and in successive portions at distinct intervals;

13. The erroneous mode of explanation of this fact, given by Legallois; another suggested;

14. The temporary independence of the circulation in the minute and capillary vessels, of the entire nervous masses, brain, medullæ, and gangliæ;

15. The effect of opium and alcohol upon the batrachia;

16. The effect of alcohol applied to the brain and spinal marrow, upon the action of the heart and the circulation;

17. The effect of crushing the brain and spinal marrow; compared with

18. The effect of crushing other organs or parts, upon the circulation;

19. The general sympathy of these different organs proved by these experiments; and

20. The want of any physiological deduction as to the natural functions of the parts themselves individually;

21. The effects of irritants applied to the web, upon the vessels which pass between its membranes;

22. The impossibility of forming any deduction from this experiment, upon the nature and function of the true capillaries;

23. The singular phenomenon of a caudal heart or ventricle in the eel;

24. The test of muscular structure afforded by water of temperatures moderately higher than that of the blood.

CAUTION TO DISPENSERS OF MEDICINE.

On Wednesday last a verdict of manslaughter was found against the assistant of a chemist at Brighton, for dispensing a prescription by which Captain Burdett, of the Royal Navy, lost his life. It appears that oil of tar was substituted for a black dose.

MEDICAL GAZETTE.

Saturday, May 26, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—Cicero.

THE LATE BARON CUVIER.

While France and learned Europe are still engaged in deploring the loss of this illustrious man, and in pronouncing splendid eulogiums upon his memory, we shall take the opportunity of paying our simple tribute, in the shape of a summary of his life and labours: even the briefest record of these must constitute no mean monument of his fame.

George Cuvier was born at Montbelliard, in the year 1769. Yet he was, strictly speaking, not a Frenchman. Montbelliard, at the period of his birth, and for more than a quarter of a century after, belonged to the Duchy of Wurtemburg; it was eventually included by republican France in the newly-formed department of the Upper Rhine. The father of M. Cuvier was a Swiss: after serving for forty years in the French armies, and attaining the rank of a chevalier, he retired on a pension as Commandant of Artillery at Montbelliard.

The capital of Wurtemburg was chosen as the scene of young Cuvier's education: at Stuttgard he devoted himself to the study of mathematics, philosophy, law, and jurisprudence; and it deserves to be noted, that the chief object of the courses laid down for him was to qualify him for a place in the administration, for his family is said to have had some interest with, and some claims on, the reigning Duke Charles. But the French Revolution produced great changes. We find our student suddenly transferred from Stuttgard to the coast of Normandy. At Stuttgard he is known to have de-

rived, from the mountainous scenery and other local peculiarities, many of those impressions which were afterwards so admirably developed in his maturer views of natural history; and at Stuttgard too he had tried his strength in the various learned societies which were found there. Removed now to his Norman residence, in the 20th year of his age, as a tutor in the family of a French nobleman, he had his first opportunity of indulging in those favourite pursuits which ultimately stamped him as perhaps the greatest naturalist that the world ever saw. It was not long till he had dissected, described, and drawn, almost all the fishes that were to be met with on the coast of La Manche; and here it was that he made his grand and original observations on the structure of the molluscous tribe.

We next find him in Paris, engaged as a private lecturer. He was soon appointed as an assistant to Professor Mertrud, in the courses of comparative anatomy at the Museum of Natural History; and immediately admitted a member of most of the learned societies of the French metropolis. Of the Institut, which was organized in 1796, he was one of the earliest members; and the numerous memoirs and discoveries which he laid before that body, rapidly extended his reputation, while they formed the bases of many of his subsequent publications. Before he was 29 he received the Chair of Natural History at the Ecole Centrale; and in 1798 published his first separate work—the *Tableau Elementaire de l'Histoire Naturelle des Animaux.* This was the basis of the *Règne Animal;* it presented a new arrangement of the whole animal kingdom—the principles of which, however, had been already given by him in a memoir on a new division of the white-blooded animals, written at the age of 26, when *he first* ventured to attack Linnæus's division of the invertebrata into the meagre and confused classes of insects and worms. His own division, at the same time it should be noticed, of the invertebrated animals into the three now well known classes, was not adopted by him until the year 1812.

In the meantime, the materials of his courses of lectures on comparative anatomy were attaining a vast extent, and he determined to give them to the public. With this view they were reduced to form by his two able assistants, MM. Dumeril and Duvernoy; and, in 1800, were accordingly published as the first two volumes of the *Leçons d'Anatomie Comparée.* M. Cuvier was himself engaged, during the compilation of those volumes, with the excavations of the gypsum strata of Montmartre, and the comparison of the bones discovered there, with the skeletons in the Museum. In the remainder of the work, which appeared in 1805, the parts which relate to the organization of insects and molluscous animals were exclusively his own, and probably no other anatomist then living was equally qualified to undertake those obscure and difficult subjects of zootomy. The Leçons altogether are known to comprise the most perspicuous, correct, and complete systematic view of the science to be found in any language.

He succeeded Mertrud, in 1802, in the professorship of comparative anatomy: and here we cannot help pausing for a moment, to contemplate the wide — the magnificent — range which he allowed himself, in the pursuit of his favourite science. Had his researches in comparative anatomy been conducted only in connexion with human, his illustrations and his contributions to the science could scarcely have extended beyond a few classes in the animal kingdom. If in connexion

with geology alone, his knowledge would have been mostly limited to the hard parts. Zoology, unaided by his general acquirements in comparative anatomy, would perhaps have been in danger of suffering additional confusion of arrangement. But with his unrivalled combination of qualities of mind, and ardour in the pursuit of all those collateral investigations which should be brought to bear on the one great end in view, M. Cuvier was enabled to attain a degree of excellence, as a comparative anatomist, which has never yet been equalled, and, in the lapse of ages, may possibly be long ere it be surpassed.

When Buonaparte returned from Egypt, and, after having been declared First Consul, was vain enough to permit himself to be chosen President of the Institut, Cuvier, who was one of the secretaries of that body, was necessarily brought into frequent intercourse with his future sovereign. The grand scheme of organizing the schools of France made this intercourse still more intimate, and Cuvier was appointed one of the six Inspectors General, whose business it was to establish Lyceums in the thirty principal cities of the kingdom. It was while on this mission, at Marseilles, that he so profitably employed his leisure moments in investigating the ichthyology of the Mediterranean.

Those examinations of the Montmartre strata, constitute an æra in geology: they were the continued labour of many years, and were crowned by the publication of the *Recherches sur les Ossemens Fossiles*, in seven volumes, 4to. 1812 — 1824. In this great work M. Cuvier embodied not only the information which he derived from his personal inquiries in the neighbourhood of Paris, and on his several visits to the interior of France, and his missions to Italy and Holland, but the abundant stores of knowledge which he procured from the correspondence of the most distinguished naturalists in all parts of Europe. The success of his labours in identifying the fossil bones of the mammalia, deservedly obtained for him the highest celebrity, while it afforded the most convincing proof of the utility of zootomical knowledge, as applied to geological investigations, that has ever been found in the records of science. No naturalist, indeed, ever enjoyed such opportunities as M. Cuvier for those particular researches; but perhaps no man ever possessed, in a more eminent degree, all the requisite qualifications for their successful employment. His indefatigable industry and enthusiasm—his extensive general knowledge—the beauty of his language, and the elevated tone of his descriptions, with the grandeur of his views, and the sublime truths which they unfold, regarding the past revolutions of the animal kingdom and the globe we inhabit, must convince every one that there was really no other individual living who could have so successfully performed what he accomplished in regard to the restoration of extinct species.

We shall not attempt, in this limited sketch, to introduce any further remarks relative to the *Ossemens Fossiles;* nor do we think it necessary to more than allude to the eloquent *Preliminary discourse* with which the work commences, —a discourse which, even had its author never written any thing else, would have perpetuated his name to a late posterity. It has been translated into several languages, and the English version, by Professor Jameson, has gone through several large editions.

In 1808 Cuvier was appointed one of the Councillors for life of the newly-erected Imperial University—an office which gave him frequent occasions

of discussing political affairs before the Council of State and in presence of the Emperor. He was sent, in 1809-10, to organize the academies of the Italian states; and his arrangements at Turin, Genoa, and Pisa, were found so advantageous that, on the return of the sovereigns of these states, the numerous measures adopted by Cuvier were preserved inviolate. Subsequently, in Holland and the Hanse-towns, the changes and nominations which he made were similarly respected and preserved; and, what is still more worthy of being recorded, though a protestant by country, birth, and education, he was yet sent, in 1813, to reorganize the University of Rome; and many of his improvements in La Sapienza were retained even after the return of the Pope.

It might be thought that the labour of collecting materials for such a work as the Fossil Organic Remains, and the task of arranging and describing them, were sufficient to have occupied the greater portion of M. Cuvier's life: but we have seen what some of his other occupations were, and to these we shall now add one fact more relative to the work just alluded to:—M. Cuvier found leisure not only to execute the drawings for, but even to engrave with his own hand, some of the most detailed osteological plates which adorn the several volumes.

But we have still other works of his to notice: his writings and researches were by no means confined to topics connected with the animal creation. He was in the habit of collecting, in his *leisure* hours, the most interesting historical facts illustrative of the progress of science, and of examining the scientific labours and composing biographical memoirs of the most eminent members of the Institut for many years back. Of these memoirs a collection has been published, in three octavo volumes, and they serve perfectly to establish his claim to an acquaintance with *all* the physical sciences, and the march of discovery in each of their various departments. The style in which they are written is easy, elegant, and perspicuous; and not a murmur has ever been breathed against their strict impartiality and truth.

The *Règne Animal* made its first appearance, in four volumes, in the year 1817—a work which is universally allowed to constitute the best outline of the present state of zoology and comparative anatomy. It was here that he may be said to have ultimately adopted his quadripartite division of the animal kingdom, after an experience of thirty years. Faults have been found, it is true, with this somewhat arbitrary division; it has been even characterized as not very uniform or philosophical, and as perhaps too much fettered by the author's early ideas of classification. But, on the whole, it is allowed to be a classical work, and is justly adopted as the great standard of reference by the ablest naturalists of every country. We need be the more brief in our notice of it, in consequence of its being, after the Preliminary discourse, that work of M. Cuvier's which is perhaps best appreciated by the English public.

It is pretty generally known, that the great undertaking on which his heart was set to the last moment of his life, was a complete system of comparative anatomy. For this vast enterprise, which he intended to surpass all that he had yet achieved, and to which he has alluded in all his publications—which was, in fact, the great study of his life, and the idol of his thoughts during nearly his whole career—he had prepared an immense series of drawings and engravings; and, indeed, the Museum of Comparative Anatomy owes its origin to his strong desire of accomplish-

ing this beloved object. What he has done for the class of fishes—and which he always looked upon as by far the most important of all his contributions to the natural history of vertebrated animals—may be contemplated as an earnest of what he would have done. It is said that he and Valenciennes, his colleague, had collected materials for an account of above 6000 species of fish, 5000 of which, it was calculated, would occupy the space of not less than twenty octavo volumes.

We have now noticed, however briefly, the principal scientific occupations of M. Cuvier—but vast and all-engrossing as they seem, they must be absolutely considered as the pastime of his leisure when taken in connexion with his functions in the state. Those functions, in fact, he always considered as his original destiny—the business to the conduct of which his early education was directed. He was about to be named a Councillor of State by Napoleon, when the events of 1814 occurred, to prevent that high distinction: it was, however, delayed only,—for in that same year he was named to the office by Louis the 18th. In 1815 he was retained as a member of the Commission of Public Instruction, and charged with the Chancellorship of the University. During the "hundred days" his connexion with the Council of State was suspended—but even then he continued to hold his important situations connected with public instruction.

From all cabals and political intrigues M. Cuvier ever kept aloof—a course of conduct which was sure to gain for him, as it did, the insidious hostility of numerous partisans. He was chiefly engaged with state business relating to the administration of the interior, and the regulation of the non-conformist religious sects; and besides this, was intrusted as King's Advocate, to defend before the Chambers all the principal projects of law.

It was while on a scientific visit, some years ago, to this country, that he was apprized of his having been elected during his absence one of the Forty—constituting the Academie Française—the highest distinction that pre-eminent talents can obtain in France, or in Europe. Soon after, he did not hesitate to decline the Ministry of the Interior, being dissatisfied with some of the terms on which it was offered to him. In 1819 he was named President of the Interior of the Committee of the Council of State—a place which he was anxious for, as unconnected with intrigue, but demanding method, constant activity, rigid discharge of duty, and a thorough acquaintance with the laws and principles of administration. He was created a Baron after this—a spontaneous mark of favour from his sovereign; and received several Orders, both from the King of France and the King of Wurtemburg,—his early protector.

But we will not attempt to enumerate the various honours and marks of distinction heaped upon Baron Cuvier from all quarters, as for a like reason we have not ventured even to allude to all the various publications to which he contributed his powerful assistance; either attempt would fill several pages of our journal. We must even now draw our brief notice rapidly to a close.

With all his mighty achievements in science, and almost every branch of human knowledge, no man, were one to judge from his personal bearing, would seem to have achieved less. In the enjoyment of competent wealth, influence, profound learning, and the highest fame, M. Cuvier's easy and unaffected manners never failed to command the love and esteem of all who had the honour of enjoying his acquaintance.

Many in this country—which he again visited not very long ago—will ever remember him with veneration and delight. His very look was calculated to impress respect and admiration. In his domestic circle, his deportment is described as having been amiable in the highest degree. His younger brother, Frederic Cuvier, is very favourably known in the annals of French science: he was appointed keeper of the Menagerie at the Jardin du Roi in 1805, and rendered much valuable assistance in organizing the Museum of Comparative Anatomy. As a naturalist he stands high in the estimation of his contemporaries.

The library of the late Baron is a rich and vast collection, and his private museum is said to be of great value: both will be purchased, it is understood, at the expense of government, and the proceeds rendered available to the widow. Madame Cuvier, no doubt, will be competently provided for: she is to have, along with a pension for life of 6000 francs, the town residence of her late husband, his house in the Jardin du Roi.

The circumstances of the death of Baron Cuvier we shall append in some detail. His funeral honours were performed with unusual magnificence: they were attended by not less than between four and five thousand persons—including all those who were of any literary or scientific pretension in Paris; and the orations pronounced over him by the representatives of the different learned societies with which he was connected, were remarkable for their number and impressive eloquence. Their theme could not have been a more fertile one; for their illustrious subject was universally admitted to have been guided in his great career by nothing but the pure love of knowledge, and the ardent desire to benefit his race. His labours, in a word, will merit the gratitude and command the admiration of all posterity.

The last illness, and death of Cuvier, were attended with some very remarkable circumstances. On Monday, the 7th instant, he complained to his friend, M. Dumeril, for the first time, of an uneasiness at his stomach, with looseness of the bowels, for which he was advised to keep his room next day, and to take some remedies suited to the circumstances. This he declined doing, because he had to preside at a council of state, a duty which he accordingly discharged. On his return, he met with the physician who usually attended him, M. Allard, and whom he took the opportunity of consulting. Cuvier had experienced at breakfast that morning a considerable difficulty in swallowing, in consequence of which M. Allard thought it necessary to advise the application of leeches to the anus. On returning home, however, his patient betook himself to his scientific labours, in which he remained engaged from two o'clock in the afternoon till half-past five, when he was summoned to dinner. It was only then that he informed Madame Cuvier of M. Allard's having prescribed for him, intimating his intention of postponing the application of the leeches till his usual bedtime, being at that moment, he said, more inclined for his repast. He sat down, accordingly, to dinner; but scarcely had been helped to some soup, ere he found that he was almost entirely unable to swallow. M. Allard was now sent for, as were MM. Dumeril and Orfila. Leeches were applied in the evening; and in the course of the night he was bled from the arm; but neither of these means was followed by any relief. Next day, MM. Dupuytren, Biett, and

Koreff, were added to the attendants, and joined them in consultation. On Wednesday morning, it was resolved to administer an emetic, and four grains of tartarized antimony were dissolved in some spoonsful of water, but only a small portion of this could be got down. At this time M. Cuvier had no appearance of illness; he had no uneasiness, and no fever; but merely complained of extreme difficulty of swallowing, and of a gradually increasing inability to move the upper extremities, without there being the slightest perceptible change in these parts. The emetic did not act upwards, but produced several evacuations from the bowels. Twenty-four grains of ipecacuanha were afterwards injected into the stomach by M. Dupuytren, by which, however, no effect was produced; and some hours afterwards, four successive doses, of twelve grains each, were similarly introduced, but without any emetic effect resulting. The difficulty of moving the hands and arms went on gradually augmenting. Leeches to the upper part of the spinal column were now recommended; and next day (Thursday) cupping-glasses were applied along the back, but scarcely more than four ounces of blood were obtained. The upper extremities were now perceived to be slightly tumefied. Various plans were suggested in consultation, particularly the actual cautery, and blistering to the spine, the latter of which only was adopted, and even that it would appear but imperfectly. By Sunday the paralysis had extended, and in fact had become general, implicating the respiratory muscles; the lower extremities were absolutely motionless, and the stomach and bowels seemed to have ceased to perform their functions. M. Cuvier, however, retained his mental faculties unimpaired, and was fully aware of the danger of his situation, constantly expressing his regret at leaving so many works unfinished. "This hand," said he repeatedly, "this hand, which has performed so many dissections, and committed so many of the results to paper, is henceforth condemned to inactivity." Afterwards, as the symptoms continued to increase, he spoke, not merely of the loss of his limb, but of his death; and expressed, with deep regret, the necessity of leaving his Comparative Anatomy incomplete: in fact, he was engaged in remodelling this great work at the time he was struck with the fatal paralysis. On Sunday, at five P.M. he spoke with difficulty, and only to express the general uneasiness which he felt. M. Dumeril, when he quitted him, remarked that he would see him again early next morning; but Cuvier shook his head in such a manner as to express his belief that they should meet no more. In a few minutes after this interview he began to sink, and expired between ten and eleven at night.

The general impression among the medical attendants was, that M. Cuvier's complaint depended upon some lesion of the spinal marrow, attended with pressure; and this expectation was the more probable, as it appears that their patient, not long before his death, had met with a fall, in which he had strained himself in the effort to recover his balance. The body was examined on the 15th, when the following appearances presented themselves. The general aspect externally remained unchanged, except that putrefaction had already made some progress; the cranium was very large; the vertex flattened, and almost quadrilateral; the thickness of the parietes, particularly at the frontal sinuses, was very great; the right side projected more than the left; and the same was observed of the parietals; the occiput was also fully developed; the inner table of the frontal bone exhibited three nipple-like pro-

jctions; the convolutions of the brain were very numerous and voluminous. The lateral ventricles were very much dilated, and contained a small quantity of reddish serum; the membrane lining them was slightly rugose; there were some calcareous depositions in the pineal gland; the origins of the nerves were sound; and the whole of the encephalon and spinal cord were without any alteration in texture, colour, or form. The œsophagus presented no appearance of disease, and all the viscera were sound. The processus dentatus was unusually large; and there existed a sort of bony projection at each point of junction between the vertebræ, along the whole anterior surface of the column. These, however, must have been long present, and cannot be regarded as connected with his death: indeed, they were supposed by the reporters, and with some probability, to be connected with the peculiar habits and gait of M. Cuvier. The entire absence of any postmortem appearance calculated to explain the phenomena attending M. Cuvier's death, has led some—particularly Magendie—to hazard the opinion that his illness was, in fact, but a modification of cholera, indicated in the first instance by diarrhœa, and subsequently by nervous depression, the intellect remaining unimpaired.

Sœmmering, as the extreme weights of the healthy human brain, gives two pounds five ounces and a half, and three pounds three ounces and three quarters—the great majority being intermediate between these two; and M. Berard, in some recent examinations, has arrived at nearly the same results. But the brain of M. Cuvier weighed three pounds ten ounces four drachms and a half, being much above the extreme weight mentioned by Sœmmering. Besides, the cerebellum and tuber annulare were compared with those of a male adult, and found to exceed them in weight only by a drachm and a half; so that, in M. Cuvier, the excess was almost wholly confined to the extraordinary development of the anterior lobes—that is, to the organ of the intellectual faculties. Again, according to M. Desmoulins, one of the characters of the brain with which superiority of intellect seems to be associated, is great extent of surface—resulting from the number and depth of the convolutions—so that a great expansion of this kind might be comprehended within a cranium of moderate dimensions. Viewed in this light, the brain of M. Cuvier was even more remarkable than with respect to its size, none of the distinguished anatomists who were present at the examination having ever witnessed convolutions so numerous, or enfractuosities between them so profound. It was at the upper and anterior part of the brain that this conformation was most strikingly developed.

APPEARANCES IN THE BODY OF M. PERIER.

The corpse retained an extraordinary degree of heat. The most remarkable circumstance about the head was *the thickness of the skull.* There was a little serous effusion under the membranes, but the brain was healthy. The stomach internally presented several portions of a bright red, from the minute injection of vessels in an arborescent form; similar spots presented themselves in various portions of the alimentary canal; and the mucous membrane was in several places very much attenuated. The lungs were healthy, the heart soft and flaccid, and the walls of the ventricles thinner than natural.

VACANT PROFESSORSHIPS IN EDINBURGH.

We understand that Dr. Christison is to have the chair of Materia Medica, as successor to the late Dr. Duncan. The professorship of Medical Jurisprudence, which will become vacant, is contested by Dr. Craigie, Dr. James Gregory, and Dr. Maclagan*. We earnestly hope that the selection will be made on grounds conducive to the interests of science—a consummation which, we fear, has been little advanced by some of the recent appointments in the Scotch Universities.

TREATMENT OF CHOLERA.

We have lately seen two letters from Dublin, in which it is stated that the saline treatment of cholera, as described in this Journal, has been extensively adopted, particularly in private families. In consequence of the statements regarding the Cold-Bath Prison being copied from our pages into some of the daily papers, many persons had provided themselves with bottles, filled with the salts, in due proportion, so as to be ready for immediate usé. The mortality has recently very much diminished, and we observe in one of the latest reports, fifty-one recoveries and only four deaths. It will, perhaps, be remembered, that some discussion took place a few months ago, in consequence of the plan of injecting saline solutions into the veins having been recommended in this Journal as a *dernier ressort* in cases of collapse. Our readers will find at page 257 of our present number an account of the only instances, within our knowledge, in which this idea has been acted upon. The results are interesting and important.

* Our Edinburgh correspondent, who supposes that this gentleman is a candidate for the chair of Materia Medica, is mistaken; it is for that of Medical Jurisprudence that his application at the Home Office has been made.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

MIDDLESEX HOSPITAL.

There were admitted under the care of the surgeon, in the week terminating on the 8th of May—two cases of femoral hernia; one of gun-shot wound of the thigh; one of aneurism by anastomosis of the lip; one of fractured ribs; one of scrofulous swelling in the axilla; one of morbus coxarius; one of diseased phalingo-metacarpal articulation of the middle finger; two of injury of the hip (bruised); one of injury of the shoulder; and two of ulcer of the leg.

Femoral Hernia—Death. Query: from inflammation of the peritoneum, or from the strangulated portion of bowel not having recovered its function?

M. C., 25 years of age, married, and without family, was admitted into Bird's ward on the afternoon of Tuesday the 1st of May, with femoral hernia of the left side. The patient had been subject to this complaint for five years, and had occasionally worn a truss, but so irregularly that she had sometimes left it off for months together, as had recently been the case. The swelling, never larger than a pigeon's egg, was only occasionally down, and upon pressure could always until now be returned. In the forenoon of Sunday last, the 29th ultimo, she was attacked with pain in the stomach, nausea, and vomiting, which she attributed to her rupture having descended that morning when she was putting up some lines. Failing in her attempts to return it, in the evening her medical attendant endeavoured to do so, and he thought he had succeeded. The following day she was tolerably easy, and was free from sickness; but in the evening vomiting recurred, and in the forenoon of the 1st inst. having again sent for the gentleman alluded to, he finding the hernia still present, after bleeding her to the extent of twenty ounces, and failing to reduce it, sent her to the hospital.

The tumor was the size of a small walnut, smooth, tense, and without pain; but there was some tenderness of the abdomen on pressure. Her countenance did not indicate suffering or anxiety; she was not sick, and had not vomited for some hours; she had had no motion from her bowels since the morning of the 29th; her pulse was feeble, and she seemed chilly. The taxis was employed ineffectually, and the effects of a clyster and a warm bath were tried with as little success. The operation was therefore performed by Mr. Arnott, the assistant-surgeon. On opening the sac some brown-coloured serous fluid was discharged,

and a knob of intestine of the size of a hazel-nut, of a dark red colour and smooth surface, was exposed. On relieving the stricture, which was very tight, and for which purpose a very small incision sufficed, the portion of bowel was easily passed back into the abdomen, and its reduction was followed by the escape of a considerable quantity of serous fluid from its cavity. This evidence of peritoneal inflammation, and the tenderness on pressure, led to the application of thirty-six leeches immediately on her removal to bed; and about an hour and a half after the operation she was bled from the arm until the pulse gave way. Three grains of calomel, were ordered to be taken every two hours; and blood was taken from the arm during the night, by Mr. Pogson, the house-surgeon. On the morning of the 2d instant she stated herself to be easier; the abdomen was less tender, bearing pressure, and there had been no motion of the bowels. A dose of castor oil was ordered to be given her, and a common enema. At half-past twelve it was found that the castor oil had not been taken, as she had been sick and vomited, but that ten grains of extract of colocynth had been given instead. She had had, since the vomiting, a recurrence of pains in the abdomen, and twenty leeches had been applied. The patient was now pale, looked distressed, and complained of great pain in the belly, increased on pressure; the pulse was frequent and somewhat hard, the tongue white and moist, the countenance a little flushed; no motion had followed the injection, nothing but a quantity of glairy mucus. She was again bled from the arm until she fainted, which occurred when she had lost about sixteen ounces of blood; the calomel was continued. At seven o'clock she was freer from pain, but the pulse was so frequent as not to be counted, and she was much exhausted. A grain and a half of opium was given directly. At eleven she had had some sleep; did not complain of pain; the tongue was moist and almost clean, but there had been no discharge from the bowels except some more glairy mucus. The castor oil was directed to be repeated. Early on the morning of the 3d, this patient, who had been brought in during the absence from town of her husband, had been by him removed to her own home, where she died in the course of the day, the bowels not having been opened.

On examining the body the succeeding day, it was found that there had been a considerable discharge, after death, of serous fluid from the wound in the groin; and on opening the abdomen a small quantity of sero-purulent fluid was found in its cavity. The convolutions of the intestines were loose, and free from adhesions, and the portion of bowel which had been strangulated had receded to a distance of three or four inches from the crural ring; externally it presented an appearance of stricture, was of a dark red colour and smooth surface, the peritoneal coat being entire. The discoloured portion of bowel (not more than an inch in length, and not gangrenous) felt thinner than the healthy, and on slitting it up, the mucous membrane presented a breach of surface at the part corresponding to the strictural impression externally, transverse as regarded the course of the bowel, and with the exception of the portion where the mesentery is attached, the ulceration involved the whole circumference of the canal. The breach of surface was filled by firm, yellow, coagulable lymph. The canal of the intestines, above the portion which had been strangulated, was greatly distended with flatus, and its peritoneal coat in several places presented slight appearances of incipient inflammation—longitudinal red streaks formed by minute vessels.

Femoral Hernia—Recovery—Bowel in an unpromising state. Query: if less injured than in the preceding case, from the presence of a portion of omentum?

S. P., 65 years of age, was admitted into Bird's Ward, about 11 A.M. on the 8th inst. with a femoral hernia of the right side, which had been down since the afternoon of the 6th. The swelling was of the size of a large egg, felt tense, but slightly irregular, and was turned up over Poupart's ligament. After coming in, she had vomited some frothy matter, and bread and milk, which she had taken for breakfast. There had been no motion from the bowels since the 6th. There was no tenderness of the abdomen, but some tension and occasional pain. The pulse was 64. She had been subject to hernia for fifteen years, and had worn a truss, but not for some months. The hernia, when down, she had always been able to return until now, when she failed in effecting this. Being attacked with sickness and vomiting during the night of the 6th, the next day (that previous to her entering the hospital) she sent for a surgeon, who made repeated attempts to reduce it. This gentleman also bled her, and tried the effect of injections.

On her entrance, the taxis was ineffectually applied, and after a consultation it was determined that the operation should be performed forthwith; which was done by Mr. Arnott, who was acting for the surgeon of the week. On opening the sac, which was very thin, and from which no serum was discharged, it was found to contain a portion of omentum and a single small loop of bowel of a very dark-brown colour; the surface somewhat rough, with distinct appearances of coagulable lymph, and its coats enormously thickened, feeling quite fleshy. Such was its unfavourable appearance. It

was suggested that it should be slit open, and attached to the edges of the wound; but this proceeding Mr. Arnott did not adopt. After dividing the stricture upwards and inwards, and drawing down a portion of the bowel from within the abdomen, shewing that there were no adhesions, he endeavoured, after pressing out its contents, to return the bowel, but failed — only the healthy portion which had been drawn down returning; the other, from the great tumefaction of its coats, could not be made to pass up. The hernia-knife was therefore re-introduced, and the stricture divided in another direction—namely, horizontally towards the pubes: but even after this, and when the point of the finger could be passed by the side of the bowel through the stricture, great difficulty was experienced in returning this single small loop of intestine, from the cause already mentioned. Ultimately, however, it was gradually and slowly reduced. The omentum was left in the wound, and, the skin having been brought over it and retained by two stitches, a compress and bandage was applied in the usual manner. A clyster which had been administered about half an hour previous to the operation, came away a quarter of an hour afterwards, and was succeeded by two or three discharges of fæculent matter;—by 10 at night she had had two or three more. At this time the patient was easy, the pulse 62, the abdomen soft and free from tenderness. On the morning of the 9th she complained of some pain in the belly, and Mr. Lonsdale, the house-surgeon, took ten ounces of blood from the arm, and directed sixteen leeches to be applied to the abdomen. As she had had no motion since the night before, a clyster was ordered to be given, and two grains of calomel with one-third of opium, every six hours. At 8 P.M. she was found to have shortly before vomited some orange pulp, which she had very imprudently, and contrary to order, eaten; and as the bowels had only been once scantily moved since the morning, seven grains of compound extract of colocynth, two of calomel, and three of extract of conium, in pills, were ordered to be given immediately, and repeated in six hours if necessary. This produced two healthy motions during the night, and some uneasiness which she felt in the abdomen was relieved by the application of a mustard poultice. In the course of the 10th, her pulse became more frequent, reaching to 120 in a minute, and so full that sixteen ounces of blood were taken from her arm; and at night, although her bowels had been relieved several times, her pulse was 106, so that eighteen leeches were ordered to the abdomen, and a pill, containing two grains of calomel, half a grain of powdered digitalis, and half a grain of opium, every four hours. On the 11th her pulse had got down to 90, and her bowels had been freely opened; the pills were discontinued. Towards evening she became so restless that fourteen ounces of blood were taken from the arm, and twelve leeches applied to the abdomen; the blood was thickly buffed, but not cupped.

On the 12th, though the tongue was clean and moist, and the abdomen soft, slight pain was experienced on pressure. The colocynth and conium pills were repeated, and acted. In the evening sixteen leeches were applied, and, as she had not slept the preceding night, thirty drops of laudanum were given to her at bed-time. The 13th she continued to improve.

As some redness of the integuments around the wound was now observed, the stitches were removed, the compress and bandage left off, and their place supplied by a poultice. For a day or two the discharge was thin, but it has now (May 21st) assumed a healthy character; the redness has disappeared, and the wound is partly healed.

Compared with the preceding case, the condition of the strangulated portion of bowel in this appeared much more unfavourable; yet it has done well. In what, then, did the most important difference between the two consist? In that first related, there was general peritoneal inflammation in existence at the time of the operation; in the latter there was not, nor did it seem that the inflammation ever extended beyond the incarcerated portion of bowel. The condition of the returned intestine was, it is true, a source of much anxiety; and it will be seen, by the treatment which was required, that some time elapsed ere the patient was out of jeopardy. In connexion with the favourable issue of this case, it may be worthy of remembrance that the pressure upon the bowel was shared by omentum, and, with the unfavourable termination of the former, that the bowel was exposed alone and unshielded to pressure, which produced ulceration of the mucous membrane.

Pistol Wound of the Thigh.

F. W. 16 years of age, was admitted into Percy ward on the 6th inst.; on that morning he had been putting a pistol, loaded with small shot, into his left hand breeches pocket, when it exploded. On examination, the skin on the upper and anterior part of the thigh, immediately below Poupart's ligament, was found burnt black, to the extent of a circle about two inches and a half in diameter, and beyond this there were some vesications of the integument. In the centre of the former was a ragged aperture, directly over the course of the femoral artery. On introducing the finger into this, it passed downwards and inwards, not between the muscles, but between the latter and the adipose tissue and

skin. On the lad's admission, Mr. Lonsdale, the house-surgeon, felt the charge of the pistol lodged beneath the skin on the inner part of the thigh, about four inches from where it had entered; and having made an incision in this situation, he removed two pieces of wadding (thick pasteboard), and a quantity of small shot, as well as a piece of the pocket. On introducing the finger of one hand into this aperture, and a finger of the other into that in the centre of the burnt portion of skin already described, they could be made to meet.

The point of importance in this case was the vicinity of the femoral artery to the wounded parts; that it had escaped direct injury was evident from the course of the charge, and from its continuation being found pulsating in the ham; but as sloughing to a certain extent must necessarily ensue, it was possible that it might become subsequently involved. It was a great object, then, to prevent the extent of this slough, by the inflammation which would be set up. Leeches were therefore applied to the thigh round the wound. Sixteen ounces of blood, which was somewhat buffy, were taken from the arm, and in the evening it was found necessary to take fifteen ounces more. Salines, with antimony, were given every six hours. On the following day, although he had been purged, and continued to take the medicines prescribed, to which was added some tincture of digitalis, in the evening the pulse was so full, the lad feverish, and complaining of headache, that sixteen ounces of blood were again taken from the arm. On the 8th, the general symptoms were greatly relieved, but on account of the swelling and redness of the thigh, leeches were again applied. By the 10th this had considerably subsided, and the slough had began to separate, attended with a very free discharge. By the 17th, the slough was completely detached, leaving a sore, presenting a healthy granulating surface. Simple dressing was now applied, and it is now (the 21st) healing.

Aneurism by Anastomosis of the Lip.

E. P. 20 years of age, was admitted into Handel's ward on the 4th of May, on account of a swelling in the under lip. It occupied two-fifths of this, and those adjoining the left commissure: this part was swollen, projecting considerably above the level of the healthy portion of the under lip; it was also somewhat everted, and of a purple colour. On laying hold of the part, its vessels were felt pulsating strongly. By pressure it diminished, but resumed its usual size when this was removed. The disease was developed in the substance of the lip, and not in its cutaneous or mucous covering; but on the latter there was an appearance of scales, leading to the apprehension that the part might here give way. The disease began a year ago without any cause, and when first observed, the swelling appeared to be the size of a pea. The diseased part was removed by excision, a triangular portion of the lip being enclosed in two incisions, as in the operation for cancer of this part. In doing this, Mr. Arnott employed a pair of forceps, (not unlike a pair of large dressing ones) one blade inlaid with wood, upon which the other and narrower one acted. Grasping the lip with this, he cut by the side of the narrow blade on the other, thus combining a straight with a rapid incision. One artery bled very freely, and after about ten ounces of blood had been allowed to flow, (the patient was a stout country girl) it was stopped by the touch of a hot wire. The parts were brought together by pins and the twisted suture; the wound healed kindly, and the patient left the hospital on the 16th instant.

REPORT OF CHOLERA, UP TO FRIDAY, MAY 25, 1832.

New cases in Great Britain (exclusive of London) since our last report ...	224
Deaths.....	77
Total number of cases throughout Great Britain (inclusive of London) since the commencement ...	12837
Deaths...	5211

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

May 1832.	Thermometer.		Barometer.	
Thursday . 17	from 34 to	55	29·76 to	29·86
Friday. . . 18	38	62	30·00	30·09
Saturday . 19	29	66	30·07	30·11
Sunday . . 20	34	66	30·16	30·12
Monday. . 21	39	70	30·13	30·15
Tuesday . 22	44	70	30·15	30·13
Wednesday 23	40	71	30·16	30·22

Wind variable, S.E. prevailing. Except the 17th, generally clear.

Rain fallen, ·25 of an inch.

On the afternoon of the 17th instant we were visited by another storm of thunder and lightning, though not of so alarming a kind as on the 15th. At Enfield, a town about two miles N.W. of this place, hail-stones fell of an extraordinary size, measuring three-quarters of an inch.

Charles Henry Adams.

NOTICE.

Mr. Middlemore's paper "On the Reproduction of the Chrystalline Lens" has been received, *but not* that "On the Malignant Diseases of the Eye."

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 2, 1832.

ABSTRACT

OF THE

GULSTONIAN LECTURES,

Read to the Royal College of Physicians on the 2d, 4th, and 9th of May, 1832.

By Dr. Roget.

Dr. Roget selected, as the subject of these lectures, the Laws of Sensation and Perception; a subject, he observed, which constitutes a distinct province of physiology, abounding in curious but difficult topics of speculation, and leading to a variety of important practical results. It embraces questions of the most momentous kind relative to the connexion between mind and matter—questions which, being involved in the profoundest mystery, must ever excite our curiosity and wonder, and on which every new fact or consideration tending to throw the least ray of light, cannot but be highly interesting and important. The phenomena of sensation and perception, involving both the functions of the body and the operations of the mind, are comprehended within the objects both of physiology and of metaphysics, which belong respectively to the two great departments of human knowledge. The same general laws govern both the healthy and the morbid actions of the body and of the mind; so that the study of any one branch would be incomplete unless it were made to comprehend the whole. Nor is the utility of the accurate knowledge of these laws confined to the exercise of the medical art; it extends to a multitude of other objects. It is evident, for example, that all those arts which address the mind through the medium of the senses—such as music and painting, depend on principles having a direct reference to the laws of perception. These laws, moreover, being intimately connected with the origin and formation of our ideas, and with the modes, as well as the materials of thought, an intimate acquaintance with them becomes necessary for the discovery of the most effectual methods of acquiring knowledge, and of discriminating truth from error. Even the art of logic may derive important advantage from the cultivation of physiology, which might, on a superficial view, be considered as foreign to its object; thus exhibiting an instance, among the many furnished by philosophy, of the alliance subsisting between subjects of a nature apparently different, and of the light that is often mutually reflected upon each from quarters the most unexpected, and from sciences the most remote.

An analysis was then presented of the series of phenomena which constitute sensation and perception, and of the succession of changes, both corporeal and mental, which these phenomena imply. These changes were arranged, according to their natural order of sequence, into different classes, constituting so many distinct processes. In the first place, the external body, which is the object of perception to the mind, must produce a certain impression on those extremities of the nervous filaments which are situated in the organ of sense, and which are appropriated to sensation. It is scarcely ever by its direct agency that it produces this impression; for it is generally by the intervention of a series of changes taking place in some medium, which is designedly interposed for the purpose of modifying the impression. In the case of the senses of hearing and of vision, these changes effected in the intermediate organs are of great importance; and they are also preceded by a succession of actions in the external medium, generally the air, which conveys the agency of sound, or of light, proceeding from distant objects. The second stage of the process consists in the actual impression made on the extremity of the nerve situated in the organ of sense, and in the change consequent on that impression. The third stage is constituted by the propagation of that change along the course of the nerve to its

termination in the brain, or central portion of medullary substance. Anatomists have improperly designated this extremity of the nerve as its *origin*, for with reference to its function it is evident that the series of changes which lead to sensation, commence at the more distant end, and terminate at the point of its connexion with the brain. The fourth physical change necessary for the production of sensation, is that which takes place in the sensorium, and is the immediate antecedent to the mental change which constitutes sensation, and which is, in its nature, wholly different from the physical change that has preceded it. In experiencing sensations, the mind is wholly passive; but in the combination and the comparison of sensations and ideas, and in the judgments to which they lead, the mind is partly active and partly passive. Hence arises *perception*, which is the knowledge of external objects as being the causes, or invariable antecedents of our sensations, and as having an existence independent of ourselves.

The phenomena, both mental and corporeal, which take place subsequently to perception, and which intervene between that action of mind and voluntary motion, were next subjected to a similar analytical investigation, and distinguished into different processes. In every instance of sensation leading to perception and voluntary motion, the whole of this series of changes must be performed, although the effect appears to follow the original cause without any sensible interval of time. Thus the sight of the written words of a discourse may be almost instantaneously followed by the muscular movements required to give them utterance.

The limits within which the lecturer was obliged to confine himself in treating a subject of such extent as that comprehended in the preceding outline, induced him to omit altogether the consideration of the several preliminary steps by which the external objects of perception have their influence conveyed through the exterior medium; and also through the external portion of the organ of sense, to the nerves of sensation contained in that organ. Taking for granted, then, that certain impressions have been made upon those nerves, the first inquiry he proposed to enter into related to the nature of those impressions, and of the immediate agencies by which they are produced.

Dr. Roget then proceeded to notice the considerations which point out the simple nature of sensation, considered as a mental change, and also those which establish the distinction between sensation and perception. The variety in our sensations, he observed, indicate to us a corresponding variety in the qualities of the material agents which produce their impressions on our senses. But the knowledge of these qualities, and even of the existence of the objects themselves, are neither the immediate nor the necessary effect of those impressions on the mind, but are inferences deduced by our reasoning powers. Thus in seeing a distant object, the immediate agent and primary object of perception is merely the light, which produces a certain impression on our retina; whence we infer, by a complicated mental process, the existence, the position, and the magnitude of the external object. But even the primary objects of perception, such as light and sound, may themselves be regarded as mere creations of the mind, irresistibly suggested, indeed, by the accompanying sensations; but yet having no real resemblance, nor even correspondence, with the impressions themselves, or with the agencies which produce them.

In illustration of this proposition, a review was taken of the various kinds of agencies by which our organs of sense are capable of being impressed. These agents were arranged under three heads: to the first belong those that may be termed mechanical, and which act by simple pressure, or the forces derived from gravitation, and from corpuscular attraction and repulsion, and which also include the chemical agencies of matter upon our bodily organs. The actions of heat, of light, of electricity, perhaps, also, of magnetism, comprehend a second class, derived from what may be denominated the imponderable physical agents. To the third class were referred those various and more subtle influences, concerned in the physical part of the process of sensation, of the nature of which we can form but still more imperfect conceptions, and which science has in vain attempted to unravel. Various instances were given, proving the total want of correspondence between the sensations excited, and the qualities in the agents which produce them, of which we have obtained a more exact knowledge from other sources.

Still more decisive evidence of the discordance between our perceptions and their causes, is furnished by the numerous instances which shew the fallacy of those perceptions, and the errors we are liable to commit when we place in them an undue confidence. Many examples of this kind were stated, in which fallacious perceptions arise when certain impressions are made in an unusual manner upon the nerves of sensation. In particular, the effects of the transmission of the galvanic influence through the facial nerves, giving rise to the appearance of flashes of light, when no light is really present, were minutely detailed, as well as those attending the action of galvanism upon the auditory, the olfactory, and the gustatory nerves. It has been supposed that in the last of these instances, where a peculiar metallic taste is excited by including the nerves of the tongue in the galvanic circuit, a consi-

derable part, if not the whole, of the effect arises from the actual presence of saline matter developed from the decomposition of the saliva by the chemical influence of the galvanism; but Dr. Roget regards this mode of explaining the phenomenon as incorrect; and states, among the reasons for rejecting it, that the effect succeeds the contact of the metals too instantaneously to warrant the supposition that chemical decomposition can have been effected to the extent required for the production of the observed effect.

The various hypotheses hazarded by speculative physiologists, as to the nature of the organic changes in the nerves connected with sensation, were next adverted to, and the questions discussed whether any mechanical displacement takes place among the particles of the nervous substance; whether they are agitated by vibrations, as imagined by Hartley; whether they imitate, on a minute scale, the fibrous contractions of muscles, as asserted by Darwin, or whether they undergo any changes of chemical composition, or of their electrical states, as conjectured by more modern physiologists, or what other conceivable modifications they may possibly experience.

Proofs were then adduced that our sensations, and consequently the impressions which give rise to them, are by no means in exact correspondence with the qualities of the agent which produces them. This appears from the circumstance that the range of our senses is restricted within very narrow limits. It requires a certain intensity in the agent, whether it be light or sound, or the chemical agencies applied to the senses of smell and taste, or the mechanical forces applied to those of touch, in order to produce the very lowest degree of sensation. On the other hand, when their intensity exceeds a certain limit, the nature of the sensation entirely changes, and becomes that of pain. A great number of the sensations commonly referred to the sense of touch, convey no perception of the cause which produces them: thus a slighter impression than that which gives a feeling of resistance, produces the sensation of itching, which is one of a totally different kind. The sensation of cold is equally a positive sensation with that of warmth; but they differ from one another, not merely in degree, but in kind, although science teaches us that it is only in degree that the external causes of these two sensations differ.

The nerves subservient to sensation are each respectively adapted to receive impressions of particular kinds, within the limits already stated, and to exclude all other impressions. These differences must be ascribed to peculiarities of organization. Hence the offices of different kinds of nerves are not interchangeable, as is the case with many other functions of the animal economy. No other nerve but the optic nerve, and no part of that nerve except the retina, is capable, however impressed, of giving rise to the sensation of light; none but the auditory nerve can convey that of sound; and so of the rest. The credulity of the public has been frequently imposed upon by persons who pretended to see by means of the fingers, as in the case of the celebrated Miss M'Avoy. Equally unworthy of credit are the stories of persons in a trance, induced by animal magnetism, hearing sounds not addressed to their ears, but to the pit of the stomach; or reading the pages of a book applied to the skin of the abdomen.

This subject was further illustrated by examples of the modified sensibilities of internal organs, which are limited to particular kinds of impressions. To the catalogue of sensations of this class, arising from the natural actions of the body, must be added the still more numerous feelings which accompany morbid conditions of structure and of function. Instead, therefore, of restricting the senses to five, as is commonly done, it would be more philosophical to extend the number much further, and, classing them according to the nature of the sensations conveyed, to consider each difference of texture as laying the foundation for a distinct organ of sense. Among the most important of these anomalous and hitherto unclassed sensations, are those attending the movements of our limbs, or other parts of the body, which are the occasion of our first ideas of space and the relative positions of objects.

Dr. Roget then proceeded to consider the physiological conditions of the external organ which influence the nature of the impressions made on the nerves of sensation. The perfection of this impression, he observed, depends upon the healthy condition of the nervous power in that part of the system. This, again, implies first, the healthy and regular circulation of arterial blood through the vessels of the nervous substance; secondly, the maintenance of a proper temperature; and thirdly, the undisturbed condition of its organization. Ample illustrations were given of each of these laws. Yet the power of sensation, it was remarked, is liable to fewer variations than many of the other functions of the system; for it often happens that the functions of digestion and secretion may be considerably disordered, nay, the constitution may be undermined by the fatal disorganization of some vital organ, while the senses, even to the last, remain unimpaired.

The variations which take place in the sensibility of our external organs of sense, independently of any apparent change of vascular action, arise principally from the impressions to which their nerves have been

previously subjected. It may be stated as a fourth law, that all impressions made upon the nerves of sense have a direct tendency to exhaust their sensibility; as also an indirect tendency to exhaust the sensibility of the system in general. Of this important law of sensation a great variety of examples and illustrations were given. One of the most obvious of these presents itself in the case of our sensations of temperature. The same body may appear warm to the touch at one time and cold at another, although its real temperature has remained the same, according to the state of the organ induced by previous impressions. This discrepancy in the sensations, arising from the varying susceptibility of the skin, is sometimes the source of fallacious judgments. Thus a different statement has been given by different observers of the coldness of the tongue of the same patient attacked with cholera; in consequence of the difference of the susceptibilities to warmth of the fingers employed in the examination.

The operation of this principle was traced through all the other senses, beginning with those of taste and smell, and proceeding with those of hearing and of vision, which furnish the most striking illustrations of its force. A remarkable case was related, in which a morbid increase of sensibility had taken place in the auditory nerves, from mechanical obstruction in the ear. On the removal of the obstruction, by the operation of perforating the membrana tympani, by which hearing was suddenly restored, the faintest sounds were productive of great distress, appearing to strike with excessive loudness upon the ear. The variations in the sensibility of the retina, arising from the previous impressions of light which it has received, admit of more accurate determination, and afford the best means of studying minutely all the circumstances connected with this law of sensation. Dr. Roget shewed that these variations are quite independent of the alterations which take place in the size of the pupil, and which follow a very different law. The influence of this law of vision has been applied, with great advantage, to the purposes of astronomy, by Sir William Herschel, in his examination of distant regions of the heavens. By the methods he employed for that purpose, the sensibility of his eye was so much increased that, when a star of the third magnitude came towards the field of view of his telescope, he found it necessary to withdraw his eye before it actually presented itself, lest he should injure the acquired delicacy of his vision. On one occasion, after a considerable sweep with his forty feet telescope, the appearance of Sirius announced itself, at a great distance, like the dawn of the morning, and came on by degrees, increasing in brightness, till this brilliant star at last entered the field of the telescope with all the splendour of the rising sun, and obliged him to withdraw his eye from the beautiful but too dazzling spectacle.

The peculiar construction of the organ of vision allows us to distinguish the effect of impressions made on particular parts of the retina from those made on the rest, and also from their general effect upon the whole organ; which we have not the opportunity of doing in the case of any of the other senses. These partial variations of sensibility in the retina give rise to the phenomena of *ocular spectra*, first noticed by Buffon, and since more fully described by Dr. Robert Darwin. A white object on a dark ground, after being viewed steadfastly until the eye has become fatigued, produces, when the eye is immediately directed to another field of view, a spectrum of a darker colour than the surrounding space, in consequence of the exhaustion of that portion of the retina upon which its image had been impressed. The reverse takes place when the eye, after having been steadfastly directed to a black object on a light ground, is transferred to another part of the same field; in which case, a bright spectrum of the object is seen.

Not only is the general sensibility of each part of the retina to light liable to variation according to the previous impression it has received, but also its sensibility to each particular kind of light, composing the solar rays, may be increased or diminished without any change taking place in its sensibility to other kinds of light. Thus the spectrum of a red object is of a green colour; because the sensibility of that portion of the retina on which the red rays constituting the image of the object were impressed, is impaired with regard to those rays alone, while the yellow and the blue rays still continue to produce their usual effect; and these, by combining their influence, produce the impression of green. For a similar reason, the spectrum of a green object is red; the rays of that colour being those only that retain their power of fully impressing the retina, which had been previously rendered less sensible to the yellow and the blue ray, composing the green light it had received from the object. Any two colours which, when combined together, produce white light, are said to be *complementary* to one another. It follows, therefore, from the theory above stated, that the colours of these spectra are those that are complementary to the colours of the objects which give rise to them.

A number of curious examples were adduced, of the variations of which the above fundamental fact is susceptible, under different circumstances and in different combinations. The several precautions necessary to be observed, in order to succeed in obtaining these spectral appearances, were

detailed. An account was given, illustrated by numerous drawings and diagrams, of the theory of complementary colours, on which many of the modifications of the phenomena are explicable. Thus, if the colours of the ordinary prismatic spectrum be extended round the circumference of a circle, so that their extremities meet, it will be found that in all parts of that circle the two colours which are diametrically opposite to one another, are complementary colours. Modifications in the colours of the spectra were pointed out, arising from that of the ground on which they are made to appear; for the natural colour of that ground will often combine and blend itself with the impression on the eye, according as it contains rays to which the retina remains sensible.

But even in ordinary vision there exists a constant tendency to the formation of ocular spectra; although the effects of this tendency are, from their fugitive nature, scarcely ever noticed when the eye, as is usually the case, is kept in constant motion. When, however, an effort is made to retain it in a fixed position, the slight movements which it still has, produce, in a short time, the appearance of a coloured border round the object viewed. From the same cause, the spectrum is generally somewhat larger than the original object—that is, it subtends a greater angle.

The judgments we form of the colours of bodies are influenced in a great measure by the vicinity of other coloured objects, which modify the general sensibility of the retina. Many illustrations were given of this principle. When a white or grey object of small dimensions, for instance, is viewed on a coloured ground, it generally appears to assume a tint of the colour which is complementary to that of the ground itself. It is the etiquette among the Chinese to employ paper of a bright scarlet hue in all their epistles of ceremony. Dr. Roget was informed by a gentleman who was formerly resident in China, that he for a long time believed that green ink was employed for writing on this paper, and that he was much surprised on discovering afterwards that the ink was really a pure black, without any tinge of colour. In this case the green appearance of the letters was an optical deception, arising from the tendency of the retina, impressed by the vivid red colour of the paper, to assume the action naturally resulting from green light, which is complementary to the red.

Many curious phenomena of coloured shadows were explained on a similar principle of spectral tendency to complementary colours; and the application of this principle to the art of painting, in as far as harmony of colouring is concerned, was pointed out.

The effect of contrast in modifying impressions is also observable in the case of our other senses, several examples of which were adduced.

The fifth general law of sensation is, that the impression made by an external agent on the nerve of sense continues for a certain time after the action of that external agent has ceased. The influence of this law was traced in each of the different senses; for it is found to extend universally to every case which affords an opportunity for observation. In the case of tastes, or odours, but especially in that of touch, it admits of being detected only when the impressions are sufficiently powerful. Ample proof, however, is afforded of its operation in the case of sound; for the sensation of a continuous musical note arises from the regular succession of aerial undulations; the impression made by each continuing during the whole period of the interval between two consecutive vibrations.

The influence of this law of sensation is exhibited in the most striking manner in various phenomena of vision. The appearance of an entire luminous circle from the whirling of a piece of lighted charcoal, is a familiar instance of this general fact. In like manner, a fiery meteor shooting across the sky in the night appears to leave behind it a long luminous train. Among other illustrations, the instrument contrived by Mr. Wheatstone, and termed by him the *kaleidophone*, was exhibited, and the mode of its action explained. It exhibits to the eye, on the principle of the permanence of sensations, the paths described by the point of greatest excursion in vibrating rods, which often constitute the most beautiful curvilineal forms. A similar method of examining the vibrations of musical strings had previously been resorted to by Dr. Young, an account of which is contained in a paper of his in the Philosophical Transactions for 1800.

The deception which takes place in the apparent figure of the spokes of a carriage wheel which is rolling along the ground, when it is viewed through the intervals of vertical parallel bars, such as those of a palisade, or of an upright Venetian blind, is an example of the operation of the same law. Instead of appearing straight, as they would naturally do if no bars intervened, the spokes seem to be considerably curved. The two spokes which happen to be in the vertical position are seen in their natural shape—that is, straight; but those which are obliquely situated, appear to have a degree of curvature, which is greatest in those which are furthest from the upper part of the wheel. The most curious circumstance is, that the spokes on both sides appear to be bent with their convexities downwards; and this happens equally whether the wheel be moving to the right, or to the left, of the spectator. A certain degree of velocity of

revolution is necessary to produce this visual deception; but the degree of curvature in the appearance of the spokes remains precisely the same, whatever greater velocity is given to the wheel, provided it be not so great as to prevent the eye from following the spokes distinctly as they revolve. Dr. Roget has traced this deception to the law of the permanence of impressions on the retina; for, on examining the circumstances of the phenomenon, he found that the parts of each spoke, that are visible through the bars, leave a trace on the retina, which, when it is referred to the disc of the wheel, has the exact curvature assumed by the spokes. The details of these observations, and of the mathematical properties of the curves in question, are contained in a paper on this subject by Dr. Roget, which was published in the Philosophical Transactions for 1825. Reference was also made to the curious observations of Mr. Faraday, many of which depend upon the same law, and which have been recently described in a memoir contained in the Journal of the Royal Institution.

Another consequence of the same law is, that if a number of different impressions be made on the same part of the organ of sense, within a very short interval of time, a compound impression will result, in which the various impressions are apparently blended into a single one. This single impression sometimes differs materially from the elementary impressions of which it is composed. Many illustrations of this fact were adduced in the case of colours, and also in the total disappearance of the spokes of a wheel which is revolving rapidly. On this principle many visual phenomena were explained; the effect, for example, of the philosophical toy called the *thaumatrope.*

The cause of this permanence in the impressions of sense was next inquired into, as well as the exact time during which they continue after the exciting cause has been withdrawn. In the case of the retina, this time has been variously estimated by different experimentalists, such as D'Arcy, Watson, and others. It admits, indeed, of considerable variation, according to the intensity of the impression. A description was given of the changes occurring in the spectrum of the sun, and of the variations of colour it undergoes, as they have been noticed by Æpinus, who observed them with great care and minuteness. The spectra here referred to are of a different class from those formerly described, and which might be termed the *reverse spectra* of objects, while these are properly the *direct spectra.*

It may be stated as a sixth general law of sensation, that vivid impressions made upon the organs of sense have, after the lapse of a certain time, during which they are not felt, a tendency to spontaneous recurrence, without the renewed action of the external cause. The periodic disappearance and reappearance of ocular spectra are referrible to this law, which, however, is applicable generally to the impressions of all the senses. Direct spectra, after recurring for a certain number of times, assume the characters of reverse spectra, reappearing with the complementary colours of the former.

The law which regulates this succession is greatly modified by the condition of the eye, and also by that of the nervous system generally at the time of observation. These phenomena were minutely described, and the various circumstances which affect them pointed out. They lead to the conclusion that the retina may, in certain situations, be stimulated by one kind of impression to those actions which constitute impressions of a different kind. The impression made by red light, for instance, acting alone, may, when that light is removed and no other light admitted, be followed by an impression similar to what would be produced by green light. The influence of this new principle is combined with that of the former in almost every case of ocular spectra, and affords a key to the explanation of many phenomena relating to the harmony of colouring, or that disposition of them in groups of objects, or in a picture, which produces the most pleasing effect to the eye.

Dr. Roget then proceeded to the consideration of the phenomena which attend the changes taking place subsequently to the first impression made upon the remote extremity of the nerve. The propagation of that impression to the central organ, or sensorium, by means of nervous filaments, he observed, appears to be regulated by conditions of organization and of vital action, similar to those already adverted to as requisite in that part of the nerve which is situated in the organ of sense. Uninterrupted continuity of nervous substance is an indispensable condition; for the division of the nerve, or its compression, in any part of its course, instantly arrests its power of communicating impressions. The conditions of the circulation through the blood-vessels of the nerve are also found to exercise considerable influence on its power of transmitting impressions, as is also the case with regard to its temperature. A comparison was entered into with regard to the different powers of the nervous system, relating to sensation, voluntary motion, and secretion, in as far as they are liable to be affected by different causes of derangement. Facts were stated which afford evidence that the chemical functions of the nerves are more permanent, and are recovered with more facility, than their other functions.

With regard to the nature of the changes taking place in the sensorium antecedent to sensation, although many hypotheses have

been advanced on the subject, we are still, and will probably ever remain, in utter ignorance. The locality of the sensorium has been the subject of much controversy. A statement was given of the different opinions that have prevailed with respect to it; and the inquiry was pursued with relation to the lower animals, among which, as we descend in the scale, we find it more and more extensively diffused over the nervous system, and apparently residing more especially in the ganglia of that system which, in the articulated class of animals, are disposed in a longitudinal series, extending the whole length of the body, until, in the zooyhyta, we lose all trace either of ganglia or of nervous filaments, every part appearing to be endowed with an inherent power of exciting sensation.

The mental change constituting sensation, to which all the changes already noticed are but the preliminary steps, was next considered. It is to be carefully distinguished from the purely corporeal or physical changes which precede it; for it implies the existence of a sentient being, endowed with consciousness, and totally distinct from the material organs with which it is associated. A variety of sensations, of which some examples were given, exist and pass away without leaving behind them any trace, or becoming objects of recollection.

The distinction was then insisted upon between sensation and perception; the latter consisting in the belief of the existence and qualities of the external objects or causes of our sensations, which is itself an inference resulting from the active powers which the mind exerts over its ideas. An account was given of the process by which these inferences are deduced, and of the distinction, as far as our conceptions reach, subsisting between mind and matter; and the incongruity of ideas that is involved in the proposition that thought is a function of the brain, was pointed out. Attention to the real grounds of this distinction furnishes a key to the solution of many questions relating to perception (especially those that concern vision) which have often been considered as difficult and embarrassing.

The sensations derived from the different senses have no resemblance to one another, and have, indeed, no property in common, excepting that they are felt by the same percipient being. But the mind, which receives these incongruous elements, has the power of giving to them cohesion, of comparing them with one another, of uniting them into combinations, and of forming them into ideas of external objects, possessing properties associated with the sensations they have occasioned. These views were developed by Dr. Roget at some length.

The foundation of all our notions of matter and of material bodies, is the idea of space, which appears to derive its origin from the peculiar sensations that attend muscular action. Combined with these are the more immediate perceptions of touch, arising from contact with the organs of that sense, and the various modifications of which unite in giving us the notions we acquire of the mechanical properties of bodies. With regard to most of the secondary qualities of matter, their ideas are derived from the exercise of the other external senses The perceptions arising from affections o the internal parts of our bodies, are generally obscure and vague. The several theories by which such differences in the nature of these perceptions has been attempted to be explained, were considered and discussed.

Great interest attaches to the investigation of the means by which we acquire a knowledge of the existence, situation, and qualities, of distant objects, by means of the senses of sight and of hearing; and the discussion of these subjects occupied nearly the whole of the concluding lecture.

It is evidently from association with the perceptions of our other senses, and especially with that of touch, that sounds suggest to us the presence, situation, and previously-known qualities of the sounding body. The analysis of all the circumstances which guide our judgment on these occasions, is attended with peculiar difficulty. An account was given of the different conjectures which have been hazarded with regard to the means by which we are enabled to perceive the direction in which a sound arrives at the ear; and, in particular, the curious experiments of Mr. Gough and of Dr. Young on this subject, were detailed.

A still wider field of inquiry was entered upon, in the examination of the mode in which the physical impressions made on the retina by the rays of light, collected into their respective foci so as to form upon that membrane an exact picture of the surrounding scene, give rise to visual perceptions. The solution of the questions it embraces involves the application of the laws both of mental and physical phenomena, and is embarrassed by an unusual complication of difficulties; for in addition to those which are the ordinary attendants upon physiological inquiries, we have here to encounter more formidable obstacles, in the perplexing subtleties of metaphysics. It requires a strong effort of mental abstraction in order to divest ourselves of the prejudices resulting from early association, and which have become so rivetted by long habit, as to constitute a second nature, and to be regarded as a necessary part of our mental frame. A careful and patient analysis of our visual perceptions is necessary for the discovery that they include ideas of space which are derived from another sense, and which vision alone would be incompetent to convey.

The conclusion deducible from this inquiry, the details of which would be of too great a length to be given here, is, that the visible appearances which the pictures formed on the retina immediately suggest, could not of themselves, and unaided by the perceptions of the other senses, have given us any notion of the real situations, distances, or magnitudes, of external objects. The primary perceptions of sight and of touch, as Bishop Berkeley has well observed, constitute two distinct worlds, which, though they have in fact a very important connexion and correspondence, yet bear no sort of resemblance to one another; the former having the same kind of relation to the latter that the words of a language have to the things they represent. This theory has received complete confirmation by the circumstances attending the well-known case described by Chesselden, of a boy, who, from being blind from birth, acquired at the age of twelve years the power of seeing, by the removal of a cataract.

Dr. Roget proceeded to the application of this theory to the explanation of a fact which has frequently been supposed to involve considerable difficulty, namely, our seeing objects in their real situation, while the images on the retina by which we see them are in fact inverted. He adduced a variety of instances in which the converse appearance was produced, namely, our having the perception of an inverted object, when the image on the retina was itself erect, as happens in looking through many optical instruments. This effect is also produced when a pin, placed very near the eye, is viewed by means of a small diverging pencil of light, admitted through a small pin-hole in a blackened card, held a short distance beyond the pin; in which case, instead of the usual inverted image being formed, it is only the shadow of the object that is cast upon the retina, in the same relative position as the object itself. This experiment was applied by Dr. Roget to a useful practical purpose, namely, that of enabling a person to ascertain, with the greatest ease, the condition of his own eye with regard to the transparency of the cornea, or the humours. It also affords a method of readily observing the movements of the pupil of the eye of the person making the experiment, by noticing the dilatation and contraction of the luminous circle, or field of view, seen through the pin-hole.

What is generally called the law of projection was next explained, namely, that by which our judgment is guided in estimating the direction of an object seen by one eye only, and which is usually thus expressed: that we imagine each point of an external object to be situated somewhere in the direction of a straight line, drawn from the point of its image on the retina through the centre of the crystalline lens. The modifications which this law receives when both eyes are employed at the same time in seeing an object, were considered at some length, and the hypotheses of Dr. Wells and others on this subject discussed. The theory of corresponding points of the two retinæ was also made the subject of particular inquiry, and the various circumstances under which double vision takes place were pointed out. With regard to the hypothesis that we never see with more than one eye at a time the distinct and simultaneous appearance of two images of the same object, was considered as a sufficient refutation. Whether this correspondence of the two retinæ be dependent on the original constitution of the organ, or be wholly the result of subsequent experience, is another question which has given rise to much controversy.

The curious observations of Dr. Wollaston respecting the evidence of a semi-decussation of the fibres of the optic nerves, derived from cases of partial losses of vision, were stated, and his theory corroborated by several facts with regard to ocular spectra. The singular effects resulting from impressing each of the retinæ with two different colours, were detailed; and the remarkable fact mentioned by Professor Prevost, of a spectrum appearing to one eye from an impression made upon the other eye, was adduced in support of the correspondence of the two retinæ.

The effects of the movements of the body, when rapid, and performed by revolving in a circle, in inducing a state of giddiness and disturbing our perceptions of the situations of objects, were among the instances adduced of confusion in our judgments, arising from unusual circumstances in the exercise of our senses. That we fail on those occasions in applying correctly the principles of visual perception, is also shewn by the strange appearance of a distant prospect when viewed with the head inverted.

Similar fallacies occur in our judgments of the position of objects with respect to the perpendicular direction, when deprived of the ordinary mode of estimating the direction of the force of gravity. Of this many curions instances were adduced.

The circumstances which regulate the judgment we form with regard to the distance of objects, were next enumerated. They are chiefly the actions of the muscles requisite for the adjustment of the optic axes, and the refractive powers of the eyes adapting them to single and to distinct vision; together with the modifications of aerial perspective resulting from the absorption of the rays of light by the atmosphere they have to traverse, and which the painter must represent with fidelity when wishing to convey the idea of distance in a landscape.

In the judgments we form both of the

distance and of the magnitude of objects, our inferences with respect to the one are often determined by our previous or supposed knowledge of the other. Multiplied illustrations were offered of this general principle, in instances both of natural scenery and of the effects of various optical contrivances — such as telescopes, microscopes, and the phantasmagoria. The illusion which takes place with regard to the comparative magnitude of the sun, or moon, when at the horizon, and when considerably elevated above it, was traced to the same law; which affords also an explanation of the mistakes to which we are liable with regard to the direction of the revolution of a wheel, or the sails of a windmill, when viewed obliquely. The idea we form of the convexity or concavity of a surface from its visible appearance, is determined chiefly by the supposed direction of the light which falls upon it, and is reflected to our eyes; and any error that we may commit with regard to the latter, is immediately productive of a fallacy in our perceptions of the former. Thus, if an engraved seal be viewed through a convex lens, at such a distance as to occasion an inversion of the image, the figure will appear to the eye to be raised, instead of depressed. Similar deceptions occur in viewing objects through a compound microscope, which inverts their images. In a picture, in like manner, the very same mode of representing an object may be made to suggest either a concave or a convex surface, according as the mind is led to conceive it illuminated from the one side or the other. This was illustrated by two drawings, superposed on each other, and exhibiting the same representation with the light introduced in different directions, appearing concave in the one and convex in the other.

One of the most curious exemplifications of the influence which collateral circumstances have upon our judgments of the position of objects, occurs in the facts described by Dr. Wollaston, with regard to the direction we are led to ascribe to the eyes of a portrait, from the direction of the other features of the face.

Dr. Roget proceeded to the application of the foregoing principles to the art of painting; the objects of which will often not be attained by effecting a strict delineation of the realities as they are presented by nature. The artist must take into account, in his representations of objects, the nature and laws of our perceptive faculties, and must even study to humour our prejudices and infirmities of judgment, in order to produce the effect he intends.

There is a numerous class of perceptions, of a secondary nature, which have their origin in the very imperfections of our senses; such as those derived from the combinations of certain elements which are themselves not distinguishable as separate objects, or are, at most, very indistinctly perceived. This happens more especially when these elements present either a connected series or successive alternations. Thus general masses of light and shade are often more distinguishable when the parts are somewhat confused than when they are each perfectly distinct. Minute or obscure objects, placed in a line, easily catch the eye, when each individual object would have been scarcely visible. The effects of what are called hatchings in engravings, where different sets of parallel lines cross one another at oblique angles, afforded many curious illustrations of the same general principles. When combined with the motions of different sets of objects, they open a wide field of inquiry, and one which affords interesting confirmations of the laws of perception.

Dr. Roget concluded by an account of the principal sources of fallacy in our perceptions. Most of these, he observed, may be traced to the operation of the very same laws which, in ordinary circumstances, direct our judgment correctly, and to the accidental interference of these laws by unusual and irregular combinations of circumstances.

The illusions of our senses were arranged under three heads, according as they are dependant on causes of a physical, physiological, or mental kind.

The first includes those in which an impression is really made on the organ of sense by an external cause, but in a way to which we have not been accustomed, such as the acoustic deceptions arising from echoes, from the unusual conveyance of sounds, or from the arts of the ventriloquist; the optical deceptions of the looming of the horizon at sea, the mirage of the desart, the Fata Morgana of the coast, the spectre of the Brocken, the phantasmagoria, the kaleidoscope, the suspended images of concave mirrors, and the other innumerable combinations of optical laws.

The second class comprehends those in which the source of deception is more internal, and owing to the peculiar conditions of the sentient organs, such as all those adverted to in the former part of these lectures, in which impressions are made on the nerves of sense by causes different from those which usually excite them. Ocular spectra of various kinds, the impressions on the eye and the tongue from galvanism, and singing in the ears from excited circulation, are among the many perceptions which rank under this head. This class also includes a great number of internal sensations referrible to the law of sympathy, and the endless variety of perceptive hallucinations arising from disordered conditions of the sensorium.

Of the third class of fallacies, including those which are purely mental, and originate in the errors of our reasoning powers alone,

many examples were offered. To this source of error all our senses are liable, but more especially those, such as vision and hearing, in the formation of the perceptions of which association exerts the greatest influence. Even the sense of touch is liable to deceptions of this nature, as in the case of the perception of two balls resulting from feeling a single ball with the fingers crossed.

But limited as our senses are in their range of perception, and liable as they must be to error, we cannot but perceive that both in ourselves, and also in every class of animals, they have been studiously adjusted, not only to the properties and the constitution of the material world, but also to the respective wants and necessities of each species in the situations and circumstances where it has been placed by the beneficent author of its being.

ON THE FUNCTION OF THE MEMBRANA TYMPANI.

By Henry Jones Shrapnell,

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Having described the form and structure of the membrana tympani, (see Medical Gazette, No. 230,) the next point of interest is the function of this membrane in the organ of hearing.

This subject has given birth to several theoretical conjectures, but which, however powerfully they may have been advocated, must nevertheless be submitted to the facts which are demonstrable from accurate anatomical investigation.

M. Magendie, to whom we are indebted for one of our most able works on Physiology, when writing of the ossicula auditus, concludes with the following questions and remarks:—"In what circumstance is the membrana tympani stretched by the internal muscle of the malleus? or when is it relaxed by the contraction of the exterior muscle of the malleus? All our knowledge on this subject is merely conjectural." Again: "the utility of the motions impressed on the bones is entirely unknown*."

Without professing fully to answer these questions, the following observations are made with the view of pointing out some errors, which have been commonly entertained respecting the action of the muscles of the malleus, and the nature of the movements of the ossicula auditus.

With respect to the muscularity of the membrana tympani itself, a difference of opinion exists amongst anatomists and physiologists. Sir Everard Home considers the radiating fibres passing from the manubrium of the malleus to be muscular, and in the Philosophical Transactions for 1825 has figured them accordingly. It may, however, be observed, that it is contrary to the laws of muscular action to be at all times in a state of elastic tension, which is evidently the state of this membrane in many mammalia, if not in man; and it retains this state after death. Also the nerves of the membrane are not visible even with the assistance of a miscroscope, while to the muscles of the malleus they are larger in proportion than to any other muscular structure in the body. Moreover, it may be asked, if the muscularity of the membrana tympani regulates the movements of the ossicula auditus, where is the necessity for the muscles of the malleus to regulate the movements of the membrana tympani? This, however, will be better explained by referring to the muscles themselves, which are attached to the malleus.

These muscles are two in number; the third, which has been delineated by Albinus as the laxator tympani, I believe does not exist; at least, I have never seen it, neither is its existence generally admitted.

The name and office of laxator tympani appears, however, to have been transferred to the externus mallei of Albinus; but as this muscle, from its insertion, can only act as a tensor and not as a laxator, *tensor mallei externus* would be its more appropriate appellation. It will be readily allowed that the part of a bone to which the tendon of a muscle is inserted, is the point at which the contractile force of the muscle will be concentrated; and that the movement produced will accord with the form of the bone and the situation of its ligaments. But while this is applicable to the bones in general and the contractile forces of their muscles, in the ossicula auditus we have to view an apparatus for motions of a par-

* Vide Magendie's Elements of Physiology, translated by Dr. Milligan.

ticular kind, namely, those produced by sounds entering the meatus auditorius externus.

As far as we know, the motions of sound depend upon the physical property of elasticity in bodies, and are evinced in what is termed vibrations. Accordingly, we find that the muscular movements of the ossicula auditus are modified by the elasticity of the parts in connexion with them, and this elasticity, as a cause of vibration, is apparently itself regulated by variable degrees of tension. The elasticity resides in the membrana tensa, the ligaments of the ossicula, and, in early life, probably in the processus gracilis of the malleus: the tension of these elastic parts depends upon the contractile force of the muscles. The particular form of the membrana tensa, of the ossicula, and the situation of their ligaments, confines the effect of their motions to a simple elevation and depression of the manubrium of the malleus in the first instance, and to a similar lever-like elevation and depression of the base of the stapes in the last part of this action. It however appears, that while the effects of delicate or distant sounds are increased at the base of the stapes, those of loud sounds are suppressed. This may be the result of a combined action of the stapedius with the tensores mallei, but such an effect in the former is not clearly apparent. The situation of the muscle of the stapedius is such, that it may act as the most delicate regulator of the tension of the whole chain of the ossicula, but what really takes place beyond the base of the stapes is entirely unknown at present. I believe that the whole apparatus of the ossicula is for no other purpose but to regulate the tension of the parts within the labyrinth, in which the lever of the base of the stapes strains the membranous expansions upon which the auditory nerve is finally distributed. In dissecting the seventh pair of nerves in the calf, I found that the portio dura, in passing from the meatus internus to the canal of Fallopius, reciprocally gave off and received filaments from the nervus innominatus, and that both separately and combined, two of these filaments pass distinctly to the vestibule, combining and entering with the large twig of the vestibular branch of the portio mollis, at the cribriform plate which projects within the vestibule, in the form of a small pyramid or process.

Hence a triple character of nerve enters the vestibule at this point, and forms an additional reason for believing that the ossicula auditus are merely to regulate the state of the parts to which these nerves are distributed. I mention this circumstance to induce those who have leisure for the investigation, and opportunities, which I am not able to command, to pursue the inquiry with avidity, in order that we may arrive at some truth upon so interesting a subject.

To return from this digression to the tensor externus mallei, it may be observed, that although its body is external to the tympanum in the deepest part of the glenoid cavity, yet its tendinous insertion is continued along the processus gracilis, through the glenoid fissure, into the cavity of the tympanum, and terminates with that process in the middle of the anterior side of the neck of the malleus. At this point of insertion, the force of the muscle may be said to be concentrated, although somewhat modified by the elasticity of the processus gracilis itself. This point is directly opposed to the attachments of the suspensory ligaments of the malleus, one of which has been described as passing to the tubercle of the manubrium from the process between the two angles of the circumference of the membrana tympani. The action produced by the tensor externus is to increase the tension of the suspensory ligaments of the malleus, by drawing the base of the manubrium forwards and slightly inwards, whence it is, in some measure, in conjunction with the action of the tensor internus.

The effect of this action appears to be that of directly increasing vibratory motion, and probably ending in the perception of delicate sounds. This particular action may be explained in a rough way, by comparing it with the effect produced by tying a string of catgut around the middle of a pencil, and securing one end of the string to a fixed point, while the other is held in the hand. In proportion to the tension of the string when pulled, will be the force of the reaction of its elasticity, producing a vibratory motion in the pencil, moved by any exterior force. Thus the catgut between the fixed point and the pencil represents the suspensory ligament of the malleus, between the process and tubercle at the base of its manubrium. The pencil re-

presents the malleus, the catgut between the pencil and the hand the tendon and processus gracilis, and the hand the tensor externus muscle, which thus has the effect of increasing the vibratory action of the malleus. The more immediate tension of the whole membrana tensa depends upon the direct action of the tensor internus inserted to the manubrium of the malleus. It will be readily conceived that an extensive range of vibration, probably equal to the whole scale of sounds appreciable by the nerves of the labyrinth, may depend upon the degree of tension produced by the tensor externus. The processus gracilis is so thin and slender, that it probably aids, by its elasticity, in producing the nice degree of tension required. There is a curious circumstance respecting the processus gracilis—namely, that it is seldom or ever to be met with in the adult subject. It appears that it becomes absorbed in proportion to the development of the external meatus, and the room which is gained by that development for the establishment of a perfect groove from the cavity of the tympanum to the glenoid fissure. At birth, and previous to the adult state, this groove is not fully formed, so as to give support in the whole of its extent for the steady action of the externus mallei; the processus gracilis is therefore required to supply this deficiency, and is afterwards probably absorbed, when no longer necessary.

This happens when the glenoid cavity is fully developed, in which state a space is obtained which affords room for the bony groove from the glenoid fissure to the cavity of the tympanum. The varied and beautiful structures impressed upon the different orders of beings by the Great Creator, is, perhaps, no where more apparent than in the comparative anatomy of this particular muscle—the tensor internus mallei. In quadrupeds it is entirely wanting, which circumstance has lately been remarked by Magendie, in the Journ. Physiol. t. i. p. 341, et seq. In birds, however, we find a structure equivalent, or nearly so, to the tensor externus; but they have no membrana tensa. Birds are enabled to articulate various sounds, and to imitate one another, as the parrot and the mocking-bird, &c.; but to quadrupeds the power of speech is denied. I have not had an opportunity of dissecting the ear of the larger species of the ape tribe, but what I have seen in the monkey so nearly resembles those of quadrupeds, that I conceive they must be alike. The great perfection of the human ear, therefore, appears to consist in this additional muscle, imparting a more extensive control over the variations in vibration of sound.

It may here be asked, can consequences of such great moment as the due exercise of reason in the power of speech, giving to man his pre-eminence over the rest of the creation, depend upon so diminutive a part of our frame as a muscle about six lines in length, and scarcely one in breadth? The examination of these structures renders this more than probable, for the nervus chorda tympani takes its course out of the tympanum along with the processus gracilis, imparting filaments to the tensor externus, and then passes to unite with the nerves of the tongue, and through the medium of Meckel ganglion, with the sympathetic.

There is another circumstance which appears to be connected with the action of this muscle. When we listen to distant sounds attentively, the mouth is gently opened. In this action the condyloid process of the lower jaw is tilted slightly forwards, and the temporal muscles are passive, so as to relieve the tensor externus from such pressure as would impede its free action. The reverse of this occurs during harsh and discordant sounds: then the temporal muscles hold the lower jaw fixed, drawing it upwards and backwards into the glenoid cavity. A similar action appears to prepare us for very loud sounds, and particularly in those which are said to set the teeth on edge.

There is also another effect produced by the above position of the mouth. In the act of listening to distant or faint sounds, the noise of respiration is suppressed by the gentle passage of the breath through the mouth and nostrils, and is thus prevented from interfering with those sounds to which we may be listening.

These natural actions are strongly indicative of deep mental attention, and characterise some of the finest expressions of the human countenance, in which both eye and ear concentrate their energies. Such are to be met with in some of the most beautiful of the ancient sculptures, and in the finest

paintings; and being the language of nature, not less beautifully described by our poet, Sir Walter Scott, in his Lady of the Lake :—

> "The maiden paused, as if again
> She thought to catch the distant strain;
> With head uprais'd and look intent,
> And eye and ear attentive bent;
> And locks flung back, *and lips apart,*
> Like monument of Grecian art;
> In listening mood she seem'd to stand,
> The guardian Naiad of the strand."
> *Canto I. stanza* 17.

The suppression of respiration, in aid of the perception of delicate sounds, is also finely expressed sixteen lines farther on, in the same canto :—

> "What though upon her speech there hung
> The accents of the mountain tongue;
> Those silver sounds, so soft, so dear,
> *The listener held his breath to hear.*"

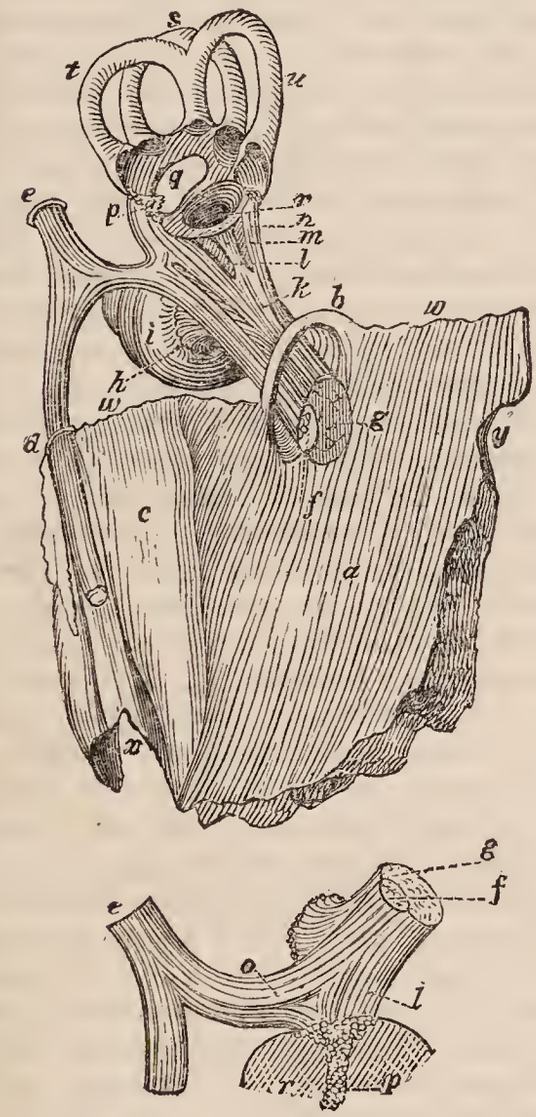

Fig. 1,—*Nerves to the Labyrinth of the Human Ear.*

a, posterior surface of the petrous portion of the temporal bone, looking into the base of the cranium.
b, entrance of the meatus auditorius internus, with the 7th pair of nerves passing into it.
c, anterior surface of the petrous portion of the temporal bone.
d, foramen innominatum, with the nervus innominatus entering it.
e, commencement of the canal of Fallopius, with the united filaments of the portio dura and nervus innominatus entering it.
f, portio dura.
g, portio mollis.
h, branch to the cochlea.
i, the cochlea.
k, branch to the vestibule.
l, large twig making a plexiform union, with filaments from the portio dura and nervus innominatus to enter the vestibule at the pyramidal cubriform plate, *p*.
m, middle twig.
n, small twig to vestibule, terminating in cribriform plates.
q, the foramen ovale, opening from the internal wall of the cavity of the tympanum.
r, entrance to the scala vestibuli of the cochlea.
s, horizontal semicircular canal.
t, superior verticle ditto.
v, posterior vertical ditto.
w, line of separation from the bone cut away.
x, internal opening of the foramen caroticum.
y, entrance to the aquæductor cochleæ.

Fig. 2.—*The same parts as in Fig.* 1, *from the Calf, with the same References.*

p, the cribriform plate, or pyramid, in this animal, extends half across the cavity of the vestibule, dispersing its filaments upon a membranous plate, which unites with the membranes of the semicircular canals.

H. J. S.

19, Russel-Street, Brixton Road.

ON BLOODLETTING.

To the Editor of the London Medical Gazette.

SIR,

SHOULD the following observations on bloodletting, which possess few claims to originality in themselves, merit a place in your publication, they may possibly call the attention of those whose opportunities of observation are more extensive than mine, to a subject of great importance, and one over which, I regret to say, empiricism exercises an almost unlimited control.

Of the utility of bloodletting in acute diseases, there is, I believe, but one opinion; but the quantity to be abstracted will depend much on the cause, the case, and the strength of the pa-

tient: and as, in bleeding, our object is not merely to draw off 20, 30, or 40 ounces, but to produce a determinate effect, either on the disease or the pulse, we must not desist until we have obtained it. Circumstances will sometimes occur which justify us in stopping the bleeding, such as nausea, convulsions, shivering, vomiting, &c. Syncope, which frequently arises from a peculiar idiosyncrasy, and not from depressed vital energy, will of itself check it: this is to be lamented, as it prevents the abstraction of a sufficient quantity. When a large bleeding is unattended by any sensible effect, either on the pulse or the disease, it would be prudent to tie up the arm for a short time, and its sinking or depressing effects may become apparent; but if, after an hour or two, no symptoms of improvement manifest themselves, more blood may and ought to be drawn off.

Men, generally, *(cæteris paribus)* bear bleeding better than women,—the inhabitants of cold climates better than those of hot: children, for the most part, bear it badly; they will not bear it with the same freedom in proportion to their age: this is a remark applicable to old men. To fix the precise quantity that should be drawn, in any disease, is a question that can be settled only by the attending physician. So much as 100 ounces have been taken from a patient in pneumonia in 24 hours, 70 from another in 12 hours, 80 from a third in 4 hours, 127 from another in 27 hours, 100 of which were taken away in the first 5 hours, and all with the best effect. There are instances where patients have lost, in three months, 392 ounces, and have done well. Mr. Cline related a case where a patient lost 320 ounces in 20 days. Sydenham considered 40 ounces as the average quantity necessary to be taken away in pleurisy; but all this only serves to show how far, in particular instances, we may bleed with safety, and not as a guide for general practice. It may be said, that, speaking generally, 25, 30, or 35 ounces, is a full bleeding, after which it is right to delay, for two or three hours, according to the urgency of the case, its repetition; in dangerous cases, patients will bear a repetition of it three times in 24 hours, and once or twice in the day for two or three days after. With regard to bleeding in children, Sir Charles Clarke makes some judicious observations. A child, he says, from 3 to 9 months old, may safely lose 2 ounces at the first bleeding, and 1½ ounce in 24 hours after; and, perhaps, in 12 hours more the same quantity; and, for every additional year an additional ounce. Thus, three ounces from a child of one year, four for a child of two years, five for one of three years, an so on to the age of eight, when they bear bleeding very well.

The indications for bleeding are, a full, hard, tense pulse, and the propriety the buffy coat, or at least a ground for its repetition. The pulse, though a leading feature in the indication for bleeding, should never be our sole guide; it will often deceive; neither is the buffy coat an unerring proof of inflammatory action, though it accompanies almost all cases of inflammation, and may thus far, by its appearance, be satisfactory. Frequency of pulse does not always indicate inflammation; it is often the concomitant of debility. In hot climates, there is a sharpness of pulse without frequency, arising from increased excitement. In inflammation of the bowels, the pulse is generally small; but, in such cases, we can generally detect a degree of hardness. In pneumonia, carditis, and hydrocephalus, we often find an intermittent pulse; but rarely in abdominal inflammation, except as a fatal symptom. The pulse is sometimes fallaciously full, chiefly in women, children, and old people; in the two former it seems to depend on preternatural irritability, in old people on diminished circulation in the capillary system or the minute collateral ramifications on the surface of the body, and perhaps also on diminished power of contractility in the arteries, so that they do not perfectly empty themselves at each systole. The buffy coat has been considered as a distinguishing mark of active inflammation, and, though it frequently accompany, is by no means an invariable attendant on it. It is absent in some cases of highly active inflammation, and is often observed where there is little or none: it sometimes appears in the first cup very evident, less so in the second and third, absent in the fourth; but, at other times, all this has been reversed. A remark has been made, that the cases in which these appearances are not seen in the first cup, but become manifest in the third and fourth, are those in which the

pulse rises during bleeding; but on this point there is still much doubt. It has been supposed to depend on the slow coagulation of the blood; that this has some influence in producing the buffy coat is perhaps true, but is not the cause. The buff appears sometimes in a high degree where the coagulation has been most rapid, as in a minute or two; and it may be seen in cases where a drop of blood is allowed to trickle over a plate, forming a thin flake, during the coagulation of which, gravitation of red globules could have no effect. It is often noticed in that state of the system which precedes apoplexy or incipient palsy, in cases of active aneurism of the left ventricle, and often through the whole course of pregnancy, when the health has not been particularly disturbed. The cupping of the blood, which appears to be the effect of increased cohesion, has been looked upon as indicative of the highest degree of inflammation, and is, for the most part, an attendant on it. When we bleed in cases of supposed inflammation, it is satisfactory to find the blood cupped and buffed; but the mere presence of these is not an indication for drawing off more blood. The buffy coat will often continue where the powers of the system cannot bear further depletion, as when inflammation terminates in suppuration. A dissolved state of the blood, such as occurs in hæmorrhœa petechialis and in some typhoid fevers, has been considered as contra-indicative of bloodletting; but many cases occur where the buff appears under the use of the lancet. Bloodletting has been employed in almost all descriptions of fevers, dropsies, diabetes, diseases depending on local or general plethora, or in what has been called increased momentum of blood to any particular part. In cases where local inflammation, however confined, hurries on the destruction of parts, it may require as active treatment and as copious bleeding as when the most important organ is attacked, as frequently happens in diseases of the eye, when that organ is affected by any of those terrible forms of disease,—the Egyptian, the purulent or gonorrhœal ophthalmia.

I remain, sir,
Your obedient servant,
RICHARD BURKE, M.D.

4, Bolton-Row, May-Fair,
May 16, 1832.

OBSERVATIONS

UPON

THE DIFFERENT MODES OF APPLYING A LIGATURE FOR THE REMOVAL OF POLYPUS UTERI.

To the Editor of the London Medical Gazette.

SIR,

As the pointing out of a simple and effectual mode of performing an ordinary operation, is, perhaps, not less useful to the profession than the history of the successful treatment of a rare and dangerous complaint, I beg to submit to your readers a few observations upon the operation of applying a ligature for the removal of polypus uteri, with a description of an apparatus for the purpose, which I have several times used, and have found free from those inconveniences which are met with in the use of the instruments commonly employed. In order to explain the advantages which attach to my own method of performing the operation, I shall briefly state the modes in general practice, and the inconveniences to which they are liable; and then describe the plan which I have adopted. The operation is generally performed either in the manner recommended by the late Dr. Gooch or by that described by Sir Charles Mansfield Clarke, in his work upon female diseases. In the former, two straight silver tubes are used, each about eight inches long, separate from one another, and open at each end. A strong ligature is passed up the one and down the other, so that the middle of the ligature is at their further end. These tubes, placed side by side, are passed to the neck of the polypus, and, when fixed at the part chosen for operation, are separated—one being carried round till it meets its fellow again; by which means the ligature is drawn round the neck of the excrescence. The two tubes are now joined together by two rings attached to a slender rod, which rings slide over the tubes, and fix them together at their upper end, whilst, at their lower end, they are secured by passing into a joint, about an inch long, formed of two tubes soldered together, to which two small loops are attached at the sides. By drawing the ends of the ligature at the lower ori-

fices of the tubes, and then tying them to the side loops, the noose round the neck of the polypus is thereby tightened. The objections to this mode are, in the first place, the difficulty of carrying the tube round so as to meet its fellow, and be fixed by the rings; not to speak of the necessity of a *straight* tube which cannot adjust itself as conveniently to the globular shape of the polypus as a *curved* one. In the next place, the tubes being eight inches long, and consequently much longer than the passage of the vagina, project beyond the external parts, to the great danger of the impalement of the vagina, or uterus, upon the upper ends of the tubes; an accident which is said once to have occurred in the operation, and to have caused death: and though this danger may be avoided by care, yet it subjects the patient to a disagreeable and inconvenient caution and position. In the other mode of operation, recommended by Sir Charles M. Clarke, the ligature is passed to the neck of the polypus by a brass rod, with a loop at the end, through which loop the ligature is drawn to the middle of its length. One end of the ligature is then wrapped round the rod once or twice—the other is left loose. The loop of the rod is carried to the stalk of the excrescence, and when there, the finger of the left hand of the operator is fixed upon a portion of the ligature, whilst, with the rod, the loose end is carried round. The two parts of the ligature are now to be held by the finger upon the stalk of the polypus, and the rod is to be withdrawn. The ends are next drawn through a single tube of eight inches long, by means of a hooked wire, and then are firmly tied to two loops attached to the lower end of the tube. The same objection, as to the danger of impalement, lies against this tube as against those of Dr. Gooch; and although the late Dr. John Clarke contrived a wooden shield, to screw upon a thread cut on the outside of the tube, I have found it very troublesome to the patient; and there is this inconvenience attached to it—that it cannot be removed without withdrawing the tube. In the above accounts, of course, I have been obliged to copy, in great part, from the works of the gentlemen whose methods I have described. The plan which I have adopted is the following one:—I have a strong cord, prepared of twisted silk, with two small loops at the ends. This cord is slightly waxed. Placing the middle of the cord across the end of the fore-finger of my right hand, and holding it there by the two ends grasped in the hand, I carry it to the neck of the polypus, and then hitch the cord over the body of the polypus by the point of my finger carried each way. It is not necessary to conduct the cord more than half, or three-quarters, round the stalk, as the tube will do the rest. The rod I have found very troublesome and unmanageable; and it has also been found so much so in the hands of others, that I have been told by an eminent practitioner that occasionally this step of the operation occupies half an hour. In every instance in my own practice (and I have repeatedly performed the operation) the application of the ligature has not taken more than a minute's time; nor can I understand the difficulty which has been alluded to and experienced by authors who have written on the subject. The globose shape of the excrescence retains the cord when once pitched over. I next fix the loops of the cord to the hook of a wire passed through a tube, and so draw them to the lower end. The tube I use is about eight inches long, curved at the upper end, and jointed like a flute in the straight part, each joint being an inch long. Of these joints there are four, and they all fit to the lower one, to which the side loops, or rings, are attached. By this contrivance, I can accommodate the length of the tube to the length of the vagina. This is a great convenience, especially after a day or two, when the operation is apt to drag down the uterus. There is no fear of impalement, nor any necessity for extraordinary caution in turning in bed. The rings upon the lowest joint are not placed longitudinally as to the tube, but transversely, for a purpose to be hereafter described. The cord being drawn through the jointed tube, is tied firm upon a strong small ivory ring, one end being passed through the ring. This little appendage saves much trouble in passing the cords through the loops or rings of the tube, which are apt, besides, to chafe the ligature. In tightening the ligature some force is necessary, which, in case of a slip, might injure the vagina, by thrusting the tube violently against it. I therefore use a small ivory shield, about the size of a crown-piece, with

perforations in it; through which perforations I pass a twine, and tie the shield to the transverse rings. An assistant steadies the tube by holding this shield whilst the cord is tightened daily. I think, if any gentleman will try the jointed tube and ring, he will find them very convenient; but as my description may not be very clear, I have ordered a specimen to be made by Messrs. Evans, the surgical instrument-makers, of Old Change, London, where the apparatus may be seen, and, should it be found beneficial, I shall not regret having occupied so much space in your pages on, apparently, so trifling a matter.

I am, sir,

Your obedient servant,

A. X.

SALINE INJECTIONS IN CHOLERA.

Queries *put by the* Central Board of Health *to* Dr. Lewins *relative to Saline Injections into the Veins in Cholera, with the Doctor's* Replies *to the same.*

Queries.	*Answers.*
1. Were any of your patients bled previously to, or after, the saline injections into their veins?	1. None before; one to the amount of twelve ounces immediately after the first injection.
2. Were the evacuations by purging, vomiting, or perspiration, increased by the injections?	2. The evacuations by purging and vomiting, in most of the cases, continued. In some of them, the purging, the discharge from the bowels at least, was increased. Perspiration was increased in all.
3. Did any of the patients submitted to the saline injection plan die; and, if examined after death, what were the appearances?	3. Yes; no less than ten of the fifteen that have been injected up to the present day, but under such circumstances as do not detract from the general merits of the practice. This will be made evident by the history of the cases that will be sent by to-morrow's post.
4. Had the pulse at the wrist absolutely ceased, and for how long; or had blueness of the surface taken place, and to what extent, in any of your patients, before the injection of the saline fluids: and how many of such patients recovered under that treatment?	4. Yes, and for hours, even at the axilla: in some of the cases, blueness of the surface had taken place to a considerable extent. Five of these patients recovered.
5. Had suppression of urine been *perfectly* established, and for how long, in any of your cases previously to the saline injection; and what effect did that practice appear to produce on the urinary secretion?	5. Complete suppression, I think, in all, except two, and for hours. In all the successful cases, the effects of the injection in restoring the secretion of urine was most evident.
6. What effect did the injections appear to have on the temperature of the patient?	6. The injections raised the temperature of the body; but in all the successful cases, when the veins were injected, the patient complained of cold soon after the injection.
7. Were the blood and evacuations analyzed before and after the injection?	7. Neither the blood nor the evacuations were analyzed; but I sent some of the blood of a patient that had been injected by the veins to Dr. Reid, for analysis to-day.

8. Did consecutive fever occur in any, and if so, in how many of your cases, whether successful or otherwise?

9. Was the quantity of the evacuations noted before and after the injections in any of your cases?

10. Please to give the details of two or three cases treated by saline injections, with age, condition of life, temperament, habits, &c.; and particulars of such other treatment as may have been adopted, in addition to the saline injections.

8. The consecutive fever in all the patients who were injected, has been slight.

9. No; but they were excessive in most of the cases.

10. Question ten shall be fully answered by to-morrow's post.

(Signed)
ROBERT LEWINS, M.D.

6, Quality-Street,
May 26th, 2 o'clock, A.M.

[In addition to the above, a detailed statement has been received by the Central Board from Dr. Latta: it is in substance the same as Dr. Lewins' letter, published in our last number. Another letter has also been received from Dr. Lewins, together with two cases in which injection into the veins was practised by Dr. Craigie. Some of the deaths, where the patients did not recover, would seem to have been the result of phlebitis; but we shall probably make room next week for such parts of the above documents as are calculated to throw additional light on this interesting subject.]

INTUS-SUSCEPTION.

To the Editor of the London Medical Gazette.

Moulton, near Spalding, 5th May, 1832.

SIR,

As a subscriber of the Medical Gazette, and who appreciates its value, and the impartial spirit with which it is conducted, I beg to present to you, sir, and your readers, with the report of the underneath case, should you deem it of sufficient practical importance for a space in its pages. Expressing my opinion of the practicability and service of reporting cases generally, whether the treatment of them be successful or not,

I am, sir,
Your obedient servant,
THOMAS WATSON.

The following symptoms led me to believe in the existence of intus-susception in the case of my patient, Mary Pepper, ætatis 34, living in the parish of Whaplode. She married at the age of sixteen, and has been the mother of nine children; for a very long period has enjoyed but a delicate state of health, owing to a very irregular and disturbed state of the stomach and bowels. My attendance was first demanded on Wednesday, the 18th inst.; and on visiting her, she informed me that she had been under medical treatment, but that the bowels had not then been relieved during the last fortnight. She had also just vomited, and the smell of the ejected matter demonstrated that she was vomiting vitiated fæces, which she also thought had been the case for three or four days previously, and which I thought very likely, as there not only then appeared to be the contents of the stomach, but also the contents of the bowels. The stomach, owing to incessant sickness, had not retained any thing for several days. The pulse, and the state of the tongue, actually indicated a typhus condition, and the general symptoms shewed that she was in an apparent sinking state, the body being extremely distended and tympanitic. I observed, in my examination, that when sickness and the spasmodic colicky pains attacked her, (which they did very quickly after each other) the convolutions of the ileum, and particularly of the colon, were actually discernable, and distinctly traceable through the abdominal parietes; indeed, I never saw the phenomenon so distinctly before. I resolved that my treatment at the onset should be principally calculated to soothe, to support, and rally my patient. Boluses were prepared with G. Opii, gr. ss. Camphoræ, gr. iv. Hyd. Subm. gr. j. Ol. Crotonis, gtt. j. with sufficient mucilage, and directed to be given every hour, or every hour and a half, with about a tea-spoonful of brandy in the like quantity of water, no gruels or other liquid to be taken until about half an hour after each bolus. By these means, some

of the boluses were at times retained. Combined with this treatment, the common enemata, with oil, were also administered regularly every four hours until late on the Friday evening following, when, finding that these means did not effect the wanted action on the bowels, I injected per rectum, (with Read's syringe) a very large quantity of water, *about three quarts*, as warm as the bowels could bear. On my visit early on Saturday morning, I injected again more than three quarts of warm water, when in the afternoon I had the satisfaction of learning from her husband that she had experienced some relief, and that a very small quantity of motion was evidently mixed with the last portion of the water which I had injected per rectum; some air also had escaped with relief. This report only caused me to omit the calomel, desired that the boluses might be repeated as usual until I should see her the following morning, unless the wished-for effect on the bowels should be accomplished in the meantime.

On visiting her early on the Sunday morning, the 22d inst. I learnt that the bowels had been relieved three times, though in very small quantities. But the report of the husband at 4 P.M. was, that the bowels were then acting every hour, and that she had vomited the fæces but once since the second motion downwards. I had then only to prescribe for the poor woman, brandy, sparingly, and boluses, with opium, camphor, and small quantities of calomel, to be repeated about every two hours and a half, with the common effervescing mixture and lemon-juice, and which she continued to take for the next two or three days. I have happily now to observe, that the poor woman's convalescence has continued progressive under the usual restoratives, and that the undeviating perseverance in the soothing treatment, which was throughout my object and design in this apparently hopeless case, has proved entirely successful. Should I meet with another case, the treatment employed here had apparently such valuable impression, that I should probably again be induced to adopt it.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Microscopic Cabinet of Select Animated Objects; with a Description of the Jewel and Doublet Microscope, Test Objects, &c. To which are subjoined, Memoirs on the Verification of Microscopic Phenomena, and an exact method of appreciating the quality of Microscopes and Engiscopes: by C. R. GORING, M.D. With 13 coloured plates, and numerous wood engravings. By ANDREW PRITCHARD. 1832. Whittaker & Co.

THE contents of this volume are of the most interesting nature. The first portion of it is devoted to the description of certain of the aquatic larvæ of insects, several of the crustaceæ, and infusory animalcules. This is followed by an account of those admirable instruments, the microscopes constructed with precious stones, by which a new world has been recently opened to the view of the philosophic inquirer. A vast variety of practical information, popularly detailed, yet in accordance with exact science, is diffused throughout the work; and the whole is illustrated by a number of very beautiful plates, from the drawings of Dr. Goring, whose reputation as an artist is only rivalled by his merits as a first-rate scientific optician.

It is needless for us to dilate on the advantages which may confidently be expected to arise to medical science from the use of the microscope, as recently improved. The mysteries of animal structure have yet to be extensively unveiled; and the wonders which the "glazed optic tube" developed in the boundless field of astronomy, are known to have their ample counterpart in the regions of structural anatomy. Without both, the study of the vast and the minute, it is not possible to have adequate notions of the glories of the creation.

The "Microscopic Illustrations" of the authors of the present work are already known to the public, and have placed the names of Pritchard and Goring at the head of the list of those whose attention has been directed to the improvement of the microscope. Sir David Brewster has done much, both speculatively and practically, with this

design; but he candidly waives the palm of practical excellence in favour of the authors before us (Treatise on Optics—Cabinet Cyclopædia); and, indeed, in their hands the microscope would seem to be not so much a merely improved as a newly-invented instrument.

It was Dr. Brewster who first pointed out the valuable properties of the diamond for microscopic purposes, as the reader may see in the Treatise on New Philosophical Instruments, published in 1811; but Sir David seems never to have contemplated the possibility of working that refractory substance into magnifiers. "In the summer of 1824," says Mr. Pritchard, "it was agreed upon, between Dr. Goring and myself, that I should undertake to grind a diamond into a magnifier. For this purpose, Dr. G. forwarded me a small brilliant diamond to begin upon; and it was proposed to give it the curves that, in glass, would produce a lens of a twentieth of an inch focus with the proportion of the radii of their surfaces as two to five. This stone I ground with the proper curves, and polished the flatter side, contrary to the expectations of many whose judgment in these matters was thought of much weight, who predicted that the crystalline structure of the diamond would not permit it to receive a spherical figure. When thus far advanced, fate decreed that I should lose the stone; and my only consolation was to discover afterwards, that, had it been completed, its thickness and enormous refractive power would probably have caused the focus to fall within the substance of the stone." * * * * "On the 1st of December, 1824, I had the pleasure of first looking through a diamond microscope, and it was, doubtless, the first time this precious gem had been employed in making manifest the hidden secrets of nature." But there were, unfortunately, various flaws discovered in this stone, and the project of its completion was in consequence given up. After the lapse of some time, not discouraged by his losses and failures, Mr. Pritchard again resumed his attempts, and "having worked several stones into lenses," at last succeeded in obtaining a perfect one.

We regret that we cannot give, in all its detail, the interesting history which Mr. Pritchard writes of his successful formation of diamond lenses; but we strongly recommend the perusal of it to the reader in the volume itself. We are also obliged, through our limited space, to barely allude to the other valuable contents of the work—the many original remarks which are to be found in the chapters on the larvæ and animalcules, and the scientific observations of Dr. Goring on the nature of test objects, and the verification of microscopic phenomena. But we will say, without any hesitation, that the "Cabinet" is decidedly the most practical and satisfactory treatise on the microscope, that it has hitherto been our good fortune to have met with.

The Dissector's Guide; or Student's Companion. Illustrated by numerous Wood-Cuts, clearly exhibiting and explaining the Dissection of every Part of the Human Body. By Edward William Tuson, F.L.S. &c.

Those who have seen Mr. Paxton's "Introduction to the Study of Human Anatomy," will at once perceive that Mr. Tuson's "Guide" is constructed on the same plan. The idea of introducing figures in the page, by which the eye may be appealed to in aid of the letter-press, is certainly good; still, whether it be useful must depend on the manner in which it is accomplished. The point in question is common to both works, but here the resemblance ends. Mr. Paxton's wood-cuts are almost equal to engravings on copper, and are really among the best specimens of the art in a department which has been prodigiously improved within the last few years; Mr. Tuson's, on the other hand, are the most wretched it is possible to imagine, and we did not conceive that, at the present period, an artist could have been found capable of doing any thing of the kind so badly. But inasmuch as it is in the addition of such cuts in the descriptive part that the peculiarity of the work consists, we think Mr. Tuson would have done well had he taken care that they were a little more decently executed; and certainly no degree of accuracy in the descriptions can compensate for the coarseness and confusion of the delineations. In another edition we recommend to the author to have fresh cuts by some skilful engraver, and his work will then become creditable to himself, and useful to his pupils.

MEDICAL GAZETTE.

Saturday, June 2, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

THE HOMŒOPATHIC SYSTEM.

SYDENHAM'S advice to a student of medicine, to read "Don Quixote," is one of those apocryphal traditions which have been preserved more for their wit's sake than for their wisdom. What is told of Radcliffe rests upon better authority. When that facetious, but able physician, was yet a student of University College, and preparing for his first degree, he was asked by Dr. Bathurst where his library was: Radcliffe pointed to "a few phials, a skeleton, and an herbal." John Hunter made a bolder avowal: when asked where were his books, he said that he read but in one book, and that was the "book of nature."

Medicine has undergone many revolutions: it has been reformed but seldom: thoroughly, never. No grand reformation like that which occurred in the religious world in the sixteenth century has ever yet been effected in medicine, nor is it probable that it ever can. The "Book" in which we have to search for the sure foundations of our professional faith, is one that contains within it the substance of all that has ever been, or ever will be known, and is so boundlessly extensive that mortal man can never read it through. Hence what is to be counted heresy, and what is not, it is most difficult in medical science to define: the standard of orthodoxy, though contained in a volume which is open to all, is yet so widely diffused through that immense volume, and so materially dependent upon inductive processes, for which not even the industry of fifty successive *Cuviers* would suffice, that it is hopeless ever to have it for familiar application in the testing of truth and falsehood.

One might be sometimes disposed to think that what the ancient philosopher so candidly confessed about himself—that he knew but one thing, and that was, that he knew nothing—might, with some semblance of truth, be honestly predicated of medicine. There are, however, *a few* certainties about the medical art, and one of these is, that the said art is wholly conjectural, and conjectural must it ever remain. And here we have an illustration of the exceeding value of certainty of almost any sort. This truth, of the conjectural nature of medicine, simple as it appears, and barren apparently and negative in its results, affords us perhaps the best test of medical heresy that we can hope to possess. Every pretended reformation of which we have any record in medical history has gone upon the principle of setting medicine upon a basis superior to conjecture; and the world, in its simplicity, has often given credence to false prophets; but after being amused from time to time, through the lapse of many ages, it is at last grown more cautious and discerning, and of late years the preachers of a medical millenium have had no sort of encouragement whatever.

We are not aware that Hahnemann has laid claim to any such mission as that of setting medicine upon a certain and sure foundation, independent of the exercise of conjecture: if we were, we should certainly pass him by as a quack, unworthy of any notice, except, perhaps, such notice as would tend to his exposure. And we may mention, that the only reason why we have taken up this subject of the homœopathic doctrines (for we would not have our motives for a moment misunderstood) is simply, because we think that they really deserve to be better understood in this country, and that the acumen of British sagacity might be not ill employed in investigating their truth or

falsehood. With this view, and in order to give every possible fair play that an impartial statement can afford to opinions about to be made the subject of discussion, we shall make no apology for laying before our readers an ample abstract from the *Organon* of Dr. Hahnemann,—indeed, so much of it, as would seem to contain the very sum and substance of his principles. The work commences with a sort of mathematical precision: and we shall observe the systematic form of the consecutive dogmas.

1. The business of the physician is to cure the sick.
2. Three things are necessary thereto: 1. An exact investigation of the nature of the malady; 2. To determine the agents which should be employed; and, 3. To employ them so skilfully, as that health may be the result of their adoption.
3. Those medicinal agents are to be preferred which are at the same time easy, quick, and durable, in their effects.
4. The object to which the physician's treatment is to be directed, is not any thing occult or undiscoverable in the patient's system, but the *ensemble* of the perceptible changes which the malady has introduced; in short, the totality of the symptoms.
5. What is called a malady, is made up of those occult and those obvious changes which occur in the system when out of health; and that treatment which removes the latter, necessarily removes the former changes.
6. The true nature of medicaments can only be determined by the changes which they produce in the animal economy, and not by their physical or chemical effects.
7. From the employment of certain medicaments, the re-establishment of health has sometimes so manifestly resulted, that it would seem vain to seek for the cause elsewhere; and it is not strange that men should thence derive their conclusions relative to those substances, and be induced to employ them in like cases: but this mode of proceeding is uncertain.
8. With the exception of a few maladies arising from miasms and certain viruses, every disease is an individuality which must be considered as new and peculiar; and a medicine that is found salutary in any one malady, cannot be employed in another merely resembling it: the only way of ascertaining the true value of a remedy is by observing the development of its action on the healthy body.
9. Every medicinal substance applied to the animal economy produces in it certain organic changes: it alters the condition of health, and gives rise to artificial disorders infinitely varied.
10. The development of the active powers of a medicament presents two very different results, according as it is applied to the body in sickness or in health: in the former it is curative, if the indication have been properly observed; while in the latter it is pathogenetic, and only becomes remedial as it possesses that quality, namely, of producing disease.
11. Since diseases are only appreciable by their symptoms, and medicines by their pathogenetic qualities (or the perceptible changes which they produce in the healthy body); it follows, that the general principle of the treatment of disease is founded in the relations which exist between the symptoms and the pure effects of the remedies employed.
12. There can be but three possible relations of this sort, *heterogeneity*, *opposition*, or *resemblance*: hence, severally, the *allopathic*, *antipathic*, and the *homœopathic* systems. Experience alone must determine their respective merits.
13. In the *allopathic* method,—the method most in vogue,—there are but three chances which are possible; 1. That the maladies produced by the medicines may be *less* violent than the sufferings for which they are prescribed,—in which case no radical change is effected: 2. That the morbific effects of the medicament are *equally* or *more strong* than the disease; in which case the disease is suspended as long as the treatment lasts,—when it will most probably reappear, unless in the meantime it has run its natural course:

and, 3. That the remedies, if violent and long continued, as in chronic cases, give rise to complications of disorders,—whence there may come two or several maladies, having each a distinct place in the system.

14. *Allopathic* treatment cannot cure in any case: having no analogy, or opposing force, to the symptoms of the disease, it can never reach the parts affected: it may suspend the symptoms for a time by heterogeneous sufferings, but it cannot destroy them.

15. *Antipathic* treatment is merely palliative. When the action produced by the remedy employed, and which may seem to effect a neutralization of the symptoms or even a cure, ceases, the reverse process immediately takes place,—not only shall the primitive malady return, but come it will with aggravated symptoms, and in proportion to the doses administered.

16. This aggravation arises from the action of the system, which always has a tendency to present a condition exactly opposed to that which is attempted to be produced upon it,—consequently to react in the sense of the primitive disease, or to develop and aggravate the symptoms. As a palliative, however, the antipathic system is sometimes useful, nay necessary.

17. The *homœopathic* method is the only one which experience proves to be always salutary. The pure and specific effects of the remedies employed being perfectly analogous to the natural symptoms, they go right to the parts affected; and as two similar diseases cannot exist at the same time in the same system, the natural symptoms give way, provided the artificial ones slightly surpass them in intensity.

18. The artificial disease, having only a duration limited by the action of the remedy, when it disappears, leaves the system perfectly sound; and the more so, as the reaction of the system against the remedy is at the same time directed against the symptoms, and is as much conducive to the re-establishment of health, as on the antipathic plan it is opposed to it.

19. It is consistent both with experience and reason, that the homœopathic relations between maladies and medicaments are the only ones that lead to prompt and permanent cure; whence we derive the following precept:—"*Cure your sick by those remedies which are capable of producing, in the healthy body, effects as like as possible to the totality of the symptoms which you have to treat.*"

20. Homœopathic remedies, as they exert all their action upon the parts affected by the disease, had need to be given in doses infinitely minute: the action required for overcoming the intensity of the disease is extremely small,—a great one would be injurious, or even dangerous.

21. No more than one medicinal substance is to be employed at a time: a complication of medicaments is inadmissible,—for it is impossible to determine how, in those cases, the different ingredients modify each other; and still more difficult to define the relations of the symptoms of the medicament with those of the malady.

22. It is the bounden duty of the physician to be sure that the remedies administered to his patient are of good quality, and in exact proportion: he should prepare them and exhibit them himself.

Such is the creed and confession of faith of every true Hahnemannian believer. But before we proceed to offer any observation on the preceding dogmas, we have something more to say, in addition to what we have said already in our former paper on the history of the Hahnemann principles. With regard, in the first place, to the origin of homœopathy, it is rather a curious fact that Hahnemann himself disclaims his having been the first to broach the system. The fundamental principle of exciting an ὅμοιον πάθος for therapeutic purposes, (whence the system derives its name,) he holds to have been acted upon even so early as the time of Hippocrates; and among the proofs adduced in favour of this position, he mentions the cure (stated in the treatise of Epidemics) of cholera by white helle-

bore. Senna for colic pains, rhubarb for diarrhœa, thorn apple for madness, and belladonna for hydrophobia, are other examples of ancient homœopathy: and descending to times more modern, the cure of the sweating sickness by sudorifics, the treatment of frozen limbs by rubbing the parts with snow, and of scalds and burns by exposure to the fire, are given as still more popular illustrations of prevalent ideas in behalf of the doctrine. It has been suggested that even Shakspeare himself (that *omnia novit* personage) positively bears testimony to the truth of homœopathy, in those lines where he says,

"Tut! man, one fire burns out another's burning,
One pain is lessened by another's anguish;
Turn giddy, and be holp by backward turning;
One desperate grief cures with another's languish.
Take thou some new infection to thine eye,
And the rank poison of the old will die!"

But however this may be, Hahnemann, as we have said, by no means pretends to any discovery of the principle, *similia similibus curantur*: he only takes merit to himself—and no small share of it—for having organized into a regular medical constitution the materials which he found going afloat about the world, and for having seasoned, by the experience of thirty or forty years, the fabric which he first so ingeniously put together.

What the great Homœopath looks upon as his grand discovery is that of the powerful virtues evolved by medicinal substances consequent upon their minute subdivision. Of the singular extent to which he carries this principle, few physicians in this country can have any adequate idea; and by those who are accustomed to limit their directions to a *Bene terantur*, or a bare *Misceantur simul*, the almost infinitessimal proceedings of the homœopaths can scarcely be contemplated without a smile. We have before us Hahnemann's letter on the treatment of cholera, written in August last, in which he prescribes no dose that exceeds two or three decillionths of a grain; and the numerical expression for a decillion, some of our readers may not be angry with us for reminding them, is unity with sixty good cyphers annexed. As we mean to reserve the homœopathic treatment of cholera for a notice in another number, we shall exemplify the extraordinary mode of dosing on this system by a single case, reported by Dr. Dufresne, in the *Bibliothèque Homœopathique*, of which that gentleman is the editor.

"A lady, about forty years of age, had suffered much from facial neuralgia; and though, by the use of sulphate of quinine and opium, she obtained occasional intermissions, she could never enjoy any thing like permanent ease. Dr. Dufresne had been treating her on the allopathic system, and was almost in despair about the case, when he found in a medical journal a short account of the pathogenetic effects of strychnos nux vomica (the symptoms by the way, produced by which, according to the *Reine Arzeneimittel.*, amount to about 1200): he was, in short, induced to try that substance, and at the same time to put to the test the Hahnemannian principles,—for he was not yet a confirmed homœopath. 'It was the alcoholic tincture,' says he, 'that I should have employed by right, but it would have occupied me from six to eight days in the preparation, so I resolved to use the remedy in the dry state. I took a grain of strychnine, and triturated it with 100 of sugar of milk, then a grain of this compound with 100 more of the sugar, and so on till each grain of the ultimate compound contained a hundred-millionth of a grain of strychnine. The latter dose was the one I administered to the patient. She was seized with a paroxysm of the neuralgia in the night, and about an hour earlier than the regular period of its attack. The usual symptoms were experienced, but it was remarkable that they occurred in an inverse order, attacking those parts last that were attacked first before. *The dose was much too strong.*

Madame B. was like a mad woman all night; the racking pains seized her whole head, and her face was swollen, and burning hot. When I saw her, on being sent for at five in the morning, I was exceedingly surprised at the appearance she presented; her eyes were half closed with the swelling, and I could only compare them with those of a person stung by bees. My position was very embarrassing; but in the evening every thing looked favourable, with a decided abatement of the swelling and pain. The night passed over without any recurrence of the tortures, and Madame B. rose in the morning refreshed, and in good spirits. There was but one slight accession of the complaint afterwards: the lady has ever since been perfectly well.'"

Dr. Dufresne adds, with regard to the desperate *overdose* which he was rash enough to give in this case, that, had he to treat Madame B.'s malady over again, he would administer to her a decillionth of a drop of the alcoholic tincture. One does not well know what to make of those homœopathic cases. We made chose of the preceding, not because we thought it the best, but because it was the shortest we could find, and might serve for a very tolerable specimen of the sort of "Facts" in which our Hahnemannians deal: we doubt, however, if the selection of more of them would at all serve their cause.

But to return to the dogmas. It is difficult to avoid the confession that there is much plausibility about most of them: even in behalf of those which are most at variance with generally received opinions, something may be said. The homœopath, for example, holds that in the totality of the symptoms, the whole of the disease exists; a position which the pure pathologist will never admit: yet the practitioner who has little time to devote to the fine theories of the schools, acts upon it every day, being fully content if he be only able to come at a full catalogue of his patient's aberrations from health. There are, however, certain diseases of which we know a good deal more than their symptoms—diseases in which their *causes* also are known; as when we recognize the presence of foreign substances, poison, or specific virus, in the human system: but for this we may see that there is an exception in the dogmas regularly made and provided.

Another striking assertion found among them is, that every new case is a distinctly new example of disease. But this of course follows, almost as a corollary to the dogma just noticed; for if diseases be admitted to be no more than so many groupes of symptoms, it must happen that there shall be as many varieties of diseases as of groupes. That such a position must tend to the direct overthrow of all our systems of nosology, is to the homœopath a matter of pure indifference, or one rather of congratulation than regret. "He cannot help it," he will say; and he asks, with an air of satisfaction, "whether the proofs of the earth's motion and of the sun's fixity were rejected on account of their tendency to overturn the established doctrines of the old astronomers?" And if it be complained that homœopathy has a sweeping propensity to level rather than to build up, its disciples are ready again in its defence, and point exultingly to the new science, of which they tell us it has laid the foundation—they allude to their *pathogenesy*. The merits of this, their new *science*, we cannot stay to examine; but the numerous volumes of their Pure Materia Medica sufficiently vouch for the ample extent of its details.

The reader, we suppose, has not failed to notice the very extraordinary assumption involved in the fifth dogma; any thing more manifestly illogical, we candidly confess, we have not for a long time met with.

SALINE TREATMENT OF CHOLERA —COLD-BATH FIELDS PRISON.

Testimonial of the Magistrates of the County, to the Medical Men, on their late successful Treatment of Cholera.

At the last meeting of the magistrates of the county of Middlesex it was unanimously agreed, that a vote of thanks, and a piece of plate of the value of one hundred sovereigns, should be presented to Dr. Stevens, for his attention to the sick, and the great success which had attended the saline treatment, in the prison of Cold-Bath Fields. Another piece of plate, of the value of fifty sovereigns, is also to be given to Mr. Wakefield, surgeon to the prison; and another, of the value of twenty-five sovereigns, to Mr. Crook, a young gentleman who lived for several years with Dr. Stevens in the West Indies, and is now a student of medicine in the London University. Small sums have also been awarded to several of the nurses; and some of the prisoners, who rendered themselves useful by their attendance on the sick, are to be recommended for pardon, through the magistrates, by the Secretary of State.

HEMERALOPIA, OR NIGHT BLINDNESS.

An interesting illustration of that singular affection, hemeralopia, or night blindness, has lately been afforded at Belfort, in France. About the beginning of February, several of the garrison of the place just mentioned complained of being unable to see either after sun-set or before sun-rise. The number thus affected, however, was inconsiderable, not exceeding twelve or fifteen, but they increased rapidly towards the end of February and during the month of March. Every evening, a little after sun-set, a number of soldiers, (for it was confined to the troops composing the garrison), might be seen wandering about, and scarcely able to find their way. Many were obliged to get persons to guide them, or to feel their way along the walls and houses, to gain the barracks. Ninety of the 36th regiment of infantry, and twenty of the 1st regiment of dragoons, were thus affected. Sixty of these were cured in the course of about ten days by means of tonics and stimulating washes to the eye, blisters to the back of the neck, and full doses (15 or 20 grains) of calomel, though this last was but seldom found necessary. The epidemic entirely ceased towards the end of April.

The blindness in hemeralopia is seldom complete; that is to say, the subjects of it are for the most part able to distinguish objects, and especially any thing brilliant, which is near them, but even these appear as if seen through a dense cloud. In some, indeed, vision is altogether abolished, even luminous bodies making no impression on the eye: in these the pupil is generally much dilated, and quite motionless, while in the former it has nearly its natural appearance. Most authors, in speaking of hemeralopia, represent it as preceded or accompanied by headache, giddiness, indigestion, or coma, or functional disturbance; but in the cases at Belfort, all the patients were, with the exception of the night blindness, in perfect health. Its causes are evidently but ill understood; and, indeed, it prevails endemically in some places, as at St. Martin-de-la-Roche and Folleunville, which have no local peculiarities by which this can be accounted for.

SWEATING EPIDEMIC.

We mentioned in a former number that a commission had been sent from Paris to the department of the Oise to examine into the sweating sickness which had manifested itself in that part of France; and we have now to add the statement of M. Hourman, one of the reporters. It appears that a febrile affection, with profuse perspirations, and a miliary eruption, has repeatedly visited the department in question, more especially in 1821; but that on the present occasion it has derived additional importance from being complicated with cholera. At Noailly it has been chiefly during convalescence from the sweating that the cholera has made its invasion. In these cases the eruption has suddenly disappeared, and profuse alvine evacuations come on, attended with cramps, and the other symptoms of cholera. In the canton of Mouy the cholera generally set in simultaneously with the sweating, and there was seldom any eruption. When

this symptom did appear, it put on the form of scarlatina rather than miliaria.

Independent of any complication, the sweating is a complaint of no great severity, but becomes formidable when attended with visceral congestions. Bleeding is the *heroic* remedy.

EFFECT OF IMAGINATION.

The following appears in a recent French Journal. A physician at Bordeaux laid a wager with a friend that he would communicate any disease he pleased, and to any one who might be chosen. Having mounted his horse, he rode to a village in the neighbourhood, stopping at every auberge on the way, and giving his instructions to "mine host." He also posted persons at different points along the road; and his arrangements thus made, he hired the unfortunate individual who was to be made the subject of his experiment to carry a letter to Bordeaux. He was stopt almost at every step by some one who asked what ailed him?—a question which he answered at first by declaring that he never was better in his life; but his confidence of being quite well gradually waxed fainter, till he felt persuaded that he was ill—very ill; and then was as easily convinced that he had a dreadful fever. Medical assistance was sent for; and, of course, among others, the contriver of the experiment made his appearance. It was found that their patient was literally in a high fever; and, consequently, that the wager was won.

This may be all very true, but it certainly is not very new, for many instances are on record proving the effect of imagination in producing sickness even to death. Nothing can justify any thing so wantonly cruel, and we conceive that in this country an action for conspiracy would lie against the parties concerned: at all events, the subject of the experiment would have served the would-be philosopher right if he had broken his head.

COLLEGE OF PHYSICIANS,

Monday, May 28, 1832.

Sir Henry Halford, Bart. President, in the Chair.

The first paper read was an official letter on cholera, addressed by Dr. M'Guffog, of Constantinople, to his Excellency the Right Honourable Sir Robert Gordon. It contained no feature of novelty: bleeding, stimulants, and laudanum, are the means recommended, particularly the first.

The second paper derived interest from the quarter whence it proceeded, consisting of observations on the subject

Of Perspiration, by the celebrated Dr. Heberden.

The essay might, perhaps, be correctly described as more interesting in a classical than in a medical point of view. Having alluded to the general fact of the external and internal parts being lubricated by moisture, the author referred to the plan, which in his day was frequently had recourse to, of endeavouring to overcome fevers by sudorifics, justly observing that it was manifestly erroneous, as in many fevers the patients sweated throughout the whole progress of the disease. The experiment of Reaumur, by which he was enabled to prolong the lives of caterpillars, by keeping them in a cold place, was detailed, and followed by the inquiry whether the life of man also might not be similarly lengthened; which naturally led to a consideration of the different modes of husbanding the perspiration. Of these, by far the most remarkable was that practised by so many of the ancients, namely, anointing the body. The learned author entered into a description of the various modes of using unguents and oils, adopted by the Greeks and Romans, illustrating the subject by numerous classical allusions and quotations. Certainly much efficacy in the preservation of health was attributed to those customs. Pliny tells us that Pollio Romilius, when he was above a hundred years old, being asked by Augustus how he prolonged his life to such an age, made answer, "by using honey within, and oil without." There is probably no greater difference between the manners of the ancients and moderns than refers to the practice of anointing; and Dr. Heberden attributed the discontinuance of the custom chiefly to the use of linen, and the discovery of spirit of wine, this proving a much more elegant vehicle for perfumes. The little effect which the disuse of anointing seems to have had on public health, is a strong

argument of its inutility. On the other hand, observed the learned writer, the consideration that sweat is only increased or condensed perspiration, sufficiently refutes the received opinion that the greatest possible discharge of this fluid is beneficial in preventing or curing distempers, since it is notorious that the sweating profusely impairs the health, and colliquative sweats are the most fatal signs and causes of weakness in disease.

ROYAL INSTITUTION.

Friday, 18th May, 1832.

Mr. Faraday on the Crispations of Fluids on Vibrating Surfaces.

This is one of those subjects on which few but Mr. Faraday would venture to entertain a large and intelligent audience for a full hour together; yet, in fact, he had to compress his abundant materials for illustrating what he had to say upon it; and perfectly succeeded in keeping every body's attention alive. In introducing the subject he referred to some observations which he had made in the last session, on the arrangement assumed by dry powdered substances lying on vibrating planes: some of the experiments made on that occasion he now repeated. He took a plate of glass about eight or nine inches square, and holding it firmly by an iron forceps fastened on its centre, drew a violin bow across the edge of one of the sides. The powder, which had been scattered evenly over the surface before the bow was drawn, now assumed in a moment a radiated appearance, collecting itself into straight heaps along the diagonals. These straight heaps marked the nodal lines of rest. If the powder was composed of particles of unequal densities, the heavier ones covered the lines of rest, while the lighter were gathered in the intermediate spaces. Mr. Faraday now substituted fluids on the plate for the powders, and the appearances which ensued were what Mr. Wheatstone has called *crispations*; they are the small waves, or numerous pointed hillocks of fluid which result from the vibration of the subjacent surface. These crispations assume different forms and orders of appearance according to the nature of the fluids employed, whether water, oil, ink, mercury, white of egg, or whatever else it may be; but it does not appear that the depth of the fluid modifies in any respect those forms. It is also remarkable, that the undulations of the thinnest oil will be invariably unbroken at their summits, while those of that highly-tenacious fluid, the white of egg, are as invariably broken. Of all these, and a variety of other similar phenomena, Mr. Faraday offered some very ingenious explanations, together with a description of the method by which he arrived at his conclusions. The whole he shewed to be concurrent with, and confirmatory of, the principles of polarization, and the theory of Young and Fresnel on the real nature of light—a theory which every day's experience tends more and more to establish as the only genuine and true one.

Having concluded the lecture, Mr. Faraday took occasion to introduce a new subject of intense interest. He noticed the death of Cuvier in the most feeling terms, and pronounced a brief but comprehensive *eloge* upon that illustrious naturalist, who was one of the very few pre-eminent characters of the age who have been thought worthy of being enrolled as honorary members of the Royal Institution. A fine portrait (recently executed in Paris) of Baron Cuvier, by Pickersgill, and lent by the artist to enhance the melancholy gratification of the evening, was now uncovered, and produced an unanimous burst of admiration from all present. No work of art could possibly be produced at a moment more calculated to give it effect—what with the noble subject represented—the excellence of the workmanship—and the peculiar circumstances under which the display was made.

In the Library were some fine specimens of carving on oak pannels, from an abbey in Flanders—the subjects from passages in the lives of the saints; there were also some beautiful examples of metals deflagrated by electricity.

May 25, 1832.

Mr. Brockedon gave a clever exposition of the nature and advantages of *Perrin's anchors*. The improvements which have taken place in the structure of this invaluable engine, from the earliest times, were interestingly described, and by the aid of models and drawings made intelligible to all. It is nearly twenty years now since Mr. Perrin took out his first patent, and his anchors have undergone considerable improvements in the interval; they have never been surpassed in excellence, though several attempts have been made by other machinists to supersede them. We were not a little amused by Mr. B.'s summing up, in which he dwelt on the paramount importance of the anchor in the concerns of civilized man—the steam-engine itself being, he thought, of but questionable utility, if unaided by the commercial use of the anchor.

In the Library there was a display of microscopic rarities, Turkish amulets, Brah-

minical talismans, &c.; and, among other things, some exquisite water-colour miniatures *à l'antique.*

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

MIDDLESEX HOSPITAL.

Venereal Phagedæna.

In the course of the present week there have been discharged several cases which illustrate, in a very satisfactory manner, the beneficial effect resulting from the treatment of some kinds of secondary venereal sores by local mercurial applications. In making some remarks upon the cases, Mr. Arnott observed that he was in the habit of applying the term venereal to sores, whether primary or secondary, arising from impure connexion, in which the full and continued effect of mercury upon the system (although in some instances it might be called for) was not generally expedient, or requisite; whilst he restricted the word chancre to the ulcer with indurated base, described by Mr. Hunter, and the term syphilitic to this sore and the scaly eruption, in both of which he regarded the full and continued effect of mercury on the system to be called for. In stating this, he wished to be understood as not expressing an opinion that there was a multiplicity of poisons, the existence of which was not proved, and to which belief he did not see any necessity for resorting, in our views of these morbid affections.

Secondary Venereal Ulcer of the Tongue, cured by the local application of the Grey Oxide of Mercury.

L. B., a married woman, 23 years of age, was admitted April 10th, with an ulcer on the upper part of the tongue, of irregular form, but of the size fully of a half-crown piece. The sore was deeply excavated, and its surface was covered with a dark grey, and in the centre brown substance, or slough. The edge of the ulcer was sharply defined, and the substance of the tongue, immediately beyond this, was swollen and more solid than natural, but of its usual colour. The pain was described as hot and throbbing, not stinging. There were no enlarged glands in the vicinity. The patient had an emaciated wretched appearance; she was pale, but not sallow.

The first impression produced by the appearance of this sore was rather unfavourable, suggesting the idea of malignant disease; but some of the circumstances above mentioned, and more especially the history of the case, produced a more satisfactory and favourable opinion of its nature. Two foul-looking circular sores, the size of a shilling, were discovered on the left leg; and another on the thigh, covered by a rupious crust. Several circular cicatrices were observed on the shoulders. Married in February, 1831, a fortnight afterwards being attacked with a discharge from the vagina: she consulted physician, who, after examination, ordered her to use black wash, and to take a pill night and morning, by which she got well in a week. Two months after, she was obliged to consult a surgeon, on account of an eruption of red spots which had come out all over her body; and he, after seeing her husband, put her upon pills, night and morning, for seven weeks, and three of these she was confined to bed by a strong salivation. She remained well for a short time, but in three months from the expiration of the treatment just mentioned she was obliged to apply for advice at a public institution, on account of sores on the thighs, back, and arms, and sore throat. She now took medicine, chiefly sarsaparilla, for three months, at the end of which time, with the exception of a sore or two on the leg, all the others, and that in the throat, had healed. She then went into the country, but in six weeks had a return of her complaints, for which she came to town and took medicine (sarsaparilla, with oxymuriate of mercury in it), with partial benefit, but she has never been quite well since.

The sore on the tongue began four weeks ago, with a small white blister, full of water: this burst, and produced a sore, which has got gradually larger. The following application was ordered to be applied to the ulcer of the tongue night and morning, and the coating to be retained on the part as long as possible:—

℞ Oxidi Hydrargyri Cinerei, gr. xv.; Mellis, ʒi. Full diet; porter.

14th.—Sore in some parts cleaning, the ash-coloured substance having partially separated. To continue the local application once a-day.

℞ Ext. Sarsaparillæ, ʒj. ter die ex lacte.

18th.—Sore of tongue universally clean, and surface florid.

℞ Pil. Plummeri, gr. v. omni nocte.

21st.—For the last two days the patient, by desire of Mr. Arnott, under whose care she was, has omitted the local application, and at two points of the sore a yellow glairy substance has reappeared, clearly shewing, as was remarked by Mr. A. that the improved appearance of the sore had been dependent solely on the local treatment. The grey oxide and honey was to be re-applied once a-day. By the 23d the sore had again resumed its clean, florid appearance, and commenced healing. The extr. sarsaparillæ was discontinued, and a drachm of the powder, three times a-day, given instead. On the 25th the Plummer's pill was discontinued. By the 2d of May the ulcer of the tongue had nearly healed, and the local application was discontinued; on the 9th it was quite so.

During this time, the sores on the leg,

and that left by the falling off of the crust on the thigh, had been treated by poultice, simple dressing, and an ointment containing balsam of Peru and extract of conium; but up to the 5th inst. they had not improved; they had a brown foul surface, surrounded by a dark-red border of integument. Mr. Arnott now desired the cinnabar fumigation to be applied to them. In a few days they lost their unhealthy character, and began to heal; two of them were so on the 21st, the third almost on the 25th, when the patient, who had now regained her flesh, strength, and personal appearance, left the hospital.

In this case, the sore on the tongue and those on the leg were equally benefitted by the local application of mercurial remedies, which were employed with the view simply of changing the character of the sore, and not of affecting the system. Nor was the last effect produced; the breath was at no time tainted, nor the gums in the least affected. That it was by local operation the mercurial remedies acted, is proved by the details given, and more particularly by the effect following the intentional omission of the grey oxide to the tongue for a couple of days. The same remark also applies to the following case, where the fumigation was attended with equally beneficial results.

Phagedænic Venereal Ulcer of the Thigh—Cinnabar Fumigation.

M. K., a married woman, 23 years of age, was admitted April 11th, with an ulcer larger than a crown-piece on the outside of the left thigh, close to the knee. It was deep in one part, exposing the tendon forming the outer ham string. Its surface was covered with a yellow, glairy looking tenacious matter, surrounded by a phagedænic edge. The discharge from it was thin; the skin in the vicinity was of its natural colour.

This patient was not communicative; she stated that the sore began a month ago, and that previous to its appearance there had been a red spot at the part for some weeks, but that she had never had any other spots nor any other complaint! Attention was attracted to a curl, arranged with particular care, on this young woman's forehead; it was displaced, and was found to cover a large cicatrix, which she acknowledged to have followed a sore which had made its appearance, preceded by a red spot, three months after her marriage (two years ago), and which sore had continued open nine months, during which time several portions of bone had come away. Further information upon the subject could not be obtained, other than that for the last twelve months, she had never been out of the doctor's hands. Now she looks pale and ill, has a very indifferent appetite, and bad nights, from pain in the sore.

After her stomach and bowels had been attended to for a few days, on the 14th the sore on the thigh was directed to be fumigated daily. She was ordered four ounces of the decoction of sarsaparilla, with half a drachm of the extract, three times a-day, and to have an anodyne draught at night.

On the 17th, one half of the sore had lost its unhealthy character, but as she complained of pain attending the fumigation, it was omitted. On the 21st, no further progress being made, this was resumed, but was directed to the part of the sore only which burrowed around the tendon, and was covered with the yellow, glairy, tenacious matter, and not to the other half, which presented a florid appearance. Three or four applications were sufficient to deprive the former of its unhealthy coating and character, which it did not resume: the yellow wash was now ordered. On the 30th the sore was healing, but, as she still complained of pain, five grains of Extr. Conii were directed to be given thrice a-day, in addition to the sarsaparilla, and she was allowed porter. This treatment was continued, the sore healed gradually but steadily, the patient got fat, and leaves the hospital to-morrow (the 29th May), being cautioned not to use the limb too freely for some time, until the cicatrix which is adherent to the outer ham-string becomes more firm.

In the case which follows, the mercurial fumigation affected the system.

Secondary Phagedænic Venereal Disease, cured by the local application of Mercury.

J. S. 27 years of age, was admitted into Pike's ward on April 12th. On the chest, shoulders, and back, were seven or eight circular sores, of various size, intermingled with the cicatrices of others which had healed. The largest sore, on the tip of the left shoulder, was about two inches in diameter; had a red, flabby, glazed surface in the centre, and a spreading phagedænic edge. In the centre of the upper lip was an ulcer, attended with considerable loss of substance, and its surface covered with an ash-coloured tenacious matter. The velum palati, and both sides of the uvula, were occupied by a superficial tawny-looking ulcer, with a broad border of a dark-red mucous membrane.

In October last, this man had a small sore, the size of a split pea, on the glans penis, for which he took three dozen of pills, one pill night and morning, under which the sore healed. He remained well until the 1st of February, when, after exposure to wet, his throat became sore. He consulted a surgeon, who applied fourteen blisters successively (seven on each side) to the neck in the course of three weeks and a half, but the throat did not get well. He was then order ed to wash himself all over with a solution of chloride of lime, and after doing this two days, an eruption of red spots, quickly forming scabs, came out all over the body. He was now desired to wash himself with a so-

lution of bay-salt, (a tea-cupful to a quart of warm water) and by this the scabs were brought off, and the sores left were dressed for three or four days with powdered Peruvian bark. He next had an ointment to dress the sores, and some of them healed rapidly. He was then directed to dissolve two pounds of tar in two quarts of water; to take a wine-glassful of this twice a-day, and to wash the sores with it, which he did until the last fortnight, during which he had eschewed physic, and taken instead a couple of glasses of port-wine a-day.

The cinnabar fumigation was directed to be applied to the throat; the yellow wash to the sore on the shoulder and two others; the rest to be dressed with simple ointment.

14th.—℞ Liq. Iodinæ, ʒss. ter die ex Aqua Menth. vivid.

16th.—Has used the cinnabar fumigation four nights; the ulcer of the velum has not changed its appearance. To omit the fumigation, and to apply the liniment of grey oxide of mercury and honey to this sore, and that on the lip.

18th.—Breath tainted with the mercurial fœtor; gums spongy; and there is an increased secretion of saliva. Omit the mercurial liniment, and use the myrrh gargle; to have a purge, and to omit the iodine.

21st.—Sores on velum palati look cleaner; that on shoulder has one-half cicatrized in the centre. As the yellow wash now gives pain, to be omitted, and replaced by simple dressing. To resume the iodine.

23d.—℞ Iodinæ, gr. ss. Hydriodate Potassæ, ℥ss. Aq. Menth. vivid, ℥viii. A third part three times a-day.

25th.—Sore on velum healing.

Rept. Mist. cum Iodinæ, gr. ⅔. Haust. Aromat. q. Opio.

28th.—Has been purged severely, with pain. To omit the iodine.

30th.—Ulcer of velum quite healed; that on lip quite florid, and partly healed. To resume the iodine; but as this was immediately followed by a return of the pain and purging, it was laid aside.

May 7th.—Sore on lip quite well, and all the others, with the exception of a narrow circle of that on the shoulder, to which, on the 10th, the yellow wash was reapplied, and which had cicatrized on the 19th, with the exception of two very small points at the margin of the axilla, which preserved a phagedenic character, but to which three applications of the cinnabar fumigation now gave a healthy appearance, so that they had healed by the 23d.

The mercurial affection of the system, as indicated by the state of the mouth, there is little doubt was produced by the fumigation in this case. It lasted for about ten days, and during this time the ulcer of the velum and lip began to heal. Although the iodine was exhibited in this case, it does not seem to have been an efficient agent in the cure, and this is stated without prejudice to the beneficial effects of this powerful medicine. The sore on the shoulder commenced healing under the yellow wash (also a mercurial application) before any constitutional effects of the medicine had shewn themselves.

TREATMENT OF FRACTURES.

To the Editor of the London Medical Gazette.

Sir,

The impartiality which you have always displayed in the conducting of your valuable periodical, induces me again to solicit your indulgence whilst I offer a few comments upon the contents of Mr. Lonsdale's last letter respecting the treatment of fractures of the fore-arm; the observations which he has there made being, to my mind, equally unsatisfactory with those which he communicated on a former occasion.

When Mr. Lonsdale first broached his sentiments upon the subject, he stated that he believed when the radius was broken the upper portion of bone was rendered completely supine. I must confess I was greatly surprised at this assertion, and as Mr. L. omitted to state how he supposed this sudden change to take place, I was naturally led to inquire of him how he could *prove* such to be the fact, and what reason he could assign for the supinator radii brevis acting so simultaneously. Mr. Lonsdale, however, has since informed me that he does not attribute the position of the bone so much to the influence of the supinator radii brevis, as he does to the action of the *biceps flexor cubiti*; for he observes, "it is the biceps muscle that I think brings the upper portion of the bone into a supine state."

Now, Mr. Editor, I have frequently heard it remarked, that a person who is in danger of being drowned will eagerly grasp at a straw, or any such frail body that he may happen to come in contact with, in anxious solicitation of meeting with some assistance. Such, I am inclined to think, must have been the idea Mr. Lonsdale entertained, when he seized the name of the biceps flexor cupiti muscle to assist him in supporting his views upon the present occasion; and I would beg to acquaint him, that I am willing to acknowledge the biceps to be a *feeble* supinator of the hand, but when he asserts the *supinating power* of that muscle *exceeds the strength of its antagonist*—viz. the *pronator radii teres*—he might almost with the same degree of propriety have compared Olympus to a mole-hill. Mr. Lonsdale, not content with stopping here, proceeds to remark, "Besides, the very action of this muscle (biceps), when it bends the elbow, in order to apply any

mode of treatment, will be to bring the position of the bone into a state of supination." Here, again, I am anxious to inform Mr. L. that the action of this said muscle, which he has been at some little pains in alluding to, is what *I always endeavour to prevent*; and I invariably request the patient not to make any muscular effort on his part, but to allow me to place the limb, and keep it by a sling in that position which I consider best calculated for the relaxation of all the muscles connected with the fore-arm—viz. *midway between pronation and supination*—by which means I consider the muscles are one and all of them in a passive state, and the biceps, instead of being in action, as Mr. Lonsdale would feign have us to believe, is literally in a state of *relaxation*, the *attachments of the muscle having been approximated*.

Having said thus much, Mr. Editor, with respect to the action of the biceps in fractures of the fore-arm, I am induced, with all due deference to Mr. Lonsdale, again to state it as my opinion that the upper portion of the bone *is not* supinated to the utmost. The following fact tends to prove the correctness of my assertion:—A man who had trodden on a piece of orange-peel in the streets, fell upon his hand, and I detected a fracture about the lower third of the radius. I placed the thumb of my left hand upon the head of that bone, and with the thumb and fore-finger of the opposite hand I grasped the radius just above the point where it was broken, and endeavoured to rotate it *outwards*, which I succeeded in doing; and at the same time felt the head of the bone turn *completely supine*; proving satisfactorily to my mind that the upper portion of the radius *was not* in that position *previous to my examining the limb*.

Mr. Lonsdale is desirous to know if I will allow that there is a *probability* of the patient being unable to turn the hand completely supine after fractures of the fore-arm; and if, in such cases, there would be any likelihood of the evil being prevented by adopting the mode of treatment which he has recommended?

My reply is, that, in *ordinary* cases of such fractures, the *probability* is that the patients, after the bones have united, will have the power both of *complete pronation* and *supination*; but in making this assertion, I am well aware that there is also a *possibility* of the patients' being deprived, in a great measure, of the use of their limbs, after a *severe* accident of this nature; for if the bones should be comminuted, and the interosseous ligament destroyed, and unfortunately any of the fragments become united to the ulna, the power of pronation and supination must be for ever lost: and where, I would ask, is the individual who would not prefer to have his hand fixed *midway between pronation* and *supination*, in *preference* to the position in which it must inevitably remain if it were placed as Mr. Lonsdale directs—viz. *completely supine*?

Your obedient servant,

SEPTIMUS RODICK.

Edmund's-Place, Aldersgate-Street,
May 22, 1832.

PROFESSORSHIP OF MATERIA MEDICA.

HAVING last week expressed our belief that a correspondent in Edinburgh was mistaken in supposing that Dr. Maclagan intended to stand for the vacant chair of Materia Medica, we think it right to mention that we have since ascertained that he *is* a candidate for that appointment. What led us to an opposite conclusion, was the knowledge that he had applied to Lord Melbourne for the chair of Medical Jurisprudence, (a Regius Professorship) which will become vacant if the Town Council elect Dr. Christison to fill the place of the late Dr. Duncan.

DR. MORISON.

AT a meeting of the Justices appointed to superintend the management of the Middlesex Lunatic Asylum, at Hanwell, held on the 25th ult. they resolved that Dr. Morison, of St. James's Square, should be appointed Consulting Physician of that establishment.

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

May 1832.	THERMOMETER.		BAROMETER.	
Thursday . 24	from 45 to	71	30·23 to	30·20
Friday. . . 25	45	71	30·16	30·07
Saturday . 26	46	70	30·07	30·00
Sunday . . 27	39	67	29·96	29.84
Monday. . 28	43	72	29·96	29.87
Tuesday . 29	40	69	29·84	Stat.
Wednesday 30	41	64	29·86	29·69

Prevailing wind S.W.
The 29th and 30th cloudy, with rain at times; otherwise generally clear.
Rain fallen, ·225 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

We have received, from Dr. P.S. Knight, an account of three additional cases illustrative of the effects of alkaline salts in gastric irritation. The cases in question were "all attended by some of the most serious precursory symptoms of cholera asphyxia." They all did well; but we do not think it necessary to publish them in detail.

"A Constant Subscriber" is informed that it is not usual to send a label with the last number of a volume, but that they can always be had by applying to the publishers.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 9, 1832.

CROONIAN LECTURES,

Delivered at the Royal College of Physicians,

By Dr. Roupell,

May 1832.

Lecture I.

On General Pathology.

The pathology of the brain and nervous system formed the subject of the Croonian Lectures which were delivered during the last and three preceding years. The disorders of the brain and spine, as well functional as those connected with obvious organic changes, were then ably treated by Dr. Hawkins and Dr. Seymour.

It is my intention, in the present series, to extend this inquiry into the investigation of the morbid changes which take place in other important organs of the body; but first propose briefly to enumerate the different alterations to which the various tissues are subject, and to examine into those laws, as far as we have been able to ascertain them, which regulate the processes we term disease.

Pathology in its widest sense, while it embraces the consideration of the peculiar alterations in texture, and the different states of parts, which are observed in disordered conditions of our frame, and while it pursues the effect of such change upon the animal organization, and traces the sympathetic affections to which they may give rise, also contains within its province the investigation of the causes on which such alterations depend, and is immediately connected with the principles, and forms the basis, of therapeutics,—in each of which views I have imposed upon myself the task of regarding pathology, and shall treat first, of the "alterations of structure."

The investigation of the causes of disease, and the endeavour to explain the symptoms by examination of the body after death, is a natural source of inquiry, and has been made by physicians at distant periods. A treatise on some pathological changes was published in Italy upwards of three centuries ago; and the subject, at various periods since that time, has occupied the attention of many distinguished anatomists.

The comparative ignorance, however, of the earlier writers, as to the functions of our system, renders their works of little value at the present day. The fanciful theories with which their narratives are interwoven, give them little more merit than that of having afforded some of the steps by which we have ascended to our present knowledge. The labours of anatomists of later times, indeed, have elevated pathology almost to the rank of a new science, and, by properly directing our inquiry in this branch of study, give us ample reason to conclude, that the physiology of our system will ere long receive more complete elucidation, and the knowledge of the nature of disease extended, will in consequence materially improve the adaptation of our remedies.

The various functions of the human frame —the complication of its numerous organs, each destined for especial purposes—naturally turned the attention of physiologists to the *investigation* of the more simple forms of animated nature, by which much light has been thrown on the uses of individual parts, and which has enabled us to ascertain the lowest degree of organization necessary, and the processes immediately essential, to life. By observing the effects which physical causes are capable of producing on the lowest scale of existence, we learn some at least of the powers which modify and control the vital actions in the higher order of beings; and discover, as the result of this search, operations in most instances analogous to those perceived in the human frame.

We observe in plants, that under a certain combination of circumstances vivification commences; we notice in their growth a determinate form assumed, and we notice a regular period of perfection and decay; we perceive at returning seasons circulation

excited, respiration performed, nutrition effected, and see them suffering under disease, acutely sensible of variations in the temperature and constitution of the atmosphere, or in the nature of the nourishment which they imbibe from the soil. We notice that plants are subject to different states of activity and repose—that they possess and exhibit a property of irritability—that they are capable of certain processes of reparation on the receipt of injury—and that they are highly susceptible of, and differently affected by, the deleterious impressions of poisonous agents.

Dr. Turner and Dr. Christison have shewn, in their paper in the Edinburgh Medical and Surgical Journal, vol. 28, that the one-tenth of a cubic inch of muriatic acid gas, diluted with 20,000 times its volume of atmospheric air, destroyed all the leaves of vegetables exposed to its influence for twenty-four hours; and a laburnum thus acted on, though removed into good air, did not recover. The effect of sulphurous acid, and sulphuretted hydrogen gases, on vegetables, are nearly equally pernicious; and the impression of the mineral poisons, Mons. Marcet has shewn to be extremely marked: when only one-fifth of a grain of arsenic had been absorbed by a branch, it perished, and when a whole plant had been thus destroyed the injurious effect of the experiment was perceptible, not only on the subject of the experiment, but also on a neighbouring shrub with which it was accidentally in contact.

In the animal kingdom we find higher properties than those in vegetables; we find a constant circulation—a perpetual source of vital energy—a sensibility to more numerous sources of excitement. We perceive, in consequence, more complicated apparatus; we trace a nervous, a muscular, and a vascular system, acting upon appropriate and specific stimuli, yet mutually dependent on, and essential to, each other. We see the vascular system—the source of the more substantial materials of which the different parts are composed—we find it first in activity in fœtal development, and trace in its after operations the vigour of life, and if not the origin, at least the means of addition of structure, and devastation in disease. The properties essential to this system, are, first, the movement in the extreme branches, by which fluids are conveyed—capillary circulation; and secondly, those processes by which parts receive their requisite support—the power of nutrition.

These are essential to life, and in their modifications, acting in conformity to established laws, we may detect all the changes incident to our frame. We watch in the embryo the oscillations of the vesicula umbilicalis. We detect the additional formation of the heart, and trace, in the regular development of the fœtus, a series of changes corresponding to the order of formation in the different classes of organized structure; and we see, at certain periods, a progressive increase or decay of the various tissues of the frame; some at birth, having already completed their growth during uterine existence—as the membrana pupillaris; others disappearing soon after—as the thymus gland; others, as the muscles and nervous system of the animal life, requiring much to bring them to perfection; and all yielding to the increasing influence of time.

The process by which nutrition and growth are performed, is yet but imperfectly known. We are, indeed, aware that the particles of which the tissues are composed are separated from the blood, which requires for its supply substances already prepared for conversion into animal matter, by the changes undergone in vegetable life, or which have already been converted into animal matter by the process of assimilation. These substances, carried to every part of the system, are elaborated in the minuter capillary extremities; but, important as the ultimate changes which there take place must be, we are obliged to be satisfied with conjecture as to their exact and intimate nature.

According to Mons. Shültz, an active movement takes place in the small corpuscles of which the blood is composed, by a property of mutual attraction and repulsion.

The process of nutrition, in the opinion of Mons. Dœllinger, consists in the separation of some of these particles, and the return of others which had already formed an integral part of the tissues; by which means the change of the original parts is effected. Some curious properties of the tissues have been ascertained by Mons. Dutrochet, who has endeavoured to search deeper into the ultimate state of parts during this interchange of particles. He observed the inclination which some substances have to pass through the tissues into the circulating vessels, and from the vessels again into the surrounding parts. By enclosing various fluids in a portion of intestine, secured at either end, and then immersing it in other liquids, he perceived that sometimes a current from without inwards resulted, at other times that a current from within outwards took place. He perceived that when the external fluids were drawn into the intestine, and a state of distention consequently induced, that a current in a contrary direction succeeded, and that the particles were then forced outwards. This state of determination to the interior of the vessels occurred when the intestine was empty, or contained a fluid of greater density than that without.

He observes, that this is not a mere capillary process—that this power of imbibition did not take place when the fluid contained in the intestine was in a putrid state,

or the portion of the intestine was not in a state of integrity; nor was it entirely a chemical change, effected by the attraction of the two fluids, as these currents were established between two liquids, which in contact, without the interposition of membrane, exerted no influence whatever on each other. According to his observation, nutritive substances readily passed through the tissues, and, in proportion to the power of this determination inwards, nutrition is performed. The chemical nature of fluids, he observes, influenced these affinities; alkaline substances induce absorption, the current inwards, while acids diminish this power, determining the current to without. At present we possess but little knowledge of the actual process which excites absorption; we know not the reason why lacteals take up chyle alone, or what urges the secreted fluids to seek their exit from the frame, but feel that some powerful influence must be exerted in determining the vessels either to reject or to take up certain substances; as we see the bladder unable to absorb the fluid it has secreted, and the cæcum to resist the passage of fœtal matter through its coats, although active absorption would take place if other matters had been in contact with these mucous surfaces. This inquiry is, as yet, far from having been satisfactorily elucidated, and the experiments of Mons. Dutrochet require confirmation; suffice it, that rapid motion of the particles of the blood seems essential to its integrity, and that the increase, removal, or diminution of the causes that maintain this, is one step to the alteration in the texture and composition of this fluid. Some, however, of the laws which regulate the process of growth, we can more distinctly comprehend. Thus—

Although the human organization has some peculiarities which entitle man to an especial class, and place him at the head of animated nature, yet we perceive in the progress of his growth a regular series of development; and when any interruption to the fœtal formation takes place, the degree of perfection of the embryo at the time indicates a corresponding step in the scale of animal creation.

By this we learn that bone, cartilage, fibre, membrane, and intestine, can be formed where neither brain, nor heart, nor spine exist.

We learn, too, by this law, to explain the imperfect fœtal productions; and to account for the greater frequency in malformations of the head and genital organs, and such as are last in arriving at the period of maturity.

In reverting to the morbid changes of our frame, and in considering the first class of these—such as depend upon altered capillary circulation—I shall notice the state connected with an increased or diminished quantity of fluids, the distention or collapse of the vessels, the simple softening from increased quantity of fluids in the tissue in the minute vessels, and the induration attendant upon its undue diminution.

In the one instance the alteration of the fluids may be in the vessels themselves, and either increased or diminished; in the other there may be more fluid in the tissues, or less.

In considering the function of capillary circulation essential to life, and approaching the physiological state of that system, I may observe, that for the healthy exercise of the living functions of some, if not all parts of the body, it is necessary that they should be capable of sustaining an increased circulation of blood, and that at other times, on particular occasions, it would appear important that even the usual supply should be reduced.

This we see exemplified in secreting organs during the period of their activity, in muscles during exercise, and in a variety of natural states. Through this power we perceive the *supply of blood* regulated by the wants of the part—the proportion determined to a secreting organ bearing an accurate ratio to the fluids withdrawn from it; and we see in this provision the means of increasing the bulk of parts which may require additional support — muscles constantly exercised acquiring double their size and strength, thus adapting themselves to those circumstances under which they may by habit be placed, and that system suiting itself to the exigencies of its situation. On the first accession of local determination, as is well known, an increased rapidity of circulation ensues; subsequent to this a dilatation of the coats of the vessels succeeds; congestion results. This state or condition may be confined to one part alone. We daily see examples of this determination; and in disease find its illustration, as the sudden turgescence of the vascular system simply, without any appreciable change of structure not unfrequently occurs, and occasions powerful and fatal results when the propulsion is towards important organs: thus we infer this state of the vessels to be the cause of sudden death in children who unexpectedly perish with symptoms of determination of blood to the head—the first stage of the acute hydrocephalus, the condition which precedes effusion; or this state may give rise in adults to apoplexy of the brain or lungs. Congestion may be entirely partial, confined to the situation in which it first occurred, and may remain a longer or shorter period without influencing more remote organs; but, if continued, it excites other actions, which give rise to various sympathetic affections, and may excite disturbed circulation in other organs.

The symptoms induced by this state, when the brain is the point of determination, are

those familiarly known to indicate fulness—giddiness, disturbed function, pain, convulsive movements, complete insensibility. When this state of distention or fulness of vessels is general, an excited state throughout the system is occasioned; fever is induced.

An opposite state of the circulating system—a diminution of the mass of fluids in the minuter vessels, is a condition as well natural as abnormous to the system. Of this an instance may be quoted in the coldness and shrinking of the extremities, from the lessened power of the heart and arteries, which we see on powerful mental emotions—a wise provision, by which the current of the blood may be restrained, under circumstances when probably the condition of the brain would be ill suited to resist its force. This state, the one of absence of blood, as well as that of fulness, may also affect a portion or the whole of the system,—those in whom this general deficiency of blood occurs presenting a well-known external physiognomy—a pale, bloodless countenance, and great debility.

It is not by any means necessary that, to occasion local determination, there should be a redundancy of blood in the system generally; nor does it follow that there should be an increase of power in the part, or in the system thus congested, by which this fulness is maintained: the minuter vessels, acting independently of the heart, they may thus dilate, without any general excitement of the system—active determination of blood to the head taking place in those of spare habit, and in many cannot be prevented by bleeding, however copious or frequently repeated. So far, indeed, from this state of congestion being one constantly of activity, and allied to strength, it is very frequently seen when debility has been induced, which appears, indeed, to be an exciting cause of irritability.

I have hitherto considered parts merely in relation to the altered quantity of fluids they may contain, which may thus be either more or less than that requisite for their several exigences. I shall next consider the result of local congestion, which, although a natural process of the system, is the forerunner, the cause, or the accompaniment of disease; but first may notice an important practical inference which may be drawn from the consideration of the state of the varied quantity of blood and activity in the circulation in one part, while the rest may be either unaffected, or actually receive less, viz.—that the pulsation of any one artery, as the radial, can be no criterion of the circulation through the others; and we perceive how treacherous a guide the pulse may prove, if our attention is directed to the beating of an artery in any one part alone. We have learnt, indeed, that the unduly slow pulse in apoplexy is quickened by bleeding; we daily find the artery weak in serious inflammation of the lungs; and when the brain or stomach is deeply implicated, we are aware of the diminished power of the arteries in the extremities, and the occasional irregularity of the heart's action.

The symptoms, moreover, which are exhibited in these opposite states, of undue fulness or emptiness of the vascular system, so accurately, under some circumstances, resemble one another, that we may perceive that a very erroneous judgment might be formed, if symptoms alone constituted the basis of a diagnosis.

Thus, while this increased excitement in the arteries, and this enlarged capacity of the vessels, when unduly occasioned, or improperly continued, is of itself productive of injury, other alterations take place, constituting numerous changes in the part, and forming the disorders of the second principle which we observe to exist in all organized structures, viz. errors of "secretion." The process of secretion is that to which, variously modified, all changes of volume in the parts, as well as every alteration in their chemical composition, is to be attributed. This process has been divided into nutritive and functional. The nutritive secretions present us with multiplied examples of alterations in texture, and these may be—

1st, The increase or diminution of the natural structure of the parts; or,

2ndly, The addition of new parts, either foreign to the system at large, or inappropriate to the situation in which it may be deposited.

Of the diseases of the first of these classes, viz. by increase of natural structure, muscular fibre gives us the most frequent example; this, however, can only be considered disease when it occurs in the heart, or some situation where the increase of bulk impedes the functions of the organ.

This increase, however, in natural structure, is not confined to muscles, but has been observed in almost all the textures of the body—as well in the cutaneous and mucous tissues—in the vascular system and cellular structure—the more spongy textures, as in the substance, the solid parenchyma of different organs. The liver in the fœtus sometimes, according to Mr. Lobstein, is increased to such an extent as even to render parturition difficult. The thymus gland is occasionally much augmented, particularly, it is affirmed, in those children who die with rachitis. Enormous enlargements of other glands are frequently met with, and numerous cases have now been collected of increase by natural structure of the brain; and examples of such a change in the spine are not wanting in the catalogue of the increase in natural structure.

Of the other state of parts included in

this class, the wasting—their simple diminution—" atrophy"—I shall now speak.

Whenever a part ceases to receive its due excitement, and consequently its proper supply of nourishment, it is observed to diminish in size and volume, and even gradually to disappear.

This state is not unfrequently met with in the limbs, either partially or entirely. When by accident the sight has been injured, we perceive the wasting of the origin of the nerves of the eye ; and those accustomed to the examination of the intestinal canal must have met with a state of that viscus of extreme attenuation, of thinning, yet without appreciable alteration in its component parts.

Atrophy in bone is very common, which structure in some individuals becomes so extremely brittle from this cause that they break from the slightest violence. A change in the texture of bone is met with in phthisis, scurvy, and gout, and is, with atrophy of the whole system, the common effect of age.

Mons. Desmoulins, in the Journ. de Physique, mentions the sensible diminution of the brain and nervous system, and ascribes to this alteration the climacteric disease so perspicuously described by Sir Henry Halford.

In the various stages of atrophy, the tissues go through a certain definite process, all returning ultimately to cellular structure, whatever may have been their original form : all fat disappears, the cellular tissue is reduced to a mere network ; the muscular structure loses its red colour, becomes white, and is weakened; its strength shrinking, or indeed disappearing totally.

All parts are perceived capable of undergoing a great change in their denseness or rarity, either naturally or by disease. Some change, indeed, is observed in all vascular alteration, and this difference of texture is the regular process at certain periods of life—organs varying from the fluidity of first formation to the rigidity of age ; and in the intermediate periods we observe much alteration—from extreme hardness and solidity to relaxation, fragility, or even complete pulpiness.

This diminution of cohesion—a change which may occur in the most solid as well as the more fluid parts—is a certain natural process essential to the performance of some important functions.

A relaxation in the tissues facilitating materially the motion of certain parts, which for a period require such an adaptation—as the fibres of the uterus relax during the period of utero-gestation, and in some animals the ligaments softening, allow motion in the bones of the pelvis previous to parturition—the parts thus for a time conforming to certain laws, return again to their former consistence, when the necessity which induced the change has passed away. Increased softening appears to be inseparable from the rarefaction of solid parts by the infiltration of fluids. It may exist from many causes, and is attendant as well upon an increased as upon a diminished activity in the parts. Tissues, in this state, yielding readily to slight force, render the heart unable to resist the pressure of the blood, and its rupture is the consequence. The pulpy state of the mucous membrane of the intestinal canal, which is often observed to take place in the stomach, has not yet received a satisfactory explanation : it appears essentially a chemical change, continuing for some days after death, when we cannot suppose that any process of secretion can go on. This softening is not moreover confined to the stomach, but is found equally in the œsophagus and large and small intestines. The brain is especially liable to that change of consistency,—a condition noticed and described by Morgagni, and since more fully by Messrs. Lallemand and Rostan.

A change in the compactness of organs, without alteration in their volume, is occasionally met with : thus we see bone acquire the hardness and solidity of ivory; muscular structure, too, assumes a greater degree of firmness, and its molecules approximate more closely : this state is observed in muscular structure, in the heart and other tissues, in which the natural elasticity and power of contraction are either lost or modified ; the walls of the heart, when this viscus has undergone such a change, remaining, after death, firm, rigid, and distended.

I have now to consider those alterations which may take place in parts where the tissues are essentially changed : almost all original structures appear capable of reproduction in their proper situations, the muscular and ganglionic alone excepted ; but various abnormous deposits are met with throughout the structure, inducing alterations of the texture of the different organs ; these deposits may be either natural structure, or new combinations of the component parts of the fluids. Tissues natural to the body, but adventitious to the parts in which they may be deposited, frequently are met with : a misplaced activity, a perverted nutrition, serous and mucous membranes, vascular tissues, fibre and fibro-cartilage and bone, may thus be produced, and all may be resolved into cellular tissue, which may be converted into serous or mucous membranes, and these again become changed into fibre, cartilage, and bone.

The cellular tissue is readily formed by exudation of coagulable lymph, and by the agglutination of two folds of serous membrane we see the ready conversion of that tissue into continuous cellular structure. Synovial membranes may undergo a similar alteration, or be accidentally formed, and condensation of the cells of this tissue form-

ing the parietes of a cyst, and the effusion of fluid into the cavity.

The formation of new vessels, which may themselves accidentally be produced in fresh deposition of coagulable lymph, is a curious process, and takes place with great rapidity. A communication was proved by Sir Everard Home to exist between new and old vessels, when inflammation had not taken place thirty hours.

The vessels, in this instance, communicated with those in the original parts; but the formation of new ones may take place independent of such connexion.

According to Mons. Laennec, the rudiments of new vessels are minute lines, which appear to be coagulated blood; these finally assume a cylindrical form, and present numerous ramifications. Sir Everard Home ascribes the formation of new vessels to certain small vesicles filled with a colourless fluid, which unite and form a vascular tissue.

The carbonic acid gas contained in the blood, and which, under the air-pump, is represented as escaping with an appearance of ebullition, may probably perform a very important office in this change. It occasions to Mr. Brand numerous channels in parts newly effused; and Sir Everard Home has noticed such an effect in the coagulum formed in the blood after its removal from the veins. Mucous membrane appears to be very nearly approximated in the lining of fistulous opening; and cartilage, or fibre, separately or conjointly, are frequently formed. The accidental development of fibre takes place in all reparation of arteries, and they become converted into this substance when they are obliterated. Into fibre are converted the cartilages and synovial membranes, in incomplete anchyloses; it forms new articulations, and is the means of the union of muscles.

Fibrous tissue is particularly disposed to be converted into cartilage; and serous membranes frequently are transformed into this substance;—and both fibre and cartilage are disposed to the deposition of bone.

Phosphate of lime prevails more or less in the economy, and having been deposited in the cartilaginous portions first allotted to it, it then is secreted in other parts of the system, ossifying all the tissues of economy, as seen in advanced life. The deposit of bone is most frequently met with in the inner coats of arteries, which had previously undergone cartilaginous deposit; and osseous concretions have been detected in the interior of the vessels themselves.

Muscular structure is occasionally the seat of this deposit. The thyroid gland, the uterus, and pancreas, are said to have been converted into bone, but in other organs this change is due to the deposition of this matter in the cellular or fibrous tissue. In the lungs, calcareous secretions are frequently met with, and cysts, in which bone has assumed the form of teeth, have been found in most parts, but most frequently in the ovary.

Much credit is due to M. Andral, for his arrangement of the various abnormous secretions of the vessels, such as are not met with in natural structure. He has divided them into organizable and inorganizable, and has classed them generally according to their chemical composition. Among the inorganizable, some are chiefly composed of albumen, others of saline or gelatinous particles, with fat and certain colouring matters, and come gases. Among those principally composed of albumen he places pus and tubercle.

The deposition of gelatine, although it has not been detected in any of the healthy fluids, is occasionally effected in great abundance in the texture of various parts, and sometimes to a great extent in the cavity of the peritoneum or pleuræ. Irregular secretion of saline matter gives origin to calculi. Fat accumulates into distinct tumors, or replaces muscular fibre, and is deposited in various viscera. The liver is particularly disposed to this deposit. Such a condition of the liver is said to be commonly observed in those persons who die by tubercular phthisis. Colouring matter, lately shewn to be a distinct substance, forms melanotic tumors, recognizable by their dark aspect; then going through certain stages of development, of induration, and softening; and by this secretion a dark colour is given to the membranes of the lungs, or intestines, in the various vessels and glands, when there deposited.

Under this head I shall arrange gaseous substances. According to Mr. Cruikshank and Mr. Abernethy, gases are secreted from the skin. Carbonic acid was found given off from a limb confined in atmospheric air; and Mr. Edwards has proved its extrication from the lungs. Air is moreover observed in the intestines, the cellular tissue, in the peritoneum, brain, chest, and uterus.

It is needless to state the improbability of any other source than the blood for these gases: in the various situations mentioned, its disengagement appears to take place as one of the processes of inflammation, distention of those parts taking place when this fluid, if secreted, would be detained, as in the intestines.

Those tissues which are capable of organization are composed principally of fibrine, and under that head are included a variety of sarcomatous, medullary, fungoid, and scirrhous tumors. The organizable depositions performing the functions of life may take upon them disordered actions, and a softening and ulceration may ensue in the parts then newly formed; and where in such a case ulceration should be set up, little attempt at reparation, in consequence of the

low rate of vitality in such parts, can be made. Among the organizable substances must be ranked parasitical animals. Of their generation from the tissues themselves various opinions are held. Cases quoted in which children have been born affected with worms while yet in utero, would seem to place such a mode of generation beyond doubt. There does not seem much required to induce the belief that a part of the body might exhibit some of the properties of life, yet remain distinct. The formation of a new vessel, the commencement, the first stage of vascularity, when a distinct cyst is formed, would illustrate the proposition—a process no more extraordinary than the generation of hydatids, which we see to contain one within another a countless progeny.

I have thus enumerated the principal changes to be met with in the tissues, and some of the circumstances connected with them, and have referred them to the alteration of those processes which contribute to the increase and growth of parts. I propose in my next lecture to inquire into the causes which produce and the powers which chiefly direct them, thus predisposing to, and influencing disease.

ESSAYS ON HYGEIA;

OR THE

Art of preserving Public and Private Health.

BY

JOSEPH ROGERSON, Esq. of Wigan; and
GEORGE ROGERSON, Esq. of Liverpool;
Surgeons.

ESSAY II.

Manufactories—Injurious Effects on Vegetable Life.

THE great progress which chemistry has made of late years, and its extensive and successful application to the arts, has added prodigiously to the number of our national productive establishments; but from the manner in which many of these chemical and other manufactories are at present conducted, clouds upon clouds of improper gases and vapours are continually being poured out, vitiating the atmosphere to an enormous extent. It is on account of those manufactories that men are more and more uniting themselves into large masses; villages are swelling into towns, and towns into thickly-peopled cities; whilst, owing to the superior attractions of commerce, joined to the impolicy of our agricultural proprietors, country places are becoming deserted, and it is seldom now that we can discover a "Sweet Auburn"—

> "Where health and plenty cheer'd the labouring swain."

We are inclined therefore to think that it would be an object of no small importance and utility to point out what it is particularly that is so injurious to public health in places where manufactures are carried on, that so the proper remedies may be applied; and with this view we shall begin by shewing in the first place on animal and vegetable life the effects of air vitiated with

Muriatic Acid Gas.

In the arts this gas escapes more particularly from chemical works, where the preparation of an impure alkali (soda) is carried on. The process pursued is this:—Sulphuric acid is poured on coarse salt, (muriate of soda), when a decomposition ensues; the sulphuric acid, from its greater affinity, unites with the soda, and the muriatic acid is liberated in the form of gas. The sulphate of soda which is now formed from the union of the soda of the salt and the sulphuric acid, is called by the workmen the salt cake, and undergoes another process, for the purpose of separating the acid from the alkali. A quantity of charcoal is mixed with the salt cake, and forms with its sulphuric acid a sulphuret, leaving the soda in its alkaline state. The soda is mixed with some of the charcoal, and in consequence has a black colour, whence it is vulgarly called the black ash, or by some the British barilla. In this state it is used by the soap manufacturers for the preparation of soap, in the place of kelp, which it has entirely or nearly superseded. This black ash is thus prepared in large quantities by professed manufacturers of it, and, as I am told, even by some extensive soap manufacturers, who have this kind of chemical works attached to their soaperies. From this account it is clear that the only part of the process which will be hurtful to the health of the public, or to the salubrity of the places which it reaches, will be the escape of the muriatic acid gas, which is usually conducted up a chimney, and poured in currents into the atmosphere. The quantity of gas thus

streaming from these manufactories is enormous, and is sufficient to vitiate the atmosphere to a great extent.

Muriate of soda, or common dry sea salt, is decomposable into muriatic acid gas (or hydro-chloric acid gas) and soda, in the proportion, according to one authority, of 62 gas + 38 soda = 100 muriate of soda; and according to another, 46 gas + 54 soda = 100 of the salt. Its specific gravity, however, is, according to Sir Humphry Davy, such that 100 cubic inches will weigh 39 grains, though, by calculation, it ought to have been 38·4 grains. The specific gravity, compared with air, will be, in the one case 1·25900, and in the latter 1·2800; but M. G. Lussac states it at 1·2780, and Dr. Thomson at 1·2847. I have made several attempts at ascertaining the specific gravity of this gas, but have not succeeded to my satisfaction. The variableness of the results I attribute to the variable, and, I believe, uncertain proportion of water inherent in the different muriatic acid gases. At this time I will select Professor Thomson's value, since it approaches the nearest to some that I obtained.

Then, since 100 cubic inches of gas weigh 39·1839 grains, and in an avoirdupois ton there are 15667200 grains, we shall have the following statement:

Grains.	Grains.	Cubic inches.
32·1839 :	1566720 : :	100 : 39983760

which, being raised into cubic feet, will be 23139. Consequently a ton of salt will give out 39983760 cubic inches, or 23139 cubic feet, of muriatic acid gas.

The physical characters of the muriatic acid gas consist in its being very little heavier than atmospheric air, and in possessing all the mechanical properties of common air, and is capable of indefinite contraction and expansion. It is invisible, but becomes visible in contact with air, in consequence of its moisture, when it forms a light hazy cloud, or a dullish white fog. When the cloud is viewed in a vivid light, or when the sun's rays are full darting upon it, it presents prismatic colours.

Its chemical properties will shew it to be acidified; it readily reddens litmus paper and vegetable blues; it has a strong affinity for water, with which it unites in many and large proportions. At 40° Fahrenheit water absorbs 480 times its bulk of gas; and a cubic inch of water, at 60°, barometer 29·4°, will take up 515 inches of gas. It drinks up rapidly and largely the water of the air, more so in a warm state of the atmosphere; but temperature increases its bulk, and changes not its chemical properties. It is a non-supporter of combustion, and is very destructive to animal and vegetable life; its odour is pungent and peculiar; its taste acid and corrosive; its action on the organs of animated beings is chiefly exerted on the respiratory apparatus. In a legal suit for a general nuisance, tried at the Kirkdale Sessions' House, Liverpool, it was proved that horses, cattle, and men, in passing an alkali works, were made by inhaling the gas to cough, and to have their breathing much affected. In the case of Whitehouse *v.* Stevenson, for a special nuisance, lately tried at the Staffordshire assizes, it was proved that the muriatic acid gas from a soap manufactory destroyed vegetation, and that passengers were seized with a violent sneezing, coughing, and occasional vomiting. One witness stated, that when he was driving a plough and saw the fog, he was obliged to let the horses loose, when they would gallop away till they got clear of it. A verdict was given in favour of the plaintiff, and 20l. damages were allowed.

We will now proceed to shew the

Effects of Muriatic Acid Gas on Vegetables.

Exp. I.—A young myrtle, healthy and vigorous, was introduced into a receiver full of pure muriatic acid gas, when the green leaves, which were numerous, were soon altered in colour into a dead livid hue. In about three minutes the tops were drooping and dead, as well as the leaves; in about ten minutes the young stalks or shoots were altered, and in half an hour the receiver was removed, when a piece of litmus paper was immediately reddened on putting it into the glass vessel. The leaves of the tree were withered, and dropt off on the slightest touch, and even the stem was withered and brittle.

Exp. II.—The same receiver, filled with muriatic acid gas, was applied over a crocus, healthy and fresh, having some bright yellow flowers. The flowers in half a minute changed into an orange colour, becoming gradually more and more deep; but some of the leaves of the flowers assumed in three

minutes a livid whiteness, more particularly those in contact with the glass. In about five minutes the tops of the leaves of the plant were of a livid brown colour, and hung down, while the flowers dropt off and were dead. In less than half an hour the whole plant, except the flowers, was withered, and of a livid brown colour, and quite dead. The receiver was now removed, and the contained air directly reddened litmus paper; and on dropping a few drops of liquid ammonia, that hazy gas which appears preparatory to the uniting of the two gases into a solid, was most abundantly formed, shewing that a large quantity of the muriatic acid gas still remained there. The ammonia poured on the leaves and flowers produced the same effects—they also smelling strongly of the gas; and on feeling and examining the flowers, they were found putrid, pulpy, and readily dissolved on rubbing, just as they do after being a long time decayed, while the leaves were tough, somewhat thickened, and dead.

On a piece of bladder carpetted round the plant, to prevent the gas seizing the water of the earth, was found much liquid, which, on examination, proved the fluid juices of the plant strongly saturated with muriatic acid.

Exp. III.—A stream, full and continued, of muriatic acid gas, was directed from the mouth of a retort, flowing from it as it would from the top of a chimney, against a rose tree, young, green, and lively; when, in five minutes, the parts nearest the retort, and consequently receiving first the gas, were altered, particularly the leaves, some of which changed to a red colour, but most of them to a whitish livid yellow, somewhat like leaves in autumn just before the fall of the leaf. In seven minutes the leaves of the whole tree drooped, turned their edges, and were fast fading; the tops of the stems hung down, and shortly afterwards the whole tree was killed. Some of the leaves and parts of the stems were covered with dew, and the remainder felt perfectly dry; but the litmus paper was reddened wherever it was applied. The moist leaves reddened it the quickest and the deepest, the stems the next, and the dry parts the least.

Exp. IV.—The above experiments were repeated on a variety of plants with corresponding results, varying only a little with regard to the time required to injure or destroy them, whether they were covered with a thin or thick epidermis, growing along the sea coast, in hot-houses, or in the open air.

Observations on those Experiments.

These experiments were made on plants growing in soil, and the gas was in that state in which it is found when disengaged from common salt by sulphuric acid: it also produced similar effects on branches separated from the tree or plant.

They shew that muriatic acid gas is extremely destructive to vegetation, injuring immediately and killing quickly, or in a very short time, every kind of plant, both by immersion in the pure gas, or by its streams. Some of the leaves and stems were found covered with dews, which were formed by the juices of the plant absorbing the gas, and forming an acid,—these dews or secretions being forced from the plant by the improper stimulus of the gas. On very succulent plants they are produced in the greatest quantity, and the branches pour their fluids so abundantly, that they float for some depth on the top of the mercury, thus affording the chemist an admirable and easy mode of analyzing the fluids of plants for the purposes of vegetable chemistry. For these reasons we should say that this gas would be more injurious in wet and damp weather than in dry, and during night than day, except on rainy ones; because, by combining with the water, for which it has a great affinity, it forms liquid muriatic acid.

From observing the currents or streams of this gas, we should conclude that it will flow to some distance in a state of comparative purity — *i. e.* without mixing much with the atmosphere. This will take place under a clear sky and on a calm day; but, in moist weather, the gas will unite with the water of the air and become a liquid—and from its greater specific gravity, sooner fall to the ground. On windy days it will be mixed or diluted more particularly with the air; but we have found that, in the recuver, it requires considerable agitation thoroughly to mix it with the air, and to keep it so. When so diluted, it will remain mixed with the atmosphere for a considerable length of time, even for days. Places, then, in

the neighbourhood of black ash manufactories, some glass works, and some soap manufactories, where this alkali is prepared, whose air is mixed with muriatic acid gas, will have their atmosphere, in calm weather, polluted for many days even from one discharge of the acid gas, and the inhabitants will, during that time, live in a vitiated air capable of chronically disordering the mucous membranes in any part of their tract that is exposed.

The quantity of muriatic acid gas necessary for the destruction of vegetables, varies some little; but the first blast will injure, if not kill; and, in all instances, not many cubic inches will be found consumed. In the receiver the very succulent plants have appeared to me to raise the mercury higher than any other kind; thus consuming the most gas. They require, too, a more continued current of gas, effectually to destroy; but even from the first blast they seem to pine for a longer time than the others.

Experiments on Vegetables, with Muriatic Acid Gas, mixed with common Air in various proportions.

Exp. I.—A healthy growing primrose was immersed in vitiated air, proportioned so that one part of muriatic acid gas was mixed by agitation with five parts of common air. The whole quantity was contained in a jar holding seventy cubic inches. In one minute the leaves were changed, and in two the flowers; but, in less than ten minutes, both leaves and flowers were entirely killed. The receiver was now removed, when the contained air slowly reddened the test; but the leaves and flowers effected it very quickly. Similar effects were produced on other plants by this proportion of gas and air—viz. one in five.

Exp. II.—Vegetables immersed in a quantity of vitiated air, containing one part of muriatic acid gas and twenty of common air, well agitated, were not so soon destroyed as in the last experiment; but this diluted gas was found sufficient to affect the foliage immediately. In a quarter of an hour the tips and edges curled, and began to fade and die; in half an hour the tips, edges, and middle were completely destroyed, but the bottom parts less so, some even retaining a slight degree of greenness. On exposure out of doors, to ascertain if they would recover, and grow, if again submitted to good air and sunshine, it was found that they uniformly died: so that this proportion of gas will kill vegetation effectually. The branches of trees resist the destructive impression of this gas the most powerfully, and, though the whole foliage be killed, will sometimes, under very favourable circumstances, recover and put out new leaves.

Exp. III.—Into a receiver containing one proportion of muriatic acid gas to one hundred and twenty of common air, vegetables were put: the tips and edges, in twenty minutes afterwards, were fading and dying. The fading and death gradually and slowly extended; and at the end of a few hours (two, three, or three and a half) about two-thirds of the leaves, from the tops, were completely destroyed. At the end of twenty-four hours the receiver was removed, when the parts near the bottom retained some greenness and freshness, but all the rest were withered and dead. The plants and leaves do not recover on exposure to the atmosphere, but the roots shoot up, or some of the branches occasionally put out fresh leaves. The air in the receiver, after the removal of the plants, reddens test paper; and the acquired colour is made more or less sensible on the plant, according to the parts most affected.

This proportion—viz. one in twenty—is therefore sufficient to destroy the *leaves* of vegetables.

Exp. IV.—The proportion of one part or cubic inch of muriatic acid gas, well mixed and agitated, with 240 of common air, immediately curled and contracted the leaves of a king's-feather, which were green, hardy, and growing; but, after a few minutes' immersion, this effect disappeared, and the leaves again expanded. In an hour they again changed, and now the tips, edges, and parts of the leaves gradually faded and died. At the end of twenty-four hours the receiver was removed, when the above-mentioned parts of the plant alone were killed. Those parts nearest the stalks were healthy-looking, and almost as green as before their immersion.

This proportion of air and gas will injure, but not kill, vegetation. It is vitiated sufficiently to check the growth, nip the tips, and give the whole plant a

stinted and unhealthy appearance. The plants, on exposure to air, never flourish well, but are always stinted and sickly; never perfectly recovering from the effects of the gas.

Exp. V.—The proportion of 500 air, and 1 gas, made the leaves immediately curl slightly, from which they soon recovered. In about two hours and a half the edges of leaves growing on the outside of the plant were blighted; but at the expiration of 24 hours, when the receiver was removed, the edges of these leaves were the only parts injured. The middle of them remained unaffected, as well as all the other parts of the plant. The residuary air was sufficiently acid to redden slightly the test-paper. The pot and plant were removed, and exposed to the open air, where it grew, but not so well as another of the same species which much resembled it, and was placed under the same circumstances, with the exception of its freedom from immersion in acidified air. The edges of the outer leaves continued blighted; but the leaves themselves did not die, though they never recovered the green vigour of health, and never flourished.

Air, impregnated in this proportion (1 gas + 500 atmosphere) does not then kill, but it impedes growth, and destroys healthy vigour.

Exp. VI.—The proportion of one of muriatic gas in 1000 of atmosphere, affects the plant directly, curling its leaves, which soon expand. The mischief produced is very slight, contracting the tips a little. One of the buds of the flowers even during its immersion put forth and opened. The air, however, is sufficiently vitiated to make an injurious impression on vegetation, and to make its effects on plants perceptible.

Exp. VII.—The proportion of one muriatic acid gas, and 1500 air, ceases to be injurious, or at least does not produce a perceptible effect, the plants growing without appearing to feel the presence of the muriatic acid gas diluted amidst such a mass of air.

Observations and Deductions from the foregoing Experiments.

These experiments shew that muriatic acid gas is strikingly and perceptibly injurious to vegetables, till it is so diluted that it only contains one part of muriatic gas in one thousand five hundred of atmospheric air; and that it is injurious and destructive to vegetable life and growth, exactly in proportion to the gas contained in the aërial mixture, *i. e.* the more gas there is in a given space, the more injurious is that polluted air to vegetation. This is only in accordance with vegetable physiology, for vegetables breathe only the constituents of the air; so that the more the air deviates from these constituents, the more injurious it must prove. Now it has been before shewn that one ton of decomposed salt gives out 23,139 cubic feet of muriatic acid gas, or 39,983,760 cubic inches of it; and by experiment it has been ascertained, that one cubic inch will render impure, or vitiate, upwards of 1000 cubic inches of atmospheric air, so as to make it unfit for the health, growth, and existence of vegetables. Therefore the muriatic acid gas from a ton of salt will injuriously impregnate 3998,3760,000 cubic inches of atmosphere. This is the very lowest estimate; and if the proportion of muriatic acid gas be greater, it will be in proportion more unfavourable to vegetation and the health and growth of plants. If the gas exists in the atmosphere in a state more diluted than 1 in 1500, we must still regard it as an improper air for vegetation, for it will not in their healthy state be in any ways useful to plants. It is improper for two reasons: first, while it remains so diluted, it is an improper hygienic stimulus; and, secondly, since its specific gravity, and its non-chemical union will cause it to separate from the common air, and fall towards the ground, it becomes in time less and less diluted, and perhaps ultimately almost pure gas, or liquid muriatic acid.

The tips and edges of the leaves of trees and plants, and the ends of the branches and young shoots, fall the earliest victims of this acidified gas and air; but the work of destruction gradually extends from thence over the other parts. Even when this gas is so diluted that it is not sufficiently powerful to destroy more than the tips and edges, it still impairs the growth and well-being of the plant.

SULPHATE OF STRYCHNINE IN PARALYSIS.

To the Editor of the London Medical Gazette.

SIR,

THE following successful case of the use of the sulphate of strychnine in paralysis, I shall be obliged by your inserting in your Gazette.—I have the honour to be, sir,

Your obedient servant,

JOHN S. GASKOIN.

22, Clarges-Street, May 24, 1832.

Master C. S., æt. 12 years, of Flempton, near Bury St. Edmunds, from his birth until about four years since was a strong athletic boy, fond of and indulging in vigorous exercises, and possessing a mental capacity equal to the expectation of his teacher. At about that time he seemed all at once to lose his inclination for his usual recreations, leaving his pony and companions, and to prefer quietude and fondling about his mother, seeking constantly to support his head on her lap, complaining of much headache, loss of appetite, &c. His medical attendant considered him suffering from hepatic disease, with fever, which soon confined him to his bed. He was, as he expressed it, unable to sustain "the weight of his head." He at no time exhibited any symptom of delirium. The febrile state passed; his incapability to rise was for some time attributed to consequent debility; but attention was soon drawn to the paralytic affection, which precluded all voluntary action of the entire spine. For some time after this there continued great intolerance of noise, and impatience if read to, for more than a few minutes at a time. The society of his playmates was irksome to him, and he was generally impatient, with obvious loss of memory. Raising the head in the least degree, even to be placed on a pillow, caused faintness and sickness, and a violent pain, which he described as resembling his idea of a "knife striking down the back bone;" and pressure between the atlas and dentatus produced also sensations of faintness and sickness. This is not the case now. He has, however, remained in the horizontal position, on bed or sofa, until about four months ago, when, by gradually elevating the body on an inclined plane, he became able ultimately to sit nearly upright, the head and back being always necessarily supported by a high-backed chair. The change to this position caused great irregularity of circulation, blueness of the lips and whole countenance, during a little while; he fell asleep almost immediately on being raised, and was slightly convulsed during its continuance, which was generally about an hour; afterwards he experienced no inconvenience during the day. All these effects passed away at the end of about a week, and he has ever since been able to sit up with the same comfort as when recumbent, but still without the least capability to move the head, or any part of the vertebral column, from its support; and on pulling him forward, so as to detach him from that support, the head, and body too, if sufficiently removed, follow the laws of common gravitation, and fall as though loosely attached. Of course he cannot stand, but he can freely use both legs and arms, and rotate the head from side to side; and it is curious to observe the power he has acquired to propel himself on the back by the action of his extremities only; to dress and undress himself, &c. The appetite, although it has been generally good, has, during the whole illness, been monotonous and most capricious—living on a single article of food for months together, partaking of it at *all* the regular periods of his meals. The common Barcelona nuts and filberts maintained him for three months; carrots supplied him for about two months; cabbages, raisins, &c. &c. for similar periods. During the last eighteen months he has adhered solely to potatoes, eating with them a great proportion of salt and some butter, and has gained much flesh; having been before considerably emaciated from repeated bowel complaints, &c. He has passed three months at a time without taking any kind of fluid, and even now the quantity of a tumblerful will sometimes suffice him for two or three days. The efforts of the mother, in tempting him with those delicacies and varieties most sought by children, were unceasing and unavailing, during the whole time of this dietetic peculiarity; the smell of any food, even of bread, which, from the commencement of the attack, he had not tasted, or tea, or other than the selected one for the time being, induced palor, faintness, nausea,

and occasionally vomiting. Such is the history of the case received from the mother, on the arrival of the boy in London, on the 29th October, 1831 *.

On carefully examining the vertebræ, the bones were found to be sound, and perfectly in position, one with the other; and the muscles immediately connected with them not diminished in volume. The countenance was healthy, the visceral functions rightly performed, the pulse good, his appetite sufficient, and his spirits excellent; and he was full of confidence that at least some instruments might be devised here to enable him to walk, which was the extreme hope in his visit to the metropolis, and was undertaken by his mother rather as a duty than with any expectation of advantage to her child.

This being a case of that form of paralysis which follows fevers occasionally in children, and having resisted, as such cases unfortunately generally do, the careful and patient administration of the most approved remedies—as local bloodlettings, perpetual blisters, and caustic issues; mercurial frictions both in the spine and extremities, and mercury internally also during several months, &c. &c. &c. and all without the slightest medicinal or constitutional effect—I proposed, before beginning any new and active plan of treatment, that we should have the valuable assistance of my friend, Mr. Brodie. The prognosis, of course, was not favourable, and the inapplicability of instruments was confirmed by one of our mechanists in that department. Hope fell, therefore, almost to despondency.

Mr. Brodie concurred in the propriety of the use of the *sulphate of strychnine*, and the patient began, on the 1st November, with one-thirtieth of a grain three times a day, and the dose was gradually increased one-thirtieth at a time, until, on the 16th November, he took six-thirtieths three times a day. On the 19th he experienced slight dizziness, with great palor, and loss of consciousness for about half a minute; but no twitchings of the muscles. The dose was, in consequence, reduced to four-thirtieths of a grain, and it was again gradually increased to one-fifth on the 24th; and no recurrence of any unpleasant symptom taking place, it was continued, without intermission, three times a day until the 12th December. One-fifth of a grain being, from the above-named circumstance, considered probably the maximum dose the patient could bear, no increase was attempted, lest it might occasion disagreement and the necessity of its ultimate discontinuance. *Sulphate of strychnine*, from Pelletier's, of Paris, and frictions with a coarse flannel along the whole spine, for twenty minutes, were the only means employed in London.

On the 27th November he expressed some dislike to his long-accustomed food of potatoes, &c. in exactly a similar manner as he had before done prior to the change of his other kinds of diet. He wished now, however, for meat; and from this time he partook of all the diversified diet of the family—of all his former antipathies—meats, bread, wines, tea, &c. &c.

On the 4th December he felt a sensation of power in his neck, and was, in the course of the day, able to elevate it from its support and move it backwards and forwards. It will be remembered, he could before partially rotate the head when supported. No further improvement was noticed until the 11th December. At 4 o'clock on that day the very frequently repeated trial was made, but on pulling him forward the back as usual dropped. About 7 o'clock he said to his mother, "I don't know, but I *feel* as though I could stand." He made the attempt with assistance, and tremulously did so for a few seconds, and in the course of the evening, by merely placing his hands on the table, &c. He was carried, as usual, to bed by his servant; and we may better and more easily imagine than describe the feelings of his mother, on perceiving him reflected in her glass, while at her dressing-table, advancing on limbs which had not borne him for so many years, upright, firmly, and alone, into her room the following morning. The joy on effecting this surprise (for he had risen before the arrival of his servant, and had dressed himself therefore without assistance), and the excitement of the boy on regaining the use of his legs, was so great that he could not be controlled from using them the whole day; he suffered, how-

* The above history having been sent to Mr. Gadge, of Mildenhall, his medical attendant, for correction, he returned for answer, "Your history of the case appears so perfectly correct, that I could not suggest any addition or alteration."

ever, no other inconvenience than that of fatigue.

During the employment of this active and powerful remedy, the patient experienced no sort of disturbance other than that described to have occurred on the 19th November; and every hope being fulfilled, the dose was decreased to a tenth of a grain three times a day on the 13th December, and discontinued entirely on the 16th.

It would be difficult to define the exact nerves affected in this disease, but the practical deduction will be less intricate. A powerful medicine, known singularly to affect the actions of the muscles, is employed and watchfully pushed to a decided influence on the nervous system; and its continuance at that dose produces three consecutive effects. In twenty-six days the restoration of the natural functions of the nerves of taste, the stomach, &c., in thirty-four days the return of voluntary power in the muscles connected with the cervical vertebræ, and ultimately, in forty-two days, over those of the whole vertebral column. As far as I have remarked, in this and other cases, of the good effects of the sulphate of strychnine, its most useful mode of administration is that of exceedingly gradual increase, and its continuance at that dose which produces *any* constitutional action. It must be observed that each improvement in this case was sudden, and, I am happy to add, continues permanent; for a letter from his mother, dated 18th January last, says, "My boy continues quite well, but his ankles swell a little; he rides his pony, which I think less fatiguing than walking, for I cannot, without great difficulty, keep him within doors." And a friend from the neighbourhood informed me, on the 29th of March, that his ankles no longer swelled, and that, a few days before, he had been "out with the fox hounds," and had followed them a sharp run of about eleven miles, with a field of sportsmen, many of whom had been for years in the habit of pulling up and chatting with him as he lay on his back fronting his house, to enjoy the passing of the hunters to their sport.

A letter, 13th instant, says he continues in excellent health.

ON A PECULIAR CAUSE OE LACERATION OF THE PERINÆUM.

To the Editor of the London Medical Gazette.

Sir,

Among the causes which, towards the close of parturition, impede the entrance of the child's head into the world, I do not find mentioned by authors one which recently fell under my notice. The case was that of a young woman, of healthy conformation, in labour for the first time. In about ten hours from the commencement of uterine contraction, the head touched the perinæum; it emerged from underneath the arch of the pubes by the slowest degrees, keeping the perinæum permanently distended. When about half the head was born, though the pains continued with unmitigated severity, it ceased to make further progress, and became immoveable. On examining carefully, I found that the margin of the perinæum below, and of the vaginal orifice on each side, was firmly and forcibly applied round the head, precisely in the line of the coronal suture, so that the integuments in that situation were constricted, as well as depressed. The advanced scalp soon became tumid, so as to disguise the elongated form of the bones beneath. At this time, from the thinness of the perinæum, the intense local suffering, the vigorousness of uterine contraction, and the signs of disturbed action in the brain, I could not avoid being apprehensive of laceration of the perinæum, on one hand, or of a rupture of the uterus, or a sudden cerebral lesion, on the other. No mischief, however, of any kind was produced; the continuing compressive force diminished the bulk of the confined portion of the head; and at the end of about ten minutes, during which I simply sustained the perinæum, and endeavoured to moderate the patient's furious irritability, it was expelled. The child was uninjured, but the whole head, after the recovery of its natural shape, was unusually large.

Reflection on the circumstances of this case has suggested to me a probable occasion of those lacerations of the perinæum, which, from happening notwithstanding the utmost care to the contrary, are considered, and may be termed, unavoidable. The shape of the

child's head during its birth is, it will be remembered, conoidal, the base being represented by the bones of the face, and the apex by the point of union of the occipital with the parietal bones behind. The mode in which the head passes the outlet of the pelvis and vulva is as follows. That part of the occiput between the protuberance and great foramen is applied against the lower edge of Camper's triangular ligament; on this it turns as on a pivot, thus causing the chin to recede from the sternum; and the vertex, sagittal suture, anterior fontanelle, forehead, nose, and chin, to be successively disengaged. The distance of these parts from the centre of motion in the occiput of course differs—the greatest, however, is that terminating in the anterior fontanelle; but this is, at the same time, the shortest of the antero-posterior diameters of the entire head.

In the next place, it must be recollected that all the cranial bones are not equally susceptible of approximation, or similarly capable of overlapping each other. The frontal bones, by reason of their connexion with the face, are less free and moveable than those of the occiput and sides of the head; a consequence of which is, that the bulk of the fore part of the head cannot be reduced or diminished in a corresponding degree with the back part.

As often, therefore, as the passage of the head through the external parts is rendered difficult, the principal resistance will be towards its anterior part, and particularly about, and on each side, of the anterior fontanelle, in the course of the coronal suture. The difficulty adverted to may be constituted by any one, or by a combination, of these conditions, namely, unusually large size or firm ossification of the child's head; great rigidity and imperfect elasticity of the perinæum; a similar state with extraordinary narrowness, of the vaginal orifice. If, with a hindrance so derived, a width of coronal suture greater than natural should chance to concur, we are furnished with the requisite conditions for the production of the kind of case such as is related above.

In that instance, interruption merely was given to the natural progress of the labour. It is evident, however, that various evil results might occur from such an accident, and that a very probable one will necessarily be—from the forcible pressure of the edges of the then projecting frontal bones against its margin—more or less laceration of the substance of the perinæum. Much additional suffering must always be created, and this I can conceive to be so acute as to give rise, in certain habits, to a derangement of the cerebral system, great enough to determine convulsions. The obstruction might likewise be so powerful and obstinate that the action of the uterus, instead of overcoming it, would rather go on to the production of a breach of continuity; or might be so wearied by fruitless efforts as at length to terminate in incapability of further exertion, while the exhaustion of the general system would be commensurate. To the child the consequence might be, by compression of the brain, apoplexy, and death.

If it be asked, why then have not convulsions, ruptured uterus, exhaustion, or an apoplectic condition of the child, been noticed to occur more frequently under such circumstances, I answer, they have been prevented by the yielding and laceration of the perinæum, which has happened to a sufficient extent to entirely release the head from constriction, and to allow the projecting edges of the frontal bones, followed by the remaining head and face, freely to escape. Regarded in this light, laceration of the perinæum, as a sequence of contraction of the margin of the external aperture on the interosseous space formed by the anterior fontanelle and coronal suture, may be considered not only unavoidable, but even salutary. Or, to speak more strictly, there is presented to nature a choice of evils, from amongst which, ever consistent and beneficent, she selects the least.

An inference, however, must not be drawn from this view of the case in favour of neglecting to take precautions against the occurrence of laceration of the perinæum, still less of promoting or encouraging such a termination. The inadequacy of the natural agents of parturition to rectify the deviation and bring the delivery to a happy conclusion, ought not to be hastily assumed; never, indeed, till laceration is on the point of commencing, and then it should be the practitioner's chief object to restrain and limit the extent of the injury. But care should be likewise taken that the accident is not unnecessarily provoked by indiscriminate and too stre-

nuous attempts to avoid it. These consist in urgently compressing the anterior portion and edge of the perinæum against the prominent bone, and in offering, through its surface generally, active resistance, instead of affording passive support; and they unquestionably would only contribute to the speedier and complete thinning, and thereby the readier laceration, of the part.

With respect to other remedial means, in extreme cases of arrest of this sort, craniotomy might become justifiable or indispensable; but under ordinary circumstances, all that can be done judiciously is merely to increase the cohesive power of the perinæum by the exact apposition of the hand, and to abstain from inviting, by injudicious pressure, the imprisoned bones to force for themselves a passage through the skin.—I remain, sir,

Your most obedient servant,

SAMUEL MALINS, M.D.
Lecturer on Midwifery.

Liverpool, May 24th, 1832.

SALINE INJECTIONS IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

A CASE of cholera has just been treated in this establishment upon the plan lately recommended by Drs. Latta, Lewins, &c. in Edinburgh. I have drawn up the particulars, and, as they may be interesting to some of your readers, I beg to offer them to you for insertion in the next number of the Gazette.

I am, sir,
Your obedient servant,
ALEXANDER TWEEDIE.

3, Abchurch-Lane, June 4, 1832.

CASE I. *in which nearly a gallon was injected, with temporary improvement, but ultimate failure.—Appearances on Dissection, and Remarks by* Mr. Tweedie.

Charles Lamb, æt. thirty-six, admitted into Abchurch-Lane Hospital, at 11 A.M. Sunday, June 3, 1832, from the "Dispatch," Whitby sloop, now lying off Wool Quay, Custom-house.

This man is a publican and gunsmith at Whitby, which place he left on last Tuesday week, as a passenger on board the above vessel. He arrived in London eight days ago, and immediately went on shore, where (in the neighbourhood of Wapping) it appears he has been living in a most intemperate way, till, having spent all his money, he found a bed for the last two nights on board the vessel in which he came to London as a passenger. Last night he went to bed sober, about ten o'clock, having eaten a little bread and bacon for supper, and being in good spirits and in his accustomed health *. About midnight he got up to go to the privy, and a looseness that now commenced kept him awake till five o'clock, when he began to vomit. At 7, cramps ensued. At 9, he was seen by Mr. W. Smith of Gracechurch-Street, who found him pulseless, and, in his opinion, past recovery. At 11 he arrived here, having had, shortly before admission, a cordial and opiate draught. His state, on admission, is as follows:—

General aspect of features very much collapsed and congested—of a dull, leaden, livid hue. The eyes deeply retracted, nose shrunk, lips livid and cold. Eyes half open, and turned up; pupils natural; tongue covered with a yellow moist fur—temperature beneath it 79°. Hands lividly blue; pulse not countable; skin damp and inelastic; feet cold, and partly livid. Voice very low; breathing 36. There has been a cessation of vomiting and purging since nine o'clock; from report they were very plentiful, and like water. He has not urined since five or six o'clock; complains of pain in the back. He is now suffering from cramps in the hands, legs, thighs, and abdomen, and is urgently craving for cold water. He states, that early in the morning he felt a sensation of singing in the ears, and was very deaf. He has worn a flannel belt (without reference to cholera) many years.

11 A.M.—Ordered warm bed; hot bottles to feet; Calomel, gr. v. ex aquâ; Sinapism. amplum thoraci et abdomini.

12th.—Has been much cramped, restless, and very thirsty. He retains small sips of water. Sinapism has had no effect; removed. It was now determined, in consultation with Dr. B. Babington, that, as the case appeared to promise an almost immediately fatal result, the plan of injecting into the circulation, as recommended and adopted in Edinburgh, by Drs. Latta, Lewins, &c. should be practised.

Quarter past 1 P.M.—The necessary apparatus being provided, we proceeded to the operation shortly after one o'clock, at which time the pulse at the wrist was scarcely perceptible; at the heart it beat 148 in the minute. Temperature under tongue 84°. Respiration 36, and he had just been cramped, and was very restless. The median cephalic vein of the right arm was opened for

* He subsequently acknowledged that he had had irritable bowels nearly all the time he had been on shore.

the insertion of the tube of the syringe, and the following was the fluid for injection, as advised by Dr. Lewins, &c.—viz.

Muriate of Soda, ʒij.; Carbon. of Soda, ℈ij.; water, 60 ounces; temperature from 110° to 115°.

2 P.M.—After a very gradual injection of 22 ounces, respiration 30; pulse at wrist 120. Has just had cramp in the right hand.

Quarter past 2 P.M.—The patient appearing much collapsed after 34 ounces had been injected, the following draught was given:—

Brandy, ℥j.; Ammon. S. Carb. gr. v.; Aquæ calidæ, ℥ij. M.

This was instantly rejected; and after a brief pause, arising from the apprehension that he was about to die under our hands, the injection was again proceeded with very gradually.

3 P.M.—Fifty-five ounces have now been injected. His pulse is obviously stronger (120); voice firmer; countenance more lively. He expresses himself as feeling better, and is quite free from cramp. Has just past a stool of alkaline property, fluid, with floating flakes, and of a pale yellow hue—in quantity about ℥vj.

Sir William Russell, who had been invited, now arrived, and not only approved of the measures that had been adopted, but was of opinion that the injection should be repeated if the patient should lose any of the ground that had been gained. Sir William looked upon it otherwise as a hopeless case.

Half-past 3 P.M.—Another stool, like the last, but less in quantity, and rather feculent in smell. Hands are warmer; pulse 120, weaker than half an hour ago. Respiration 36. Says he is better. Has taken, to quench his thirst, the following draught occasionally, and is not restless.

Acid. Tartar. gr. xx. Sodæ Carb. gr. xxv. Aquæ, ℥ij. actu efferv. sumend.

Ten minutes to 4 P.M.—Pulse irregular, and very weak—so indistinct as not to be counted at the wrist; between 120 and 130 at the heart. Breathing 42; surface warm, but not moist; hands less warm than when last felt; voice husky; complains of thirst; countenance again more collapsed.

Rep. Injectio.

Quarter to 5 P.M.—Fifty-four ounces have been injected. Pulse 120, stronger than at any previous period. Voice firmer; intellect very complete; he notices all things around him with much shrewdness, and looks greatly enlivened. Whilst the injection was proceeding he had a dejection, about a pint, coloured like dark urine; but he says he is sure there is no urine in it. It smells feculent. He has drank water and thin arrow-root from time to time, all of which he retains. There is a gentle dew on the forehead; breathing 42, very hurried. Whereas before the injection he was very restless, he has now turned quietly on his side, and is composing himself to sleep.

℞ Ext. Hæmatoxyli, gr. x. T. Catechu, ʒss. Aquæ Menth. Pip. ℥iss. M. ft. haust. st. s. et post sedes singulas.

5½.—Pulse keeps up; perspiration over the body.

6 P.M.—Has been in a profuse clammy sweat since half-past five o'clock, and now complains of intolerable heat, tossing off the bed-clothes; pulse is very irregular, scarcely to be felt, ranging from 120 to 130; breathing 48, much oppressed; there is an unsettled manner about him, which, however, does not amount to delirium; has just passed a stool similar to the last noticed, and about half-a-pint in bulk.

6½ P.M.—He is pulseless, and gasping for breath slowly, as if now dying. Though alone, with only nurse's aid, I am about to inject again.

¼ to 7 P.M.—Dead; ℥xij. of fluid thrown in; but he did not rally, and died under the operation.

The body was opened 13 hours after death, in presence of Dr. B. Babington, Messrs. W. Smith, Charles Gaselee, and A. Tweedie.

External.—Much lividity posteriorly, as well as anteriorly, on the legs, thighs, scrotum, and parts adjacent; ears quite blue; limbs rigid; configuration very muscular.

Chest.—Pleuræ plastic and dry; right lung healthy; left lung adherent by old bands; neither inordinately congested; heart rather flaccid; pericardium contained ʒij. of liquor; coronary veins *not* unusually injected; right ventricle and auricle loosely full of dark, currant-jelly like blood, not fairly separated, and quite as dark as in other cases of cholera; vena cava ditto, and not remarkably distended; left ventricle the same, in less quantity; left auricle empty; aorta contained a small quantity of the like.

Abdomen.—Cavity dry and plastic; omentum natural, if any thing a little redder than healthy; surface of small intestines not nearly so pink as in most cases of cholera, indeed very little redder than in ordinary disease; liver pale, large, rather fatty, and containing little blood; gall-bladder nearly empty; stomach natural without, *pale* within, and containing about ℥viij. of the fluids lately taken; there was roughness, in a very dubious degree, of the inner coat; small intestines, from duodenum downwards, containing a large quantity of watery fluid, like very thin gruel, or barley-water, of alkaline re-action; mucous membrane quite pale and watery; no pasty matter adherent any where; at the lower part of ileum a few, *and very few*, single glands enlarged; large intestines natural without, ensanguine with-

in, containing similar watery fluid to that found in the small bowel; no enlarged glands; kidneys, &c. &c. sound; urinary bladder literally empty, and firmly contracted.

Head.—Brain and membranes natural; there was about ℥j. of fluid in the ventricles, and at the base together. No other morbid existence was discovered.

Remarks.—There were many singularities in the life and death of this patient. Before the first injection of fluid he was in a most restless condition, with a cold sweat over his body; and in such an extreme state of collapse and depression that there seemed no prospect of other than an almost instant death. The injection rallied him; but even whilst this was in progress, at one period (2¼ P.M.) he appeared so nearly *in articulo mortis* that we were about to desist; nevertheless, we persisted, and he roused; the cold sweat completely ceasing. At the second injection, too, we thought that he was breathing his last; however, towards its termination, he became marvellously restored; his pulse and warmth rose; and his feelings were so happy that he was enabled, to our great delight, to make shrewd remarks on the objects and events about him, and even to be jocose and facetious respecting them. This state, as well as the degree of restoration that had now been attained, will best be illustrated by the relation of a few anecdotes. Thus, towards the close of this last injection, I happened to notice, "Well, we have almost injected fifty-six ounces." Ah!" replied he, "I shall contradict that: there are only fifty-five and a half: don't you see you have spilled some." It was observed that the veins on the back of his hand seemed more full. "Yes," he said, "something must be full after all this." Some brandy was put into a little arrow-root for drink. "What is here," he asked, "besides arrow-root." "Nothing but a little sugar," said nurse. "I am sure there is something else." "Only a little brandy to flavour it." "Do you call that nothing," said he, as he finished the grateful draught with much apparent satisfaction, exclaiming, in a tone of hearty sincerity, as he returned the cup into nurse's hand, "thank God for that."

In this tone did he converse, familiarly and jocularly, so that it was impossible for us to withhold our sympathy and congratulation; but this bright prospect was only of brief duration, for within half an hour he began to flag; perspiration came on, at first warm, then cold; and rapidly fatal was the advance of his illness from this period.

This was one of those cases in which from the first it seemed that "death had marked him for his own."

It is strange that though one hundred and twenty-one ounces (nearly a gallon) of fluid passed into the vein, the blood in the *right auricle* of the heart, and indeed every where else, should nevertheless have been so thick and dark as it was: this is the more remarkable when it is remembered that twelve of these ounces were thrown in during the last eight or ten minutes of life—in fact, whilst the patient was dying. The serous cavities were quite dry, but the quantity of watery fluid in the bowels was very plentiful; so that it would seem as if the watery parts had found their way into the intestines from the circulation in an infinitely more expeditious and complete manner than the existing state of physiology had prepared us to anticipate.

As immediately arising from, and connected with this case, I would most respectfully throw out the following queries:—

1st. What would be the effect of such an injection into the veins of an individual in sound health?

2d. Whether there are not other diseases besides cholera in which this mode of treatment may be used with advantage?

3d. What is the explanation of the circumstance noticed in the postmortem inspection of the foregoing case, that no visible trace of the injected fluid was to be found in the general mass of the blood, although ℥xij. were injected as the man was dying?

4th. Is it advisable to inject muriate of soda in this way? Common salt is a purgative, and is in daily use for that purpose in enemata. Will it not be purgative also when injected into the veins? The quantity contained in one hundred and twenty ounces of fluid injected is ℥ss. quite enough to *induce* purgation under common circumstances.

Case II.—*In which Injection into the Veins was practised without success at the Free Hospital in Greville-Street.*

On the 31st of May, about one o'clock P.M. one of the nurses was attacked with cholera. She had the usual symptoms,

but with the cramps more than commonly severe, and was passing fast into a state of collapse. The saline treatment was used by Mr. Whitmore, as in the cases formerly detailed in this Journal, under which she gradually rallied, and at noon, on the 4th inst. she was considered by her attendants as nearly out of danger. About 7 in the evening of that day, however, she was seized with violent cramps in the stomach, when the saline remedies were suspended, and some opening medicine, with a small quantity of calomel and opium, was administered. She continued to get worse, and in consequence of a mistake on the part of her nurse, the saline powders were wholly omitted. She continued very ill all day on the 5th, the stomach being so irritable that no medicines could be retained for a moment. On the 6th she was so ill that not the slightest hope was entertained of her recovery, and Dr. Stevens proposed saline injections into the veins as a *dernier ressort.* There were six medical men present, all of whom concurred in the propriety of the experiment being made. The operation was performed by Dr. Stevens and Mr. Marsden, one of the regular attendants at the hospital. About three pints, of of the same strength as used in Scotland, were thrown in, soon after which the pulse rose, becoming fuller, and the blood more florid. The apparent benefit, however, was but temporary, and at seven she expired.

It may be proper to state that the relapse, with cramp in the stomach, which led to the suspension of the saline treatment, was brought on by her having eaten a lobster, (a whole one!) which had been brought into her clandestinely by a companion, a few hours before.

There is reason to believe that this case was complicated with an affection of the brain, and altogether it seems to have been one in which little was to be expected from the injection; at any rate it did no harm, and perhaps prolonged life a few hours.

As an offset to the above case, it may be stated that Mr. Whitmore has had in all about thirty cases of cholera since he commenced the saline treatment, and of this number he has lost only two; one of these cases was that of a man in the above hospital, who was brought in in the very last stage, and died soon after admission; the other is the case of the nurse detailed above. In addition to these, there have been lately in the same hospital seven cases, six of which were very severe. They have all been under the saline treatment; four of them have been dismissed cured, and the other three are now considered as out of danger.

CASE III.—*In which Saline Injection into the Veins was adopted with success* *.

MARTHA SMITH, aged 38, a noted drunkard, thin and debilitated, in 6th month of pregnancy, admitted into the hospital at 8 P.M. 16th May, 1832.

It appears she has had vomiting and purging since Sunday morning, 12th instant. Cramps came on about four hours ago in both legs; great evacuations, both upwards and downwards, like dirty water. The countenance is now collapsed, eyes sunk, tongue cold, pulse imperceptible at wrist, very small in brachial artery, 124.

℞ Muriat. Sodæ, ʒiij. Carbon. Sodæ, ʒj. Aq. Calid. ℔vj. solve ft. Enema statim injiciend. Sinapisms to spine and epigastrium. Let her be placed on heated tin mattress.

Nine.—Has a good deal of vomiting; is getting warmer; pulse now perceptible in right wrist; tongue warmer; she allows the enema to come away without giving notice to nurse.

Saline Enema as above, with the addition of white of eggs, to be repeated every half hour.

Ten.—Vomiting and purging of watery fluid, with slimy matter in it.

Half-past ten.—Cramps have returned severe in left leg; pulse again quite imperceptible; urgent thirst, and constant vomiting.

Rept. Enema et Pulveres Effervescentes.

Half past eleven.—Breathing becoming much affected; extreme restlessness; cramps severe in legs, and every symptom of sinking.

Let the following saline solution be injected into one of the veins of the arm.

℞ Muriat. Sodæ, ʒj. Carbon. Sodæ, gr. x. Aq. calid. ℔iij. solve temp. 105° F.

Noon.—When about ℔j. had been thrown in, the pulse was perceived to flutter at the wrist, and gradually strengthened as the injection was proceeded with. By the time ℔iiiss. had been injected, the countenance, which was before quite death-like, now beamed with the appearance of health, and she began to converse freely. Pulse 96, moderate.

To have ℥j. of gin in warm water, with sugar.

Half-past one.—The gin was immediately

* For this and the following case, to which we referred last week, we are indebted to the Central Board of Health.—ED. GAZ.

rejected; pulse has again gradually become imperceptible, and respiration quick and laborious.

Two.—Let the venous injection be repeated to ℔vij.

The effect of the injection as formerly was very striking. To see an individual who seemed *in articulo mortis* brought back, as it were, in so short a space of time to an apparently tolerable state of health, could not but astonish the beholder. Before the injection was finished, the pulse had returned to a healthy fulness and firmness. Expresses herself much relieved; no purging, but vomits a good deal of serous matter.

℞ Muriat. Sodæ, ʒij. Carbon. Sodæ, ʒj. Alcohol Dilut. ℥j. Aq. Calid. ℔iij. M. ft. Enema statim injiciend.

Four P.M.—Enema retained about an hour and a half; surface of body now comfortably warm. She has not passed more fluid by stool than was thrown into the rectum.

Six P.M.—Has slept softly for an hour, the first sleep she has had for many days.

℞ Subm. Hydrarg. gr. v. Pulv. Opii, gr. ss. M. sumat stat. et rept. 3tia quâque horâ.

Nine P.M.—Complains much of vomiting and sense of weakness; countenance rather collapsed; breathing difficult.

Let ℥lxxx. be injected into the veins again gradually.

After the first few ounces were thrown in, she complained of an acute pain at the epigastrium and faintness, probably arising from the fluid being thrown in too fast upon the heart, or from the passing of a bubble or two of air, which may have got in from the inaccuracy of the injecting apparatus used: be that as it may, the circulating system was so much affected, that the pulse, from being distinct though feeble, became quite imperceptible; but on stopping the injection for some minutes, the pulse gradually returned, and the pain abated. She expressed herself as always getting relief from the operation.

Eleven P.M.—Vomiting continues urgent.

App. Emp. Cantharid. Epigast. Effervescing draughts occasionally.

17th.—Has passed about ℔j. of urine, of natural appearance; this is the first she has made since she was brought in.

From this time she went on gradually to improve, but stomach continued very irritable, and the matter vomited was bilious.

On 21st labour pains came on, and she was delivered of a still-born female child.

22d.—Symptoms of phlebitis in right arm came on, proceeding from the wound upwards; but this yielded to the ordinary treatment, and she may now be considered out of all danger, though she is not yet reported cured.

THOS. CRAIGIE, M.D.

Leith, 26th May, 1832.

CASE IV.—*In which Saline Injection into the Veins was adopted with temporary improvement, followed by Relapse and Death.—Appearances on Dissection.*

GEORGE COUSINS, aged 10, was brought into quarantine at 9 A.M. 13th May, on account of his mother being ill of cholera. About an hour after admission, began to vomit and purge, and it appears that he has had diarrhœa severely all the morning. Pulse 120, extremely weak; complains much of sickness; countenance collapsed; areolæ rather dark under the eyes; voice very weak. He had hot air-bath immediately, and got the following dose:—

℞ Ol. Ricini, ℥ss.; Tinct. Opii, gtt. xv.; Aq. Menth. Pip. ℥iss. M. ft. haust.

Half-past 11 A.M.—Draught retained; sickness has gone off; complains of heat of bath. Let it be removed.

Noon.—Has vomited some watery matter, with undigested potatoes in it, and again a rice-watery fluid with flocculi. He has now a considerable degree of jactitation; countenance more sunk, and great desire for cold water.

These symptoms went on increasing in severity, in spite of sinapisms to spine, effervescing draughts, calomel and Dover's powder, warm water enemata, &c. and head symptoms were now making their appearance.

Half-past 2 P.M.—Pulse quite imperceptible, and has been so for an hour and a half. He lies quiet and drowsy, with eyes turned upwards; face bedewed with cold perspiration; hands and feet cold, and very blue.

My colleagues, Drs. Combe and Lewins, saw him with me at this time, and concurred with me in thinking him not only beyond all hope of recovery, but likely to die within an hour or two.

From what I had seen of the resuscitating powers of Dr. Latta's treatment on the boy's mother this morning, by venous injection, I determined on giving it a trial, though this was a case rather likely to bring discredit on the remedy than otherwise. The following solution, at temp. 102 F. was slowly injected into the median basilic, by means of a common silver blow-pipe attached to Reid's enema syringe:—

℞ Muriat. Sodæ, ʒj.; Carbon. Sodæ, gr. x.; Aq. Calid. ℔vj. solve temp. 102.

3 P.M.—A few minutes after the injecting was commenced the pulse returned to the wrist; the blueness and coldness of the extremities gradually wore off; the countenance was much improved, and the whole fluid was injected within twenty minutes.

Half-past 3 P.M.—He has now a healthy blooming appearance, is sitting up in bed and looking about him as if awoke out of a

dream. Pulse 110, natural; extremities of good colour, and warm; voice much stronger.

Half-past 4 P.M.—Pulse has been gradually falling off since last report; is getting listless, and dislikes to be troubled with questions. Breathing becoming laborious, and head symptoms more marked, with squinting to a slight degree superadded.

7 P.M.—Pulse again imperceptible; respiration quick and laboured; countenance collapsed; tongue and breath cold; says he is dying.

Let the venous injection be repeated to ℔iij.

Half-past 7 P.M.—Pulse immediately returned, of natural strength and fulness, and continues so.

9 P.M. — Lies very quiet; pulse good; breathing more natural; surface of body covered with a warm perspiration.

10 P.M.—Large watery evacuations from the bowels came on soon after last injection. The quantity cannot easily be guessed, but must have been considerable, as it is running through the mattress on the floor. Pulse scarcely perceptible; screams loudly, like a child in hydrocephalus.

11 P.M.—Pulse quite imperceptible; is sinking fast. Venous injection attempted a third time, but desisted from, as it was not productive of the first good effects. Both pupils much dilated. Died at 2 A.M. 14th.

Dissection 15 hours after death.—On exposing the brain and spinal marrow, but before opening their investing membranes, the least pressure with the fingers on the middle of the hemispheres of the brain caused a remarkable undulation down to the middle of the back, shewing the existence of a fluid beneath the membranes, and, on opening them, about two drachms of pure serum flowed out. The surface of the brain was rather vascular, and the blood in the most minute vessels particularly bright; a few ecchymosed spots on its surface. All the other viscera were found healthy. The urinary bladder contained about half an ounce of urine.

THOMAS CRAIGIE, M.D.

Leith, May 26, 1832.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Outlines of Medical Botany, comprising Vegetable Anatomy and Physiology, &c. &c. By HUGO REID, Member of the Royal Physical Society, and of the Society of Arts, &c. &c.

A VERY good little book—well timed, and well executed. It is divided into two parts; the former contains a succinct account of the structure and functions of vegetables, sufficiently detailed for elementary purposes; the latter includes an exposition of the natural arrangement of Jussieu, and of the system of Linnæus, with some useful comparative tables, shewing the situation of any plant in either of these classifications. The descriptions are illustrated by means of woodcuts, so that the eye is appealed to at the same moment as the understanding. The student of botany will find Mr. Reid's "Outlines" of considerable assistance to him.

A Practical Treatise on Uterine Hæmorrhage, in connexion with Pregnancy and Parturition. By JOHN T. INGLEBY, Member of the Royal College of Surgeons in London, &c. &c.

"*Nec temere, nec timide,*" is the motto chosen by the author of this volume—would we could say that it was appropriate. Our duty as impartial critics compels us to express a very unfavourable opinion of this treatise. The views taken by the writer, and the principles he lays down, are generally limited, and frequently erroneous; so that, with every disposition to be lenient, we must caution our readers against trusting to the method of treatment recommended by Mr. Ingleby under the appalling emergencies often presented by uterine hæmorrhage.

Reports of Medical Cases. By DR. BRIGHT. Vol. II. Part II. Price 9*l.* 9*s.*

Phenomena and Cure of Chorea.

DR. BRIGHT divides chorea into acute and chronic; but we pass by the latter as constituting rather an awkwardness than a disease, being but some gesticulation, independent of the will, and kept up by habit.

The acute form, it is almost superfluous to observe, affects children more frequently than adults, and our author thinks girls oftener than boys, a remark in keeping within our experience. We shall not describe symptoms which we presume to be well known to most of our readers, but confine ourselves to the more formidable or less frequent

modifications of the disease, beginning with the only instance before us in which the malady proved fatal.

Sarah Ford was admitted into Guy's Hospital, when 13 years of age, (Jan. 1825) labouring under well-marked chorea. The July preceding she had had an attack of rheumatism, from which she had entirely recovered. She sometimes had mental delusions, and had suffered about five weeks before her admission from sore throat and globus hystericus. Her spirits and strength were now depressed; she was constipated, and the menses had not appeared. Purgatives, metallic tonics, narcotics, (hyoscyamus with camphor) and the shower-bath, were the chief remedies used, and she recovered so as to be discharged in April. She continued free from complaint for many months, when, from mental agitation, she suffered a relapse; but this was not of long continuance, and she then continued well till about Easter 1829, at which time she met with a severe disappointment, (a love affair) and this was followed by an immediate return of her former complaints.

"She was brought to Guy's, being now seventeen years of age. It was with difficulty she could either stand or sit: she threw herself about in every direction, contracting her mouth forcibly, speaking with the utmost difficulty; and when desired to show her tongue, protruding it most forcibly, and shutting her teeth upon it.

"What mode of treatment was adopted I do not know. She seemed, however, to improve under it at first, but then became worse, and at length could not be taken from her bed: she threw herself in all directions, beating her hands against the boards, which were put to prevent her falling on the ground; and she bit her tongue most dreadfully. She gradually became very low and exhausted; her mouth and teeth covered with sordes; and she had nearly the aspect of a person labouring under fever. She seemed sensible, though, from the difficulty of answering questions, she sometimes appeared incoherent. All these unfavourable symptoms increasing, she gradually sunk.

"*Sectio Cadaveris.*—When the dura mater came into sight, it was obvious that no effusion had taken place into the ventricles, as the convolutions were felt distinctly, and seen through the membrane. The skull was thin, and the sella turcica and other projections of the basis were rather prominent. A small quantity of fluid escaped from a wound made by the saw through the dura mater into the arachnoid; the effusion, however, was but little: the membranes looked moist, and between the convolutions slightly watery. There was not the least coagulum in the longitudinal sinus, nor in the lateral sinuses, and only a few drops of uncoagulated blood.

"The surface of the convolutions was rather more vascular than usual, as were the processes of the pia mater, which descended between them; but the large veins going into the sinus were empty on both sides. The appearance called the centrum ovale was decidedly more dotted with dark points than usual, and they were more permanent, being very evident depressions or holes, the open orifices of vessels; and in many places the knife, which made the cut, left behind it the vessels drawn from their situations like bloody streaks. It was also observable, that on the surface of the cineritious matter, when the pia mater was drawn off, the depressions formed by the vessels entering from the pia mater were unusually obvious.

"The lateral as well as all the smaller ventricles were remarkably free from effusion, scarcely a drop of fluid being found in any of them. The plexus choroides, and more particularly the velum interpositum, turgid with blood; the vessels running over the corpora striata and the thalami were full and large. The other parts of the cerebrum and cerebellum were perfectly healthy.

"The spinal cord was most carefully exposed from behind, so that a complete view was obtained of the brain and it, in connexion with each other. The dura matral covering or theca was healthy. The theca was then opened along the back part; a small quantity of fluid escaped, and rather more vascularity than natural was observed. Almost half way down, and from that point to the cauda equina, were seen five or six bony plates, not above the tenth of an inch in diameter, attached to the pia mater by small peduncles, and distended like little fungi, with their tops nearly smooth. The whole pia mater was rather vascular, but nothing like inflammatory appearance or softening could be discovered. The corpora pyramidalia and olivaria, and the upper six inches of the spine, were most carefully examined at the time; the lower part of the spine was opened thirty-six hours after, (having been kept to be drawn,) at which time it had probably become in some degree softened: however, the most careful examination, by cutting into both the columns, gave no indications of disease; but at one spot, not larger than a grain of barley, some faint vascular redness was perceived.

"The lungs and heart healthy; but the heart contained very little coagulated blood, and, together with the aorta and valves, was much stained. The liver healthy, though rather large. Stomach and intestines, spleen,

pancreas, and mesenteric glands, all natural. We endeavoured to trace the ganglia of the great sympathetic in the cardiac plexus, but could discover no particular diseased appearance. The kidneys were whitish, mottled, and rather large in size. I should suspect the urine was coagulable, but there was none in the bladder on which to make the experiment.

"The uterus was rather large, and its cavity was extensive; in the left cornu was a deposit of about as much clear transparent mucus as would cover a sixpenny-piece. The ovary on the right side contained a cyst of the size of a small hazle-nut, full of a tenacious dull red substance, of just sufficient consistence to allow of being cut; the fallopian tube on the same side was quite pervious, admitting of the passage of air from the blowpipe; but it presented a remarkable appearance, having the points of the fimbriated extremities tipped with deposits of semi-transparent bone, looking like large grains of sand, of irregular and rather botryoidal form; and a deposit of the same kind was found on the outside of the broad ligament. The ovary on the opposite side was more healthy, having in it a few vesicular bodies. The fallopian tube on that side had none of the bony deposits. Attached to the ligaments of the uterus on each side was a small vesicle of the size of a pea, hanging by a peduncle, along which vessels were seen to pass."

This is the only case related by Dr. Bright which proved fatal, and in it abundant proofs of uterine irritation were manifested after death. The author, however, speaks of five other cases which he "has known to terminate unfavourably." One was a woman in the Manchester Infirmary, in the fourth month of pregnancy; two others occurred in Guy's Hospital, under the care of Dr. Currie and Dr. Marcet. They were both plethoric, and at the age when uterine irritation was likely to exist. We do not observe that any account is given of the two which remain.

Palsy from Mercury.

Paralysis is not unfrequently met with in those whose occupations expose them to the continued action of mercury. In many of these instances the metal seems to be brought in its volatalized state into contact with the lining membrane of the lungs: this, however, is not to be regarded as absolutely necessary. Three very good cases are given, in which one of the chief manifestations of the disease consisted in the peculiar jerking, irregular, and involuntary movements of the hands and arms when the individual was directed to lay hold of any thing—symptoms, it may be observed, which bear some analogy to chorea, and which are always most remarkable when the patient is at all agitated. Due regulation of the bowels, and the exhibition of tonics (sulphate of zinc) generally suffice to cure the disease.

Spasmodic Wry Neck.

This is a peculiar and interesting affection, which we have more than once seen mistaken for rheumatism of the muscles on one side the neck. It chiefly affects females, and consists in a spasmodic contraction of the muscles, often with much pain, by which the head is dragged awry, and retained in that position for days, weeks, or months. We have seen it last for nearly a year, and then subside. In a fatal case related by Dr. Bright, which occurred in a female sixty-seven years of age, the disease seemed to depend upon an organic change in the theca of the spine, consisting of some effusion under the arachnoid, with congestion of the veins; the diseased structure surrounded many of the nerves at their exit from the spine.

Neuralgia.

Under this title our author includes several nervous affections, the more active consisting of sciatica, or other painful conditions following the course of particular nerves. It is often allied to rheumatism, but at other times seems entirely distinct from it. The following is at once a brief and sufficiently characteristic illustration:—

Neuralgia cured by Subcarbonate of Iron.

"George Burney, aged 38, was admitted under my care, February 3, 1830. He had been labouring, for the last three weeks, under most severe paroxysms of acute pain shooting down his legs, apparently from the loins, but chiefly affecting the leg and the back part of the foot; these pains increased towards night. The left leg was most affected, and the pain sometimes seemed to pierce from the heel to the instep. There was no inflammation of the part. I gave a fair trial to colchicum, to the combination of calomel, antimony, and opium, and to purgatives, with no effect: cupping from the loins gave temporary relief two or three times; guaiacum and bark were nearly useless; but by means of the subcarbonate of iron, in doses of half a drachm every three

hours, the pain was quickly diminished, and he was entirely cured in ten days."

Another curious form of neuralgia is that attending *herpes zoster*. Dr. Bright has seen this prove excessively troublesome, and has used various means, with little benefit, for its relief. The carbonate of iron deserves a trial.

Neuralgic Pain succeeding to Herpes Zoster, cured by Subcarbonate of Iron.

"Shortly after, another case occurred in an old woman, who was under my care during the whole progress of the disease: the eruption died away in its usual course, but the pains which remained were intense, and the daily complaints were most distressing: opiates were of no avail, and I determined to try chalybeates; accordingly I ordered a scruple of the subcarbonate of iron three times a day, and this I increased from day to day; but the relief was so rapid, that a very few days served to remove the pain entirely."

This is the only case in which Dr. Bright has hitherto tried the iron.

The second form of neuralgia is that more formidable spasmodic, agonizing pain in certain nerves, particularly of the face, which comes on in paroxysms, to which the term *tic douloureux* has been more peculiarly applied. The author regards this as sometimes functional and sometimes organic; a circumstance which serves to account for the great difference in the results of treatment. The remedies which, upon the whole, most frequently succeed, are subcarbonate of iron and the liquor arsenicalis, with due attention to the bowels. Where these fail, some change of structure is to be apprehended, and Dr. Bright has seen at least one case corroborative of the idea of Sir Henry Halford—that tic douloureux is sometimes connected with preternatural growth of bone. The following is such a case:—

Tic Douloureux depending on a Tumor at the basis of the Skull.

"Mary Grossmith, aged 40, from Westerham, was admitted under my care into Guy's Hospital, in August 1827. She was thin, and her countenance was strongly marked by the effects of long suffering. Her most prominent symptom was extremely acute pain on the left side of her face, which was seldom completely removed, but became more severe in paroxysms. It was regarded as tic douloureux by all who had seen her, and resisted all the means employed for her relief. Within about a fortnight of her death, three molar teeth on the affected side were drawn at different times; after each operation, the pain was for a time rendered less severe, but an offensive discharge proceeded from the wounded gums, and for a few days before her death, a discharge of the same kind took place from the nose also.

Sectio Cadaveris.—The membranes about the upper part of the brain offered nothing remarkable, but the quantity of serum, both external to the brain and in the ventricles, was more considerable than is natural. The fifth ventricle was rendered very conspicuous. The brain was softer than in perfect health, and the medullary matter slightly mottled with a light purple cloud. The dura mater, immediately under the anterior part of the left middle lobe, was considerably but irregularly elevated by fungoid tumors, equal collectively to about the size of a pigeon's egg. There was a corresponding depression in the substance of the brain, which at this spot was slightly adherent and disorganized, but not completely softened, nor was the raised portion of the dura mater ulcerated or materially altered. The bone beneath the tumor was diseased, and in some parts offered no resistance to puncture. The morbid growth appeared to have extended from the sphenoidal sinuses. The mucous membrane lining all the nasal cavities on that side were similarly affected, but to a less degree. There was a soft pedunculated polypus of about the size and shape of a raisin attached between the turbinated bones. The branches of the portio dura, so far as they were laid bare in the removal of the diseased parts, exhibited no morbid appearance. (See Cat. Guy's Mus. No. 1667.)

The pleura on the right side was very generally united by old adhesions. The whole lung was rather consolidated there, with one or two immature tubercles of the size of sparrow's eggs at the apex, and miliary tubercles were distributed thinly through all parts: the left was much less diseased. The heart was remarkably small. The abdominal viscera were much wasted, and there did not appear to be any fat in the cavity. The peritoneum was free from adhesion, and there was no effusion in any of the cavities of the chest, or in the abdomen. The mucous membrane of the stomach was thin, soft, and a little discoloured; that of many parts of the small, and the first part of the large intestines, was of a grey colour, of different degrees of intensity, from an infinite number of black points. The patches of the aggregate glands were slightly elevated, probably in part owing to the attenuation of the coats of the intestines: the solitary mucous glands were beautifully distinct, raised almost like vesicles, and surrounded by an areola of grey points.

There was nothing remarkable in the other

viscera in this cavity, except that the spleen was small, and the uterus rather large and indurated."

Hemicrania is also mentioned by Dr. Bright as coming under the genus neuralgia. Arsenic is the remedy to which he chiefly trusts.

MEDICAL GAZETTE.

Saturday, June 9, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

REMUNERATION OF MEDICAL MEN.

THAT "the labourer is worthy of his hire," is a maxim which all are ready enough to admit in the abstract, but which very many seem much disposed to forget, when it is to be practically applied to the members of our profession. There is scarcely a day in which the press does not put forth some gibe in reference to the cupidity of the "doctors," when they do but seek the just reward of their services, or some illiberal insinuation about their living on the distresses of their brethren,—as if they caused the evils which they relieve. Our attention has been called to the subject on the present occasion, by perceiving that certain worthy scribes in some of the countless journals, magazines, and miscellanies, to which the first of June gave birth, have taken the opportunity to reiterate the gross misrepresentations of a contemporary, whose vituperations no one in the profession, and none but the ignorant out of it, ever think of regarding as any thing more than the ebullitions of spleen, or the yet more unworthy proceeding of invective manufactured merely as a saleable commodity.

A new aspirant for fame, in the same walk of literature, has just presented himself; and as he promises a repetition of his lucubrations on the management, or rather the mismanagement, of our public hospitals—a subject of which every line shews him to be profoundly ignorant—we shall probably take the liberty, on an early occasion, of bestowing upon him a little wholesome castigation. His great source of quarrel with the medical attendants of hospitals is, that they receive remuneration, though but indirectly, for their services; and this leads us to the only point upon which we can touch at present, but to which we earnestly call the attention of our readers—namely, that the emoluments derived by medical men from the exercise of their profession have diminished, are diminishing, and ought to be augmented.

In considering this subject, the first question we are inclined to ask is—have the members of the profession been true to themselves? Were we to endeavour to illustrate this by facts taken from the records of private practice, we could have no difficulty in shewing that the competition, generated by a limited field, and almost unlimited reapers, too often leads to sacrifices, not to say artifices, in the struggle, which, if they secure to the individual a victory on some particular occasion, do so at the expense of permanent injury on the great scale, even to the successful party. The rule by which a man consents to have his claims and pretensions measured on one occasion, will be applied to him on others; and he who once shews the public that he may be bargained with like a tradesman—that he is ready to undersell his neighbour, and will attend almost for nothing rather than lose "a job," inflicts a wound not only on his own respectability, but upon that of all his brethren. The more high-minded members of our profession may, perhaps, suppose that we are drawing upon our imagination for a subject of declamation, but the experience, or the consciousness, of

others, will convince them that this is not the case. We have known a physician offer to attend for five shillings a visit, to undersell the apothecary, and the apothecary supply his medicines at prime cost to keep out the physician,—and this where the parties who alone were benefitted by this wretched contest were perfectly able to pay, and with incomes perhaps equal to those of both the competitors for their "custom" put together. We blush in recording such transactions; it is our business, however, not to hide the blemishes of our profession, but to expose them, that their existence may be known, and the proper remedies applied. Publicity is the great antidote to any thing essentially mean; and we warn those medical pedlars who hawk their wares for what price they can obtain, that if certain recent exploits be repeated, we shall expose them, that the scorn they excite may prove a warning to others.

But turning from those displays of private rivalry and intrigue, let us see whether the proceedings of our brethren on public occasions be always of the most judicious description, in reference to the value which they place upon their services. Formerly, the medical attendants of most, if not all, of our public dispensaries received some remuneration for their labours; now, there is scarcely one instance of the kind in the metropolis: and the questions naturally arise, if the public be thereby better served, or the character of the medical profession enhanced? We have no hesitation whatever in answering both the queries in the negative. As matters now stand, the governors of such institutions have no claim upon their medical men on the score of obligation—no hold upon them on those common principles of action by which men always have been and always will be influenced. They who know that their services, which are often of the most laborious kind, are to be gratuitous, will do as little, and that little for as short a time, as they find to answer their own particular purpose. Now this purpose, for the most part, is one merely of individual improvement. A dispensary is regarded as a finishing school, at which the physician or surgeon remains for a few years, and practises on the poor that he may be able to practise on the rich. Not once in a thousand times does he remain to give the former the benefit of the knowledge he has acquired. This may seem putting the matter in a light unfavourable as regards our profession, but it is only the natural course of proceeding, supposing that we are to be influenced by the same motives as other men. As society is constituted, medical practitioners cannot afford to give their services gratuitously longer than such services prove of advantage to themselves.

That the public is not benefitted by this arrangement, it requires no argument to prove. Neither, it is quite clear, is the respectability of the profession likely to be increased by the tacit admission that their services may be had without requital, or by the exhibition so often afforded of eleemosynary institutions, manifestly got up to serve the projector, under the mask of charity. So easy has the formation of these things been, since it became the custom to give all medical attendance gratuitously, that almost every one who is not overburdened with business forthwith sets up a dispensary, infirmary, or asylum; and accordingly we now have something of the kind in nearly every street. If the dispensaries in London were reduced to one half of their present number, and the income at present wasted on house-rent and other appliances, which each requires, was appropriated to bestowing upon the medical men remuneration proportioned to their labours, the poor would

be more carefully attended, and by more experienced men; while the general practitioner would be provided with many a patient, who at present meanly prefers going in his shabbiest coat to a dispensary rather than pay the "Doctor," though perfectly well able to do so: and this we protest from personal knowledge to be an evil very frequently resulting from the present system.

But the fact is, we are not true to ourselves. Could any thing, for example, be more mistaken than the conduct of one of the candidates for the Marylebone Infirmary, who, on the recent vacancy, offered to do that gratuitously, for doing which Dr. Hooper received a good round sum per annum? What! can the largest and most opulent parish in London not afford to pay for the attendance—the daily, the laborious, the responsible attendance upon the poor? It is most unreasonable and unjust to expect gratuitous services,—most unhandsome to allow of it, and derogatory to the character of the profession that it should for a moment have been contemplated. It is all very fine to talk about humanity, benevolence, and so forth: there is neither humanity nor benevolence in such a case. When the baker and the butcher gratuitously provide meat and bread for the living, and the undertaker and sexton find a coffin and a grave for the dead without fee or reward, then, and not till then, will it be reasonable to ask the medical man to attend parish patients without a salary. But then we are constantly told, when we use this argument to men out of the profession, that "there is a mighty difference between the tradesman parting with goods, which he purchases, and the doctor with advice, which costs him nothing." How little reflection is manifested in this remark! Time and knowledge are property—commodities as expensive in purchasing as meat and bread; nor could the monstrous mistake that they "cost us nothing" ever have been made, even by the most thoughtless, had not the competition resulting from an overstocked profession led many to give their services gratuitously rather than not have them accepted, in the hope that they may indirectly obtain the reward which is denied them as the immediate price of their property; just as where competition of any kind exists, the parties undersell each other often to their own ruin, but always in expectation of ultimately obtaining a return for their outlay. The tax of gratuitous exertion levied on the medical profession is so great, and has endured so long, that, like other familiar things, people cease to be sensible of it; and it is not against its use, but against its abuse, that we would protest. Far be it from us to advocate any doctrine which would tend to deprive the poor man of our assistance in the hour of sickness; or to desire that, as a body, we should yield that high character for benevolence, to which, surely, men are well entitled, who, as a general rule, from the earliest to the latest period of their professional career, are more frequently employed in charitable labours than any other class of society; and who have no other more selfish object than the desire of knowledge, and the rational prospect of making this an honourable passport to worldly advancement. But in these latter days—as man may no longer pluck and eat, as our first parents did—there is a point at which benevolence itself should stop, and that point is transgressed whenever a medical man gives his services gratuitously to public bodies who can pay, and do pay, all but him; or when he consents to receive less than the fair remuneration for his services from those whom avarice, and not necessity, renders thrifty: if the practitioner who does so be a poor man, he injures himself—if a rich one, he is guilty of an act of injustice to his

less fortunate brethren. And yet we appeal to all acquainted with the subject whether this be not very frequently done!

CHOLERA.

We this day lay before our readers detailed accounts of some cases in which saline solutions have been injected into the veins of patients labouring under cholera: of these, two occurred in London, and the others in Scotland. Several other instances in which this treatment was adopted within the last few days, have also come to our knowledge, particularly one in Marylebone, and another in Hull, in all of which the patients ultimately sunk under the disease. The proportion of those who have been saved by the saline injection is as yet extremely limited, being less than one-third; but it is to be considered that it has only been adopted in the period of extreme collapse, and under circumstances of a desperate nature. All that we can say for it at present is, that it gives the patient one throw more of the dice for existence; and that when the proper moment has arrived, that chance, however small, ought not to be denied him. But great care should be taken that it be not precipitately adopted, as it carries with it new sources of danger, and produces phenomena of which the patients may die, even if the remedy should enable them to struggle through the immediate effects of the disease. As to the quantity of water used, too, it must not be forgotten that in many cases the collapse takes place without any evacuations; and, consequently, that large quantities of fluid cannot be required to supply a waste which has *not* occurred.

We understand that mercurial inhalation, in the Indian fashion, was lately tried at Hull, but we have not heard the result.

It is a mistake to suppose that cholera has entirely left London: in addition to the three cases above alluded to, as treated by saline transfusion, we have heard of six or eight more within the last week—some of them severe.

OF WHAT DID M. CUVIER DIE?

(*From a Correspondent.*)

With the exception of Magendie, who ventured a broad guess, none of the French physicians even pretend to assign the cause of M. Cuvier's death. Yet their patient did not perish for want of active and energetic treatment. We have before us the exact and very interesting report of M. Rousseau, who was the late Baron's assistant at the Jardin-des-Plantes, and was present *en famille* during the whole time of his patron's illness, being also an eye-witness of his death; and this report appears to us to contain some particulars specially worth attending to: we shall follow M. R. briefly in his relation of the principal facts belonging to each day.

On Monday, M. Cuvier had slight diarrhœa, with disturbance of the bowels, for which he took a lavement, with some drops of laudanum in it. On Tuesday he felt quite well, and gave his accustomed lecture at the College of France with even more than his usual energy; so much so, indeed, that he was *covered with perspiration* at its conclusion. The day was rather cold, and M. Cuvier walked home, contrary to his ordinary custom. He dined as well as usual, and in the evening attended a *soirée* of the Professors, at the Museum, where he talked a good deal. It was on the next morning, Wednesday, that he complained of the stiffness and difficulty of moving his right upper extremity; yet he attended the Council of State, and on his return had an appetite for dinner; but though he could eat his soup well enough, he was surprised to find that it was almost impossible for him to swallow any thing more solid. That night leeches were applied to the anus. On Thursday the right arm was perfectly paralyzed; deglutition was more difficult than ever; but he could walk about very well. The pulse was normal, beating from 80 to 85 in the minute. One

of the medical attendants, however, thought fit, in the course of the day, to bleed the patient largely: *two pounds of healthy looking blood* were drawn from the left arm. A mustard foot-bath was used in the evening, and a large blister applied to the back of the neck. The night was spent very restlessly; and about three A.M. the pulse seemed so hard and full that the attendants were induced to repeat the bleeding, which they now did from the right arm. After this the patient's muscular powers sunk rapidly, though his nervous sensibility and intelligence were not at all impaired. On Friday morning he was ordered a little tartar emetic, which however did not act upwards. His mouth was then observed to be filled with a copious flow of mucous saliva; and this, together with the difficulty which he felt in swallowing the emetic solution, induced the patient himself to remark that he was like a person labouring under *hydrophobia*. In the afternoon, M. Dupuytren, in order to excite the action of the œsophagus and pharynx, threw into the stomach four-and-twenty grains of ipecacuanha, but no vomiting ensued. In three hours after, double the quantity was employed, but without the occurrence even of nausea. At seven in the evening, a strong lavement of salt and water (saturated) was given: this produced *a super-purgation*. Same night, two or three large "English vesicatories" were applied along the course of the cervical plexuses, and the patient was in a most restless condition. On Saturday morning it appeared that the left leg was beginning to be paralyzed. At the patient's earnest request, some *bouillon* was conveyed into his stomach: he was also removed from his bedchamber into his spacious saloon. The blisters did no good; they did not even irritate the skin. In the course of the day he had given him some iced raspberry-vinegar, and enjoyed comparative repose; but the night brought on much severe suffering. All power of motion and swallowing was now extinct. Twenty leeches were applied to the region of the mastoid processes. "When I saw him on Sunday morning," says Dr. Rousseau, "it seemed as if he had grown on a sudden ten years older; his voice also was wonderfully changed." That day (Sunday, 13th—the day of his death) the patient began to lose all hope. When any new measure was proposed to him, he shook his head with a desponding assent. He was *cupped on the loins* about noon; and again, about eight in the evening, he was persuaded to suffer himself to be *cupped below the scapulæ*. This operation fatigued him greatly. At a quarter to nine he asked the hour, and complained that his faculties were leaving him; "and at a quarter to ten," says Dr. R., "I observed three or four slight motions of the head and a feeble expiration, which I found had deprived the world of a man of vast knowledge and the most extraordinary genius. He died in his arm-chair, sitting erect, with his head neither inclined one way nor the other. His figure was majestic, in the attitude of deep contemplation. So like the life did he seem, that his family would not believe the melancholy fact; but the illustrious patient was no more."

M. Rousseau then goes on to give an account of the post-mortem appearances,—in which there is nothing differs materially from M. Berard's report, of which we gave an abstract in our penultimate number. But the whole of the information that has reached us comes only to this—that nothing could be discovered by the French pathologists to satisfy them as to the cause of M. Cuvier's death. There was *apparently* no organic lesion to which the fatal issue could be traced, unless, perhaps, those curious prominences of the cervical and dorsal vertebræ, which attracted some small degree of attention; but these it was thought had existed so long, and had so little of a morbid character about them, that nobody would venture to assign to them the production of a catastrophe so rapid.

Now the case is, beyond a doubt, a most curiously complicated one, and affords considerable room for conjecture. But there are a few points in it which we think may serve to clear away at least a part of the difficulty. That there was *some* organic lesion present, if but pathology could find it out, is what we will take leave to assume, and what none of the medical attendants seemed willing to deny. Then, from M. Rousseau's shewing, it is more than probable that, on the Tuesday, M. Cuvier *took cold*. We wish to say as little as possible regarding the extraordinary treatment to which the patient was

subjected; but there is one peculiarity more in this matter, which we cannot avoid noticing: M. Cuvier was in his sixty-third year—in (according to "wise saws") his *grand climacteric.*

We are not inclined to put *much* faith in the climacterical arrangement of human life; nor can we understand—any more than Sir Thomas Browne, who condemns it as a "vulgar error,"—why the ninth septennary period should be considered as so pre-eminently fraught with danger; but the experience of antiquity will have it there is something in the matter that deserves not to be entirely overlooked. Nor can we refuse to lend our attention, moreover, to a learned authority of our own time.

To Sir Henry Halford the credit is due of having re-called the attention of the profession to the effects of climacterical changes: it was he who first well described that sudden and general alteration of health which occasionally occurs at a certain period of life—any time, as he remarks, between fifty and seventy-five. Now when it occurred to us at what particular age (63) M. Cuvier died, and at the same time what difficulty there was in attempting to assign any very plausible hypothesis to account for his death, we could not help looking into the learned President's pages *, for a confirmation of the view which we felt inclined to take of the subject: and certainly there are some points in the essay "on Climacteric Disease," which deserve more than a passing degree of notice, with reference to M. Cuvier's case; a few of them which we deem most pertinent we shall briefly lay before the reader, and leave the rest to his own leisure and reflection.

"But it is seldom that we have an opportunity of observing this malady in its simple form; and never, I believe, but in a patient whose previous life has been entirely healthy. We find it generally complicated with other complaints, assuming their character, and accompanying them in their course; and perhaps this may be the reason why we do not find the climacteric disease described in books of nosology as a distinct and particular distemper. It blends itself with the effects of any fixed organic mischief in the constitution; takes on the appearance of any periodical irritation to which a patient may have been subject, or adopts the features of a casual disease. When it is associated with organic mischief, it is difficult to distinguish the climacteric complaint from that train of symptoms which commonly supervenes, sooner or later, on diseased structure; but its presence ought to be suspected if the complaints are all unusually exasperated—if a fatal result be threatened earlier than is usual in the common course of things; and, above all other indications, if that character be impressed on the countenance which peculiarly distinguishes this disorder."

Again, Sir Henry observes, "of the various immediate causes to which this malady may owe its commencement, there is none more frequent than *a common cold.* When the body is predisposed to this change, any occasion of feverish excitement, and a privation of rest at the same time, will readily induce it." * * * * "A fall, which did not appear of consequence at the moment, and which would not have been so at any other time, has sometimes jarred the frame into this disordered action."

And with reference to the suggestion which we ventured to offer above, as to the *remarkable* treatment employed in M. Cuvier's case, we shall make one short extract more, and have done. "Physicians will not expect me to propose a cure for this malady. In fact, I have nothing to offer with confidence in that view beyond a caution that the symptoms be not met by *too active* a treatment. Whatever would weaken the general system must be detrimental; and it seems, in all cases of this kind, more prudent to direct local than general evacuations for the relief of occasional congestions in the blood-vessels."

CUVERIANA.

Measurement of the Head.

In a former number we gave the weight of M. Cuvier's brain; we can now state, from M. Rousseau's notes, the dimensions of his cranium. The head having been shaved, the measures were as follows:—

Greatest circumference, 22 inches 4 lines.

* Essays and Orations, by Sir H. Halford, Bart.

From the occiput fossa to the root of the nose, over the vertex, 13 inches 4 lines.
From one meatus auditorius ext. to the other, over the vertex, 15 inches.

We are not informed of the measurement of the facial angle; but it was much developed. Moulds of the head and entire bust have been taken.

It may be worth mentioning, that M. Rousseau's note of the weight of the brain makes it an ounce heavier than it is stated to be by M. Berard. According to M. R. the weight of the cerebellum was 5 oz. 1 dr.; of the whole brain, including the cerebellum, 3 lbs. 14 oz. 4½ dr.

M. Rousseau adds, that the parietes of the cranium were generally moderate in their thickness; that, in some places, they were even delicately thin; and that, on the whole, he never remembers to have seen a skull more symmetrical.

Cuvier's Income.

The whole amount of the late Baron's income did not exceed 1680*l.* The following may be considered as a correct list of those appointments from which he derived any emolument. He received as

	Frs.
Member of the University Council,	12,000
Counsellor of State.....	10,000
Professor to the College of France,	6,000
——————— Jardin du Roi.....	6,000
Member of the Acad. des Sciences,	1,000
Perpetual Secretary to ditto	6,000
Member of the Acad. Française ...	1,000

In all, 42,000 frs. per annum.

JEREMY BENTHAM.

This very eminent man died on Wednesday last at his house in Queen-Square, Westminster: he was in the 85th year of his age. We cannot do less than record his demise—as we do with unfeigned respect—for he was always a well-wisher, and an able advocate, of our profession, and testified his regard for its pursuits by the bequest of his body for anatomical purposes. Dr. Southwood Smith is the favoured individual whom he appointed to carry that object into effect.

SIR JAMES MACKINTOSH.

Perhaps we ought not to close our obituary without also noticing the death of this distinguished man, which took place early in the last week. About fifty years ago he took a degree in medicine at Edinburgh; and just at the close of his studies was offered an appointment at the court of Russia, which, however, he declined, having never, we believe, devoted much time or attention to the practical part of his profession. The appointment was then given to Sir A. Crichton, who remained many years at St. Petersburgh. Soon after this, Sir James Mackintosh abandoned medicine, became a literary man, and studied law. It would be foreign to the objects of this journal to follow his career any farther, particularly as we are not aware that in his parliamentary, or any other capacity, he ever evinced any zeal for the interests of medical science. During his later years he had become hypochondriacal, and on several occasions on which we met with him he always took the opportunity of asking us to feel his pulse.

MEDICO-CHIRURGICAL SOCIETY.

Tuesday, May 8, 1832.

Mr. Lawrence in the Chair.

Malignant Diseases.

The second part of a paper by Mr. Travers was read by the secretary (Mr. Partridge); it treated of the subject of malignant diseases, including all those of a cancerous or schirrous nature affecting either the exterior or interior of the body. Another portion of Mr. Travers's communication still remains for perusal on a future evening, but from the very elaborate nature of the paper we think it better not to attempt any account of it till we have the advantage of seeing it in print.

After some remarks from Mr. Langstaff, on a passage in the paper in which hydatids were mentioned, and which he (Mr. L.) would prefer to denominate serous cysts,

The President, with reference to Mr. Travers's observations on cancer of the uterus, expressed a wish that his friend the secretary (Dr. Lee), would state the result of his experience as to the nature of that complaint, and its most usual initiatory symptoms.

Dr. Lee was of opinion that the complaint was by no means of an inflammatory nature in its commencement; it began in the part

from some disposition in the texture, but inflammation was not present. It was certain that local bleeding and antiphlogistics produced not the least effect upon it. The most usual period for the occurrence of the disease was about the age of from forty-five to fifty. He had seen it in a young woman, indeed, of eighteen, whose uterus was completely filled with a malignant fungus. That very day he had opened the body of a woman who had died of the disease. There was nothing, he said, very peculiar in the case; the deceased was about fifty years of age, and the morbid structure in that phagedenic ulcerated condition which is usually met with; but the case he thought worth noticing for another circumstance;—the deceased had two swelled legs from *phlegmasia dolens*, and upon examination, it was found that the external, middle, and internal iliac veins on both sides were impervious, from plugs of effused lymph with which they were filled for a considerable portion of their length; and this was the sixth case in which he had observed the same phenomena.

Sir Charles Bell desired to know how long such a condition might have existed during life in the case just mentioned by Dr. Lee; and how the circulation in the parts was performed.

Dr. Lee did not consider the peculiarity in question as incompatible with even a protracted continuance of life. The circulation was maintained through the collateral branches.

A paper was then read from the pen of Mr. Howship—

On the Phenomena and Appearances induced by Partial Obstruction in the Cerebral Circulation.

Two interesting cases were detailed by the author, the first demonstrating the consequences of inflammation of one or more of the large superficial veins of the brain. These consisted chiefly in an injected state of all the capillaries, which had to pour their blood into the obstructed vessel, giving to the brain in the vicinity a peculiar variegated appearance when cut across, as if studded with numerous dark points. An approach to the same condition is represented in one of Dr. Hooper's plates of the Brain, and the appearance is still more closely seen in an engraving in Dr. Bright's work.

The other case was that of a patient who had laboured under bronchocele for above fifty years. The enlargement of the gland was very considerable, and on the right side was seen the trunk of the common carotid very much enlarged, lying close under the integuments, and pulsating with amazing force, while the carotid on the left side was quite quiet. Mr. Howship conjectured that the great impediment to the return of blood through the jugular vein, which was stretched and flattened over the surface of the tumor, had caused the necessity for this increased excretion of the carotid. At first sight it would have seemed, from the carotid running over the projecting surface of the tumor, that it must have been elongated, to accomplish the apparent circuit; it was found, however, on measuring them after death, that the vessel on both sides was the same, but that the manner in which the innominata was formed admitted of the carotid taking the above course, without any absolute elongation as compared to the other.

COMPLIMENT TO MR. WHITMORE.

We omitted to state in our last, that Mr. Whitmore has also been awarded a donation from the Magistrates of Middlesex, for his attention to the cholera patients in the prison of Cold-bath Fields.

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

May 1832.	Thermometer.		Barometer.	
Thursday . 31	from 38 to 65		29·57 to 29·45	
June				
Friday. . . 1	43	59	29·56	29·61
Saturday . 2	35	68	29·66	29.84
Sunday . . 3	49	69	29 76	29.54
Monday . . 4	49	63	29 46	Stat.
Tuesday . 5	48	63	29·46	29·53
Wednesday 6	45	61	29·46	Stat.

Wind variable, N.E. and S.W. prevailing.

Except the 2d instant, generally cloudy, with frequent showers.

Rain fallen, ·825 of an inch.

Charles Henry Adams.

BOOKS RECEIVED FOR REVIEW.

A. Cornelii Celsi de Re Medica Libri Octo, ex recensione Leon. Targæ; accedunt J. Rhodii Dissertatio de Celsi Vita, Schillingii Quæstio de Celsi Ætate, L. Targæ Prefatio et Index Libb. MSS. Editorumque, Excerpta de Balneis, de Ponderibus et Mensuris Romanis monitum breve, cum Conspectu Capitum. Præfixis Characterum, Balnearum, Instrumentorumque Tabulis. Editio Secunda, accuratissime emendata, opera et studio Georgii Frederici Collier, ex Aula Magd. Oxon., e Colleg. Reg. Medic. Lond., et ex Acad. Lugd. Bat. M.D. 1 vol. small 8vo.

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*** The above was received some time ago, but had been mislaid.

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CROONIAN LECTURES,

Delivered at the Royal College of Physicians,

By Dr. Roupell,

May 1832.

Lecture II.

On General Pathology.

The branch of pathology which regards the causes of disease—the sources from which alterations of structure originate—is even at the present time a subject involved in much doubt, and has consequently been variously accounted for, and received very opposite modes of explanation.

The inquiry into the origin of disorders is highly interesting, whether we regard it with reference to the prevention of diseases, or consider it as connected with the physiology of the system; or whether we view it as the only certain ground-work for directing the treatment of the numerous complaints to which our frame is subject.

In approaching this investigation, I may observe, that the only view with which we can practically regard the states of health and disease, is to consider the one as the due and proper performance of the functions of each individual organ, and the other as the departure from, or the derangement of, such properties, whether of structure or of office.

Now, if for the integrity of action a certain condition is requisite, any and every alteration must imply some difference in the physical relation of the component particles of the laws which regulate their movements, or control their power.

Such considerations solely can guide us in the successful cultivation of pathology, or can lead to the knowledge of the proper administration of remedies—the great object of our immediate pursuit.

The first step in any investigation of the disordered condition of a system, must be the inquiry into the mode by which the *healthy* functions are performed, and the requisites necessary for the due exercise of the various organs. Having become acquainted with these preliminaries, we may then ascertain and appreciate the different stages of morbid alteration, and perceive how they may be excited or maintained. In approaching these considerations, I may add, the idea of "materialism" need not enter; I merely am considering the physical condition of the organs which are allowed by all parties, if deranged, to influence function.

Human ingenuity has been amply exercised with regard to the origin of disease, and the promulgation of theories and opinions has been commensurate with the intricacy and obscurity of the subject. All these I shall not attempt to enumerate; some I shall notice, but, in passing over others, would pay a tribute to the great talent displayed and invention exercised; feeling, as I do, that the expression of opinion, or even the hazarding of suggestions, on matters but little understood, ought rather to be encouraged than condemned. Conjecture provokes discussion and inquiry, and, by leaving to experience to assign to each hypothesis its relative value, conduces to the adoption of that which is best calculated to account rationally for the effects we witness in the varieties of mental and bodily derangement.

I shall mention, however, some few of the reigning theories, and shall then venture to express the opinion which I have formed on the mode by which many of the disorders of our frame arise; regretting that the proneness to adopt certain notions exclusively—the attempt at rendering all the phenomena of disease conformable to some one favourite dogma—has operated to the detriment of, and continues injuriously to influence, our rational investigation of disorders. The theory of disease which has the most numerous advocates at the present day, is that which assigns disordered effects to the pro-

perty of the tissues which occasions the action of the minuter vessels, by which the current of the blood is increased or moderated: to "*irritability*," either local or general, is attributed the beginning, the cause, and all the results of disordered actions.

Health and disease, according to Mons. Broussais, are the varied effects of but one principle; and that principle is "irritation." When health alters, it is always because the *external stimulants* occasion the accumulation of irritation in some one part, or because they are entirely absent from the system. Irritation is, then, the first cause or point of departure of all disorders which take place in the system. Inflammation, hæmorrhage, nervous disorders, every species of morbid alteration, is but, according to Mons. Broussais, the effect of this one excitement. Some physical change, no doubt, exists in all disorders; but to refer them *all* to one source, will appear sufficiently *controverted* when we extend our inquiry into the processes which ensue, and the modes by which disordered changes take place, as well as into the natural result of the changes occasioned by age in many tissues; as we see the conversion of cartilage into bone—the induration of nervous structure—the obliteration of minute vessels—and other alteration in parts, as they become unfit for the exercise of their respective functions.

The great *variety of products* observed in different individuals—the *consequence* of simply excited action—prove that a simple *increase* or diminution of one single principle cannot be sufficient to account for all the changes which we are constantly in the habit of noticing, or satisfy us that many and peculiar causes are not operating and combining to produce these effects.

The idea of referring all disease to one source, and of deducing one consequent mode of treatment, is a view far too simple of the processes of our system, and not consonant to facts observed by examinations when death has occurred, or in accordance with the symptoms presented during life.

The undoubtedly specific nature of many affections—the different modes of invasion of some—the varied period of continuance of others, many of which observe a definite cycle—the peculiar manner of propagation in some cases, and the diversity of symptoms in all—must effectually militate against the possibility of one similar and common origin of all maladies. Another great topic of discussion is the possibility or impossibility of a general, a simultaneous effect, being produced throughout the system — a morbid impression acting universally. All diseases, by some physiologists, are looked to as arising from a primary *local* affection; a disordered change in one organ is by them considered as the *necessary* precursor of the constitutional symptoms; which are thus attributed to *sympathy* with the original disease.

Sympathetic affections doubtless take place, and few pathological states exist without giving rise to disturbance in distant parts; but too much is ascribed to the nervous agency and to sympathetic influence; real derangement is often overlooked, or confounded with nervous feelings; and, I conceive, in the simultaneous impressions of various systems, in many instances, we shall be able to prove a *general* cause operating to account for the phenomena of the complicated symptoms.

By some pathologists the entire operations of health and disease are referred to the solids—to the nervous system—as the power solely acted on, and controlling the whole organization. Others have attributed to the fluids alone this agency. In systems so immediately united, so connected as the solids and fluids, to refer to either exclusively the proclivity to morbid change, must, as it appears to me, be incorrect. Should the nervous system be the *recipient* of impressions, it owes its fitness for this office to the action of the fluids circulating in proper quantity, and having duly undergone their appropriate arterial purification.

The living body may be regarded as possessing three principal systems or modes of action, each of which serves distinct purposes, either as connecting it with surrounding objects, or necessary to its internal operations. These are the nervous, the muscular, and the sanguiferous systems; the one contributing to sensation, the other to voluntary motion, the third to the physical or chemical changes of parts themselves.

The properties of the solids, by which the processes of disordered changes of structure, as well as the healthy actions in all the tissues, are performed, are "*sensibility*," "*contractability*," and (may I add) the "property of *permeability*;" by which the nutritive fluids conducted to the various organs in their progress yield to each tissue its appropriate nourishment. In the consideration of *diseases*, the *fluids* have more recently been denied any share in occasioning these effects; an opinion with which I cannot entirely concur, and shall endeavour to shew that these may be altered either in their constituent parts or chemical composition. Whatever is capable of producing any obvious disturbance, either in the natural functions of the body or its component parts, must be considered a "*cause*" of disease.

In strict reasoning, we may perhaps be required to refer all causes which operate to the injury of our frame, to external agencies; but more commonly they are divided into external and internal; the first class, the external, operating without any apparent previous change, the others arise from the altered powers of the system itself, or the action of its

component parts on each other; but most frequently both internal and external causes combine to give rise to diseases. The external agencies which operate in their production, are the atmosphere, with all its varieties of temperature, dryness, and moisture; specific contagions or noxious exhalations; improper diet; and mechanical violence.

The internal causes are peculiarities of constitution; hereditary predisposition; the effects of education; and different employments: all those circumstances which produce debility; excesses of all kinds; undue privation of sleep; and the superabundance or deficiency of the different secretions. Of the external causes of disease, the actual source of some can be demonstrated, in a peculiar specific virus, which may serve as a germ to excite and maintain the propagation of the peculiar and distinct affection. Thus small-pox, syphilis, and some other diseases, originate, may be kept up, and disseminated, by their own original principle.

The action of these causes varies in its site and extent; some exert particular affections in certain remote parts, as the poison of typhus on the brain; another virus, that of dysentery, the mucous membrane of the bowels; others produce an entirely local action, or only affect the constitution after a considerable period, as syphilis: many others produce active constitutional symptoms as well as local irritation, as small-pox and measles.

The inquiry as to the origin of these poisons and their primitive source, has led to little definite result: it has been denied that new diseases can originate, although no reasonable doubt can be entertained, that a combination of circumstances might produce a modification of matter similar to that which originally occasioned the disorders with which we are familiar; and we feel no difficulty in conceiving such an origin of new diseases: the history, indeed, of many of the most marked afflictions of our race, are not described by more ancient writers, and would thus lead us to believe that some of the most familiar disorders are of comparatively modern date; and the progress in the modification of others would also lead us to infer, that those longest known may eventually disappear from our globe. We see some disorders, however, when thus excited, continue unaltered in their nature or their characters; variola accurately conforming to its early description, while syphilis, lepra, and some others, appear, by descent through successive generations, to become less formidable in their nature, and more tractable in their treatment.

The atmosphere is the most fruitful source of deleterious impressions on our system, not only from the noxious impregnations which it may contain from specific poisons, and which it may convey; but without such contamination, we find in the simple impression of the surrounding medium, an agent constantly in operation, and in this climate, from modes of life, or from the capricious sway of fashion, an evil of serious magnitude, occasioning more destructive ravages than the disorders more popularly dreaded; and is more baneful than the most pernicious miasms, against which we take ample precautions, and which we are ever ready to combat and to fear.

Dr. Currie, of Liverpool, gives us a good account of a paroxysm of fever excited by the simple effect of cold, the result of the mere impression on the skin; the cold fit, the hot stage now distinctly and accurately marked, and terminated by perspiration. The influence of the sudden abstraction of heat was perceived in the diminished temperature, which continued for some time; the power of generating heat then returned, but increased, and, until the exhalant power of the minuter vessels was restored, the fever remained. The effect of the constriction of the minuter vessels by cold is not in a slight degree unpleasant, and occasions a succeeding reaction highly consistent with enjoyment; but if the degree of cold be increased, the circulation of the extreme branches of vessels is diminished, and finally the heart's action is enfeebled, the nervous system is depressed, the senses are blunted, and torpor and insensibility result. The effects of cold vary much in different individuals, and in the same individual at different periods of life. The degree of temperature of the atmosphere being lower than that of our system, caloric is consequently abstracted; this is naturally furnished, and we bear a slight reduction without inconvenience, the loss of heat being readily supplied, and takes place to a great extent, provided the transitions be slow; but if suddenly affected—should we at once be exposed to a considerably diminished temperature—we feel the loss which we are not able for a time to supply, as on the first approach of winter we feel unpleasantly a degree of cold, to which we become indifferent in proportion as our system acquires the power of generating heat. Now this faculty varies, not only at different periods of life, but of the day, and in the various classes of animals; many warm-blooded animals at birth not being able to maintain a temperature above that of the surrounding atmosphere, and requiring the heat of the parent, and, in birds, of the nest, to support life: this property is, however, in a few days acquired, when they become able *themselves* to maintain their proper warmth. Some animals are very deficient in the property of generating heat, and, yielding to the impression of cold, become torpid, during winter. In man, the property of generating heat is diminished

with age, and is less active during sleep, so that the influence of the agency of cold at night may account, in some degree, for the pernicious effect of exposure, often attributed to the injurious impression of nocturnal miasmata. In the infant, at birth, the power of supporting its proper temperature is low, (thus approximating the human frame to that of some inferior animals) and it requires the cherishing of the mother to maintain its due degree; a sufficient argument against the idea of *hardening* the frame by early exposure to cold.

The degree of dryness or moisture of the atmosphere much influences the radiation or abstraction of heat from the skin: when the air is *cold and moist*, its effects are particularly striking, a much less degree of cold producing a considerable difference in the power of resistance to its effects, and impressing us with particularly chilling sensations: under this *combination* of *cold* and *moisture*, the *faculty* of supplying the deficiency of heat is diminished, and operates with the actual loss, which is of course also considerable, from such an excellent conducting medium. Such a state of atmosphere is alone sufficient to occasion the excitement of fever. The effect which cold may thus have in suppressing secretion, is certainly one cause by which the lungs may be loaded, and the asthmatic coughs be produced, or the bowels disturbed: the primary effect of cold may, however, act upon the lungs, and excite inflammation of its membranes. A dry and cold atmosphere, provided no active current of wind carry off the exhalations, is less injurious than cold with moisture; but the very dry state of the air is capable, from the great rapidity of evaporation, of producing exhaustion, and is one of the most influential, in inducing the effects perceived in ascending mountains, and produces the *withering effect* noticed on plants during the prevalence of the east wind, so obvious on vegetation, and to which we are so highly sensitive.

The action of heat on the frame is various: by heat the expansion of fluids is occasioned, the solid parts relaxed, and the inclination to exertion diminished; while the secretion from the skin is much increased, respiration quickened, and the power of generating heat is changed—is essentially diminished, though the standard of the natural temperature is higher in warm climates than the temperate zone.

The circumstances connected with the production of caloric, and which regulate it, entitle this to belong to the order of secretions, and point out a connexion between this process and respiration, the oxygen consumed being proportioned in animals to the power of generating heat; and we perceive in the greater proportion of oxygen contained in the atmosphere during winter, a provision as well for supplying this principle, and rendering respiration less necessary when the lungs may be oppressed and their functions impeded. On this subject, however, we have yet much to learn, but may presume between the adjusting of this power, amongst other causes, the affections common to those periods of the year, those diseases prevalent in seasons when these changes take place.

With the variation of temperature, as connected with the atmosphere, may be considered light and electricity.

Light is highly essential to the well-being of plants: during light their perspiration is much increased, and to this cause, as is familiarly known, their colour is in a great degree attributable, the agency of the sun's rays enabling their leaves to decompose carbonic acid. Not deprived, or not for any length of time, of this influence, *we* do not notice its effect; but some experiments of Dr. Edwards show considerable effect on animals, if secluded from light. Having placed some frog's spawn in two vases, under similar circumstances, excepting that the one was completely darkened, he observed that the embryo, when thus situated, was slower in going through its proper stages of development; that the animals when hatched deprived of light, were not so quickly transformed as those in their natural relation to this agent; that having undergone some of their changes, the further progress of their mutations was suspended, although nutrition did not appear to be impaired. He observed, indeed, that the tadpole thus situated grew in weight, acquiring indeed double or treble its primitive size, yet did not, as usual, (when it had arrived at the age when the metamorphose into the conditions of reptiles usually takes place,) undergo that transition; thus rendering light *one* of the active agents, with the consequent excited movements of animals, an important power in directing the form and figure of animals. How far light may operate in the production of scrofula and some other diseases, we do not know, but observe the effect its absence produces on plants—a white and sickly aspect—and occasions a somewhat similar discolouration, or rather absence of colour, and proclivity to disease, in man.

It may be difficult to separate light entirely from the other properties of the sun's beams—its chemical rays, and production of caloric, by the operation of which combined, we see such obvious effects occasioned in the vegetable kingdom. A number of chemical changes are induced, the result of which, as on the performance of all other chemical changes, is an abundant extrication of electricity. To its disengagement from the vegetable kingdom in the growth and nutrition of plants, with other such processes, the atmosphere (according to Mons. Pouillet) derives a great portion of its electricity. Of the part which this agent

performs in the production or direction of the phenomena of life, we are not yet assured. By some *an explanation* is detected in this power of all the vital movements—the actual long-sought *spring* of animation; while others, on the other hand, seem unwilling to allow to it any agency at all. That a power obviously pervading all matter—palpably extricated in every chemical change of combination or decomposition—that such a power should not be made subservient to, or much influence the operations in our bodies, either as cause or effect, would indeed be matter of surprise, when we consider that the simple approach of a part of the body to an excited electric, or to an electrified conductor, will produce sensation—when we see that electricity, issuing from an electrified body, produces taste, an acid sensation—when we see the power of electricity capable of restoring in a considerable degree the functions of parts which have been deranged by the division of its nerves, as shewn by Dr. Wilson Philip and others, with regard to digestion, we must admit its power. When we perceive that the simple passing the hand on glass sensibly affects the gold leaf electrometer, and the least contact of two persons isolated is sufficient to extricate a sufficiency of electricity to occasion the deviation of Coulomb's electrometer, we cannot doubt the perpetual presence of this fluid, and its important influence; be its production the *effect*, or its agency the *cause*, of any of the processes of our system.

Of electrical influence as a cause of disease, we may notice the different states of atmosphere with regard to this fluid, its accumulation in some situations—in elevated spots and occasionally in valleys—when we observe that the atmosphere is sometimes positive, at others negative — that these changes are sudden and repeated—that vapour has a great capacity for electricity—that it varies with the rising and the setting of the sun—that it is weakest in the evening, and during the prevalence of easterly winds, as shewn by Mons. Cavallo,—at all of which times we acknowledge an atmospheric influence, which is sufficiently marked in some persons in more disturbed electrical states, as during the prevalence of stormy seasons, and in those occasionally ascending mountains. This was amusingly illustrated, as I was credibly informed by one of a party, during a journey across the Andes, where the state of the atmosphere was such as to produce sensible effects on the body, inconveniencing and alarming some ladies who were of the number, by its effect on the hair. Mons. Decandolle observed the influence of this agent in the growth of the young shoot of a vine, which, during a thunder-storm lasting an hour and a half, grew more than an inch—an inch and a quarter. To the influence of all these causes combined, we may attribute the effects of climate, and explain many of the varieties in the organization, as impeded growth, and many of the peculiarities of the passions and dispositions of the races of man, as well as in the diseases to which he is incident. These are the principal external agencies, which, with diet, influence the body, and prove active excitants to disease. Diet has received an attention proportioned to its importance; but we are yet to look for assistance from chemists for the full elucidation of its real influence and mode of operation.

We lay down rules of regimen founded on observation, and the natural repugnance exhibited during illness to certain kinds of aliment, which have been proved to be hurtful; yet the principle by which this injurious effect is actually produced, is not satisfactorily accounted for by the prevailing doctrines of disease. This subject, in the hands of Dr. Prout, has already received much valuable arrangement; but the influence of these causes, and their power of producing certain particular diseases, as their chemical characters may vary—as azote may abound or be deficient—or as they may be too much impregnated with acid, or alkaline matters, is yet to be elucidated. To the *chemical combinations* to which they may give rise, or to the influence they may produce, in the minuter vessels, must, I conceive, be ascribed a great part of such effects and influence.

Among the constitutional, or internal causes of disease, are education and professions, which, by constantly exerting peculiar impressions on the system, are calculated to occasion a proclivity to certain disordered states, whether by over activity of mind, or too little exercise of the intellectual faculties: these, by inducing disorder of the connected parts, may prove essentially a source of active disease; or the undue exercise of any organ, without allowing proper intervals of repose, keeps up a state of activity which eventually may predispose to increased excitement or debility.

The influence of the passions, or other powerful mental agencies, on the frame, and the production of organic change as well as functional disease merely, can only be named in the series of causes.

The mode by which diseases noticed in parents descend to their offspring, though too apparent to be doubted, has not been yet actually explained. Some of the diseases which descend appear to have their origin in that power by which the resemblance is conveyed, and is allied to the particular state of organization—an actually similar conformation being often observed; in those, for instance, who, having an additional finger on the hand, give this peculiarity to their offspring: thus in deafness, blindness, and other vices of conformation—probably the tendency to epilepsy, asthma, or mania,

may be actually connate; while other diseases, as rickets, owe their origin to the deficient nourishment supplied to the fœtus; and others again are dependent upon the state of the circulating fluids of the parent—the humours of the body—upon an actual *poison* pervading the system, as syphilis, and, as has been supposed, lepra and psoriasis.

A disposition to certain diseases, as phthisis and gout, is clearly transmissible; and no explanation thoroughly satisfactory is given. The disease at birth is not present, but at stated periods of life, and under circumstances apparently trivial, makes its appearance. Now as these disorders may be engendered by modes of life, or exposure to the injurious influence of climate (circumstances which operate on the nutrition of the body), we must look to those processes, therefore, for the origin of such diseases, which regulate the functions of nutrition and secretion.

Some disorders, as lepra, syphilis, and some specific virus, apparently are mitigated by descent, while others, as small-pox, strictly conform to the original description; while the proclivity to others, as gout, for example, and the other hereditary dispositions, by proper diet and attention to those circumstances calculated to give vigour to the frame, and by attention to the means by which nutrition is performed, may be entirely prevented.

The temperaments to which such great importance formerly was attached, the outward signs or indications of which were much studied in former days, and which were regarded as impressing the peculiar mental character on the individual, receive another and more rational explanation as to their original cause—namely, from the nervous system: cause is substituted for effect; the external characters being attributable to the state of the brain and nervous system influencing the processes of nutrition and circulation, and rendering the disorder of the fluids consequent on this action of the nervous power.

The next subject to which I shall allude, as capable of inducing disorder in the system, is that of the circulating fluids.

The quantity of blood in a part is of itself, if beyond the natural proportion, an excitant of disease; it may occasion the rupture of a vessel—it may stimulate the capillaries—and, having occasioned their dilatation, may give rise to increased effusion or nutrition. Many causes may operate to occasion the increased determination of blood, and the dilatation of the vessels: an enfeebled state of the circulating powers, as observable in aged persons, where the retardation of the blood is often observed to take place without any symptom of excited action, and to give rise to the senile gangrene—a dependant position, or mechanical obstruction, may also occasion turgescence—a state far removed from excited inflammatory action.

The harmony of the circulation, or the balance in the distribution of the quantity of blood, may naturally be altered by the state of perfection, decay, or activity of nutrition in the part; thus the head in infancy—the chest in those of more advanced age—the abdomen in those still older—and the glandular system in the latest periods—are particularly susceptible of disordered action.

The means in operation by which the quantity of fluids circulating may be diminished, are those which occasion general debility, or check the current flowing to a part: that this state may be a cause of disease is clear, when for the necessary functions a certain quantity of blood is requisite.

I have hitherto considered those origins of disease which arise from external agency, and those which spring from some constitutional causes, and the effect of the blood, from an increase or diminution in its quantity. Another question yet remains: can the fluids be primarily altered in their nature? Do we not in the present state of chemical science see reason to believe that the ancient doctrine of the origin of disease may, in a modified degree, be true? Now that our observations on the different states of the fluids have accumulated; now that facts are more dispassionately considered; and now that our modes of analysis are rendered more accurate and precise, can we maintain the belief that the fluids are absolutely inert, and that they are identical in their compositions under all circumstances?

Do we consider the changes of the component principles of the blood which take place during circulation to have any relation to chemical affinity? If so, can we hesitate to admit the supposition that extraneous substances may influence the properties of attraction, or may prevent the formation of natural combinations?

The idea formerly entertained must surely be abandoned, that the circulating fluids were all prepared by one process of digestion; that absorption was effected by the agency of lacteals and lymphatics alone; that the deficiency of fluids could alone be supplied by absorbent vessels, which, by exercising a controling power—a guardianship over the matter admitted to the system, permitted none to enter the blood except those calculated to strengthen or support the system. The power of imbibition, proved by M. Magendie to belong to all the tissues, throws open the door to the entrance of substances possessed of every variety of chemical agency. The crowd of experiments made on the fluids of animals exposed to the action of poisonous and other matters applied to absorbing surfaces, will confirm this fact. Verdegris, sugar of lead, hydrocyanate of potass, alcohol, prussic acid, iodine, mor-

phia, and many others, have all been detected in the blood, or in the secretions. Other substances, again, introduced into the tissues, have eluded discovery, not from want of power to enter the system, but having entered, from combining with the blood.

Some poisons, indeed, exert no obvious injury when either in contact with absorbing surfaces, as the poison of the viper. When this secretion was applied by Fontana to the tissues themselves—when nerves, tendons, muscles, cellular tissue, were wounded, and this matter introduced into them—no injurious result ensued; but when in contact, when mingled with the blood, the train of consequences is well known. That the fluids themselves which circulate are *altered*, or at any rate, *contaminated*, with such poisonous matter, the experiments of M. Verniere clearly prove. He injected the blood of an animal exposed to the influence of nux vomica, into the veins of another, who perished by the specific effects of the poison. This is still further illustrated by the death of leeches applied to those who have taken poison; and that this change may extend to a considerable distance, the intoxicating quality imparted to the urine when the amanita muscaria has been eaten—a mushroom possessing highly-exciting properties, and eaten by the inhabitants of northern Europe to induce intoxication—are proofs that the blood and the secretions can become mingled with substances entirely differing from those essential to the maintenance of the body; and some of which must exert a chemical effect on the fluids, and must be allowed capable of interfering with the ultimate changes in them; and that this effect is actually produced, we see in the altered state of the blood when sulphuretted hydrogen has been inhaled, which turns the blood to a black colour; or when nitrous oxide has been mixed with it, when it assumes a chocolate hue; or when prussic acid has been swallowed, when a blueish cast is imparted, and an oily consistence and appearance results.

But not only are examples to be found in poisons accidentally met with; in diseases we have examples equally striking. Mr. Sewell has informed me that he has communicated the itch from one animal to another, by injecting the blood into the veins. Dr. Home, of Edinburgh, tried the experiment of applying the blood of persons labouring under measles to an abraded surface in healthy persons, and thus succeeded in communicating the disease. A curious, though not an unparalleled case, was mentioned to me the other day by Sir R. Dobson, of the infection of small-pox having been communicated by a mother to her child in utero. A few days before her confinement she was exposed to the infection; she had, however, previously suffered from the complaint, and escaped, but the child after birth went regularly through the regular stages of the disease. How in these cases can the contagion be supposed to be conveyed, or where does the infection first develop itself? I may be told, that mingled with the blood, the poison exerts no change on it; that the influence is on the solids only; still must the blood be impregnated, and, consequently, vitiated. Should the solids alone be affected, why should not the excision of the primary small-pox pustule put a stop to the disease?

Do we not see, without any nervous affection at all, an altered state of the fluids—a change, preceding that series of symptoms which constitute fever? Dr. Stevens informs us that previously to the appearance of the yellow fever, or its premonitory or first symptoms, that the blood had already undergone a morbid change. May we not with some reason believe, as has been asserted, that the fever which succeeds the copious alkaline secretions in cholera is attributable to the circulation of a fluid ill suited to maintain the proper action of the nervous system?

We are too ignorant of many important processes performed in the blood, to come, perhaps, yet to any positive opinion as to the entire part which this agent performs. We know not how the fluids which are secreted from the body are formed; the nature of respiration is yet very imperfectly taught; the exhalation of carbonic acid gas from the lungs; the quantity of nitrogen, at different seasons, either absorbed or exhaled by animals, as proved by Dr. Edwards, who informs us that it was increased in spring, and diminished in autumn; while the introduction of oxygen into the system, (in its free state, shewn by that distinguished physiologist to be elaborated in the system,) points out a series of important changes probably taking place in the capillary vessels.

The mode in which the secretions are formed, or the matters to be excreted, then becomes a matter of great interest. Are these processes performed in the organs to which we usually attribute them? Do the kidneys secrete the urine? Do inflamed vessels alone secrete pus? The presence of urea in the blood, when the kidneys had been removed, as shewn by Messrs. Prevost and Dumas, decide the question as to the possibility of this matter being generated in the vessels. The detection of cholesterine, the base of biliary concretions, has been found not only in the gall-bladder, but, by Caventou, in an abscess in the jaw—by Breschet in the intestines. It has also been discovered in the ovary, and in other situations. The presence of most of the substances in the blood, as well those of natural parts as some of the abnormous formations, have now been found in the circulating current. The blood will not now be asserted to be always the same; that it ever should have been considered so

seems strange, when we see it obviously altered, as when the serum is milky; and do we consider it matter of indifference that it should contain urea, bile, pus, mucous-like, or tubercular matter? or that it should be deficient in its albumen, its colouring matter, its fibrine, or its salts?

Can we attribute to the nervous system all these alterations, and suppose them capriciously excited by a system which remains comparatively quiescent?

Then the fœtus, as mentioned by M. Chaussier, has been born with tubercles formed in utero: was this the operation of the nervous impression? Or may we not, with more obvious truth, assign this production of scrofulous matter to the fluids then circulating, to their quality, which thus disposed to the formation and deposition of this matter? How shall we explain the disease in children hide-bound—a hardened state of the cellular tissue, owing to the deposition of albuminous matter (a condition in which infants are occasionally born)? May we not presume it to be owing to the effusion of the peculiar substance deposited, which has been discovered in great abundance in the circulating fluids?

When, besides these infinite products of health and disease, we notice the alteration of its coagulating power by numerous poisons, or, on the contrary, have reason to conclude that it forms coagule during life, and observe the formation which has been noticed both in this country and abroad—the presence of tubercular-like matter in these concretions—or observe the formation of cysts, and even the formation of *pus* in them; when we mount up to the original compounds of this fluid, and perceive their source to be the atmosphere and the nutriment taken into the system; and when we conclude, as we have at least some ground for doing, that much goes on during the circulation of the blood, and that many of the processes (subject always to due nervous influence) originate in the vessels, and are owing to derangement of its actual constituents, we shall not be accused of merely advocating ancient doctrines, unfounded in principle and dangerous in practice; that we should see the ancient practice revived—that our patients should again be loaded with blankets in small-pox, to concoct the humours, or that the spissor and dentor alone should guide us.

That such ideas should be revived, it were as easy to believe as that we should again consider the blood, its appearance, and its parts, as the ancients did, who regarded the ascending vapour which rose from the stream as it flowed from the body, to be the "*spirit;*" who considered the serum—*the white blood*—to be the source of all the secretions; who accounted the bile to be a principal, because a shade of the colour of that secretion was observed in the fluid part; or that we should regard the clot as the blood proper, while, to the darker part of the coagulum, we should give one of the most important of the principles, the atrabile, the melancholia."

ON THE

REPRODUCTION OF THE CRYSTALLINE LENS.

By R. Middlemore,

Assistant-Surgeon to the Birmingham Eye Infirmary.

I have always considered that the lens is nourished by the circulation of vital fluids through its substance, and that it does not exist as an unorganized mass, or maintain its vitality, and derive its nutrition, by a process of imbibition. The vascularity of the lens has been asserted, and, with few exceptions, demonstrated, by Mæbringius, Winslow, Albinus, Camper, Hovius, Mæller, Heister, Senac, Haller, Zinn, Hunter, Young, and many other authors, and is now pretty generally admitted. Zinn has given two excellent representations of the vessels of the lens, in one of which the natural magnitude of parts is preserved; and in the other a much enlarged view of them is presented, with the following explanation. "Arteria lentis crystallinæ in facie ejus posteriori conspicua, et quidem, fig. 1, magnitudine naturali, et fig. 2, microscopio*." (270.)

It is not my intention to enter upon the investigation of the primitive development, and the mode of nutrition, of the crystalline lens, at all minutely, for these circumstances are not immediately connected with my present object—a recital of various experiments, with a view of determining whether or not the crystalline lens is reproduced after it has been removed from the eye.

It is universally admitted that the crystalline capsule is an extremely vas-

* They who are much interested in this subject will find an admirable account of the anatomy, development, and physiology of the crystalline lens, in the following works: "Descriptio Anatomica oculi humani iconibus illustrata, auctore Johanne Gottfried Zinn." "Observationes quædam botanicæ, et anatomicæ de nasis subtilioribus oculi et cochlea auris internæ, auctore Johanne Gottfried Zinn." "Scriptores ophthalmalogici minores." Edidit Justus Radius.

cular part; the engravings of Ruysch and Sœmmering exhibit this fact most satisfactorily; and it may be asked, why is this texture so liberally supplied with blood-vessels? In early life, the vascularity of the crystalline capsule is much greater than in adult and advanced age; and this circumstance is undoubtedly connected with the growth and increase in the consistence of the lens. The crystalline lens is not originally formed by vessels, which exist in the situation it subsequently occupies, but is secreted by the vessels of the capsule, which vessels are eventually prolonged and extended, until they form the true nutrient tubes of the crystalline itself; so that if the capsule of the lens were, from any defect of development, originally wanting, the lens could not by possibility be formed. The lens having thus originally no nutrient tubes, but constituting merely a secretion from surrounding vessels, is not one of the earliest developed parts of the eye, although it usually possesses in infants, at the period of birth, a certain and very distinguishable degree of consistence, and a well-defined figure. Now in conducting various experiments upon animals, with a view of ascertaining whether or not the lens is reproduced after it has been once extracted, it is important to bear this fact in mind; and if such experiments are intended to determine with accuracy the extent of reproduction in a given space of time, many other circumstances, hitherto neglected to be mentioned by those who have performed experiments relating to this subject, must also be taken into consideration. If the lens be removed from a very young animal, it is often exceedingly soft; its organization is not completed, and it is only imperfectly developed; and in such case, if the operation be carefully performed—if the posterior capsule be uninjured, and if the lens be permitted to escape by pressure upon the eye-ball, without any, or with only a very slight laceration of the anterior capsule—it will be very likely to be reproduced, and will then be formed, first, by a certain amount of secretion from the vessels of the posterior capsule, and secondly, by the organization of such secretion; just as occurred when it was originally formed and constituted a part of the living system. But, on the contrary, if the animal be old, or if the posterior capsule be materially injured, no reproduction will be likely to take place, because the secreting power of the vessels of the capsule is at that period much enfeebled, or the capsule itself, on which the reproduction of the lens depends, is in a great measure destroyed.

MM. Coiteau, and Leroy d'Etoille, whose experiments first attracted particular attention to the reproduction of the crystalline lens, appear to allude to the latter circumstance in the following words. "L'incision de la cornée fut faite à sa partie supérieure, à cause de la plus grande commodité que nous trouvions à operér dans ce sens, et parceque nous imaginions que l'humeur aqueuse s'echappant moins facilement alors par la plaie, elle pourrait s'introduire dans la cavité de la membrane cristalline, en tenir les parois écartées, et s'opposer à leur adherence, que nous soupçonnions être la cause de la non-reproduction du cristallin*."

For the purpose of experiments of this kind, I think that a rabbit, about five months old, is better than any other animal, and I believe that age to be preferable for the reasons I have previously mentioned.

In the prosecution of my inquiries, I have removed the lens from the eyes of dogs, of cats, and of rabbits; but the rabbit can be kept much quieter, and the eye is steadier, and more prominent, the cornea thinner, and upon the whole, I think that experiments performed upon the eyes of the rabbit are more exact and satisfactory in their results than those performed upon the eyes of cats or dogs. The injury the eye of the latter sustains in consequence of their vehement struggling, when attempts are made to divide the cornea, very often interferes with the success of the operation.

I have removed the lens of one eye from nine animals, in connexion with the present inquiry; namely, three times in dogs, and in an equal number of instances in cats and rabbits; but with regard to the two former animals, I have not been able to arrive at any exact conclusions, for the reasons already stated. The circumstances connected with the operation upon the eyes of rabbits I shall proceed to detail.

There is a brief notice in the Lancet for 1828, of the removal of the crystal-

* Journal de Physiologie, tome vii.

line from the eye of a rabbit, which the writer of the case says was reproduced in *six weeks*, of the general form, but of a much softer consistence than the original lens. On referring to the six experiments detailed by MM. Coiteau and Leroy D'Etoille, in the "Journal de Physiologie," I find that the following circumstances, (which, for convenience of reference, I have appended in an epitomized form) characterized the results of their operations.

First Experiment.—Animal killed a fortnight after the operation. Result: no lens in the right eye; left eye, a small soft crystalline.

Second Experiment.—Animal killed a month after the operation. Result: no lens in the right eye; left eye, a crystalline rather less consistent than it is naturally at the same age.

Third Experiment.—Animal killed five weeks after the operation. Result: no lens observed in the right eye; left eye, the contents of the crystalline capsule had not the proper lenticular form, nor the solidity of the natural crystalline.

Fourth Experiment.—Animal killed a month after the operation. Result: textures of the right eye changed by inflammation; left eye, "nous (say the operators) trouvâmes un corps *à peu près lenticulaire*, de couleur légèrement ambrée, épais d'une demi-ligne, analogue du reste à une membrane épaisse, ou à une portion de crystallin desséché."

Fifth Experiment.—Animal killed six weeks after the operation. Result: left eye, no reproduction of lens; right eye, lens reproduced, perfectly transparent, and of a lenticular form, but scarcely so thick or so consistent as the natural lens.

Sixth Experiment.—Animal killed six months after the operation. Result: lens reproduced in each eye, contained in its proper capsule, quite as voluminous and consistent as those previously extracted.

From the preceding statement, it would appear that in only five out of the twelve eyes operated upon, was the crystalline lens (in various degrees of perfection) reproduced. I shall now relate the result of three experiments, recently performed by myself, with the assistance of medical friends, upon the eyes of rabbits, with a view of ascertaining if the lens is ever reproduced when it has once been removed from the eye.

First Experiment.—I removed the lens from the right eye of a rabbit, about three months old, by an incision of the cornea; but the opening in the cornea was rather too small, and the lens was therefore only partially extracted. The operation occasioned great inflammation of the eye, and the animal died in about nine days afterwards, but no trace of a regenerated crystalline lens could be discovered.

Second Experiment.—On the 10th of January, 1832, I extracted the crystalline lens from the left eye of a rabbit, about four months old, by first puncturing the cornea with Beer's knife, and afterwards enlarging the opening by means of a pair of probe-pointed curved scissors. It was not necessary to divide the capsule afterwards, for the lens projected on the complètion of the incision in the cornea, and was removed without any difficulty. The iris was extensively prolapsed, and all my efforts to restore it to its proper situation were quite unavailing. I have daily notes of the progress of this case for a fortnight after the operation, but I shall only trespass on the time of your readers by extracting from them the following brief notice. "The eye went on improving until the 23d January, when the inflammation had entirely subsided; the corneal flap had united; the prolapsed iris had been withdrawn from the lips of the incision; and, excepting that the pupil is pulled towards the incision of the cornea, (around which there exists a slight degree of opacity) the eye is perfect in appearance." This animal was killed on the 14th of March by a blow with the hand behind the ears; and to my astonishment, the cornea gave way at the place of the incision, in consequence of the blow, which was by no means a powerful one. The posterior capsule was entire, but I could scarcely determine satisfactorily whether or not the anterior hemisphere of the capsule was present. In the place of the lens there was a semi-gelatinous fluid, surrounded by a substance of a more consistent quality, which was perfectly transparent, and became opaline on immersion in undiluted spirit of wine.

Third Experiment.—On the 10th of January, 1832, I removed the lens from the left eye of a rabbit about five months old, in the presence of Mr.

Startin, Mr. G. Elkington, and other professional friends. In this case it was necessary to enlarge the opening in the cornea made by the cataract knife, with a pair of curved scissors; and knowing the inconvenience arising from the small size of the incision in the preceding case, I extended the opening in the cornea to about two-thirds of its circumference. The lens was easily removed without at all injuring the posterior capsule, and it was considered to be a very large, and perfect, and, considering the animal's age, a very firm lens. The corneal flap soon united, and in the course of a fortnight nothing remained to indicate that an operation had been performed, except that the iris was adherent to the cicatrix of the cornea, and the pupil consequently rendered cordiform. I killed this animal by dropping a small quantity of prussic acid into the mouth, on the 12th of March, (rather more than five months subsequent to the removal of the lens) and afterwards dissected the eye in the presence of several medical students. I removed the cornea and iris very near to the junction of the former with the sclerotica. A small quantity only of aqueous humour escaped; for in consequence of the adhesion of the iris to the corneal flap, the anterior chamber was much diminished in its extent; the anterior capsule was only observed distinctly at the circumference of the new lenticular substance, but the posterior capsule was easily discovered in a perfect and uninjured state. The new substance, which I have considered to be the reproduced lens, is scarcely seen in the preparation accompanying this paper to be so entire and perfect as when the eye was first examined; for you will observe that there is a slight deficiency of the lens on one side, or rather an excavation at that part, which was originally filled by a fluid, intermediate in its degree of consistence between the aqueous humour and the natural lens. This was well seen at the time the dissection was made; but when the eye was immersed in spirit, the more fluid part dissolved, and became afterwards precipitated in the form of a brownish sediment; whilst the solid part, which accurately resembled the natural lens in firmness, form, situation, and transparency, became, as you may perceive, densely opaque. I have, therefore, supposed that the absence of a small portion of the lens, so apparent in the preparation, was owing to the want of a due degree of consolidation of that matter, which eventually acquires the firmness of the natural crystalline. It will be remarked that the reproduced lens is larger in its circumference than the one I removed from the eye (which is also sent for your inspection) four months before the animal was destroyed, and that it is much less convex*.

It will be seen in the two last cases related, that although no cooling lotions were applied to the eyes after the operation, no measures taken to prevent the bulging of the flap, to promote the union of the divided cornea, or to reduce the prolapsed iris, the organ was as speedily restored as usually happens where the best attention is given to insure the success of an operation for the extraction of the crystalline lens.

It will also be remarked, that in killing the first rabbit by a blow behind the ears, the recently united cornea was ruptured; and this leads me to repeat a caution I have several times mentioned with respect to the free administration of mercury soon after the operation of extraction, lest the united cornea should become disjoined, from the absorption of the connecting medium.

From the preceding, and other experiments which I have not thought it necessary to detail, it would appear, 1st, that the lens is exceedingly likely to be reproduced if removed from the eye of a young animal without at the same time injuring the posterior capsule, provided that no extensive inflammation of the eye be excited by the operation, and that the anterior and posterior capsules do not coalesce; 2, that the new lens is at first almost fluid; and that it acquires its proper figure before it obtains its due degree of solidity; 3, that it can scarcely be said to have acquired its full development until a year after its formation commences, calculating from the time when the former lens was extracted; but this period will vary according to the age and other circumstances connected with the condition of the animal which is rendered the subject of experiment.

I believe that if the second, or reproduced lens, were removed, another would be produced; and I see no reason why,

* The preparation arrived safe, and presented the appearances described by Mr. Middlemore.—Ed. Gaz.

if the capsule possesses the property of producing a second lens, when the original one has been extracted, it should not also possess the property of producing a third crystalline.

There is one important inquiry connected with this subject upon which I have not yet made any observation,—I allude to the reproduction of the lens in the human subject after it has been extracted by an operation. In performing the usual operation upon children for the cure of cataract, the capsules are often destroyed: hence an explanation of the non-reproduction of the crystalline in such subjects; and with regard to old persons, I have already stated my reasons for believing that the lens is not reproduced after it has been removed by the operation of extraction; but I am convinced that cases have occurred where, even in the human subject, the lens has been regenerated after it has been extracted, where the operation has been performed upon a person not much advanced in life, and without materially interfering with the integrity of the posterior capsule; still, so many circumstances are requisite to assist in the accomplishment of this result, that it can scarcely be matter of surprise that the event so rarely happens, presuming that the cases in which it is inferred to occur are characterized by the patient's capacity to distinguish objects at the usual focal distance, without the aid of glasses.

DUPLEX TWINS — SOME OF THE VISCERA COMMON TO BOTH, AND OTHERS DISTINCT IN EACH.

To the Editor of the London Medical Gazette.

Sir,

The following instance of the union of male twins, you will perhaps deem worthy of insertion in your journal.

They were born at West Bromwich on the second of April, and lived about ten minutes. The mother was advanced in her eighth month of pregnancy, when labour appears to have been brought on by deep anxiety, occasioned by one of her children having strayed from its home. She was attended by a midwife, who informed me that on examination she found the two heads presenting, face to face; and in order to facilitate delivery, she pushed up the head of one, which it appears probable tore the integuments forming the upper part of the junction. Other particulars of the labour I have not been able to ascertain, as the midwife, being agitated (as she states) by its unusual character, cannot give a very exact account.

The junction extended from the ensiform cartilage to an inch above the pubis, and evidently contained some of the abdominal organs. At the lower and anterior part it is a pouch, into which the umbilical cord terminated.

An examination of the interior was made by Dr. James Johnstone, Mr. W. S. Cox, of Birmingham, and myself. The following were the peculiarities observed. The walls of the junction were composed of common integument, muscular fibres, and peritoneum, the anterior being a continuation of the abdominal parietes, the posterior those of the "back," the space between them being occupied by viscera. Thus a communication existed just as extensive as the limits of the union described.

The diaphragms were united, forming a muscular boundary to the upper part of the common abdominal cavity.

The substance of the livers was also continuous, appearing as one, but having two gall-bladders. The gall ducts pursued their usual course. The stomachs were natural, but connected by an intestine extending from one pylorus to the other, apparently formed by the duodena united. From about the middle of this there proceeded at right angles one intestine only, about twelve inches in length, constituting a jejunum common to both; its extremity was somewhat expanded, from which two bowels, the ilia, took their origin. The remaining portion of intestinal canal was natural.

The pouch before mentioned was an umbilical hernia.

There was one placenta, and one funis, which contained four arteries and two veins.

The two sets of vessels separated at the base of the hernia for their usual destination.

Nothing peculiar was discovered in the contents of either thorax.

I am acquainted but with one similar conformation, which is related by San-

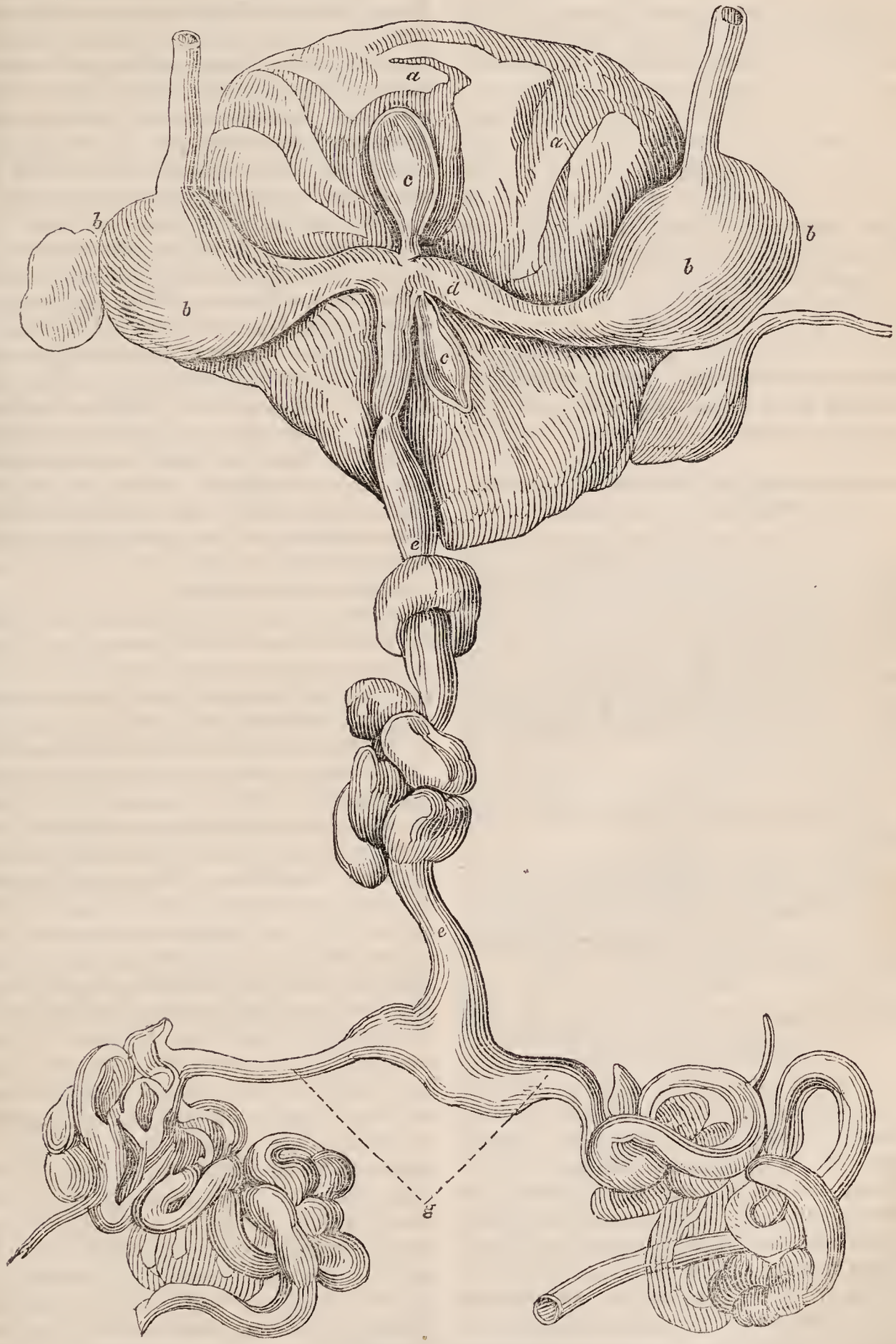
difort: he, however, gives no account of any dissection.

The livers, stomachs, and intestinal canal, are removed, and separately preserved. I have presented them to the Birmingham school of medicine, in whose interesting museum they have now a place.—I remain, sir,

Your obedient servant,

GEORGE BAYNALL, M.D.

West Bromwich, Staffordshire,



Description of Engraving.

a a—United lung.
b b—Separate stomachs.
c c—Separate gall-bladders.
d d—Common duodenum.
e e—Jegunum common to both.
g g—The bowel forming separate ilea.

ESSAYS ON HYGEIA;

OR THE

Art of preserving Public and Private Health.

BY

JOSEPH ROGERSON, Esq. of Wigan; and
GEORGE ROGERSON, Esq. of Liverpool;
Surgeons.

ESSAY III.

The subject of Muriatic Acid Gas continued.—Its injurious effects on Animal Life.—Its reputed Disinfecting Qualities.

HAVING in our last essay treated of the effects of muriatic acid gas on vegetables, we will now proceed briefly to lay before the reader some of our

Experiments on Animal Life with Muriatic Acid Gas.

EXP. I.—A young lively mouse was placed in a receiver filled with pure muriatic acid gas, and the vessel then put into a mercurial trough, with a piece of card floating on the surface of the mercury as a standing place for the animal. In two minutes and a half it died, after much gasping for its breath and struggling very little.

EXP. II.—Repeated on an older mouse, and on a bird, with corresponding results.

EXP. III.—A stream of gas was directed against a cage in which were enclosed two mice, when the old one expired in about ten minutes, and the young one in fifteen.

Observations on these Experiments, and Deductions from them.

The pure muriatic acid gas is extremely destructive to animal life, affecting chiefly the respiratory organs by preventing the circulation of red blood, and in general by injuring the physical texture. After death the lungs are found gorged with blood; and black blood seems to have been in circulation. When respired by man it irritates the higher parts of the trachea and the epiglottis, most frequently producing coughing and irregularity in the breathing, or a kind of temporary stoppage of it, and a stifled and oppressed feeling. It has corresponding effects on animals; for horses, in passing near where this gas is being poured out, will cough and be spasmodically affected in their breathing. In general its passage into the respiratory organs is very limited at first; the top of the trachea is irritated, and the epiglottis closes spasmodically, thus excluding or preventing its further passage: but when there is no other air, or when the trachea has become habituated to the stimulus of the gas, it will then even permeate the air-cells of the lungs for a time without distress.

The respiratory organs are not the only sufferers; every mucous membrane in the body shall be stimulated by it into disorder or disease. The conjunctiva of the eye, being the most exposed, is sorely afflicted; its texture is opaquely thickened, or entirely disorganized. If not long exposed it suffers much less, which is often the case, for the pure gas kills so soon that time is not allowed for those changes in structure to take place which would be the unavoidable consequences. The gastric and intestinal mucous linings, even in this short time, sometimes suffer.

Muriatic acid gas has been used, and even now is occasionally used, as *a disinfectant agent*—as the corrective of air in rooms which have contained noxious or putrid effluvia or miasmata, animal or vegetable, and which have had within them infectious and contagious diseases. The experiments of the chemist Morveau, and the case of the Cathedral of Dijon, are always thrust forward as prominent and striking examples. In medical literature there are found many words without any real or determinate meaning attached to them, or without having any real or existing object to represent; being, in fact, only so many idealities or non-entities: and these become the objects of the hottest disputes and the most subtle reasonings. Repetitions at length persuade belief, and custom sanctions and confirms it. But in hygeia and in medicine we should observe carefully the action of agents on the organs of the body, and judge them, from the effects there produced, to be good or bad for the animal economy—to be beneficial or injurious to public health. This is a philosophical and rational test; and if we proceed, according to this maxim, in the present instance, we shall find that every air which injures the respiratory organs, or which

is irrespirable or disorders the texture of the pulmonary apparatus, is impure and bad, and the atmosphere, polluted or vitiated by it, is infected—is, in truth, infectious. No matter what is the nature of the gas, nor of the source from which it emanates—if it destroy the purity of the atmosphere, be mixed with it, and render it unfit for healthy respiration, it is an infectant, and will produce diseases, or be capable of infecting all those who may be placed within the sphere of its action. Some of them are more subtile and powerful than others; some can act at greater distances than others; and some will affect certain states of texture rather than others; but still the general rule remains undisturbed. All these airs are infected and infectious, differing only as to the degree and extent of the injury they are capable of inflicting on the organs of life.

In this manner, if common air be mixed with any injurious gas, as muriatic acid gas, it will be more or less impure or infected, and will act injuriously on the lungs, be infectious by creating corresponding diseases on numbers, and be detrimental to public health. Muriatic acid gas is therefore an infectant or infecting agent, for it vitiates the atmosphere, and will produce the disease of one or more of the organs of those individuals who may chance to be exposed to its deleterious influence; and every air, or every room containing it, is infected, and is more or less unhealthy, and will only be purified or *disinfected* by introducing into the chamber, or substituting for the infected air, an atmosphere properly oxygenized; so that the lungs can breathe it with impunity and benefit, and living beings can support it with health and comfort.

Muriatic acid gas is in truth far from being a *disinfectant*; and we lay particular stress upon this word, because the weight of a name exercises with many a great power; and because a word, conveying a wrong idea, often becomes positively dangerous. Patients made to breathe this gas under the idea of its clearing away the infection in a sick room, are only breathing another kind of infected air; and those practitioners who recommend its use, and themselves practise it, are acting empirically, and opposed to all true physiological principles, for medicine is, and ought to be, based on physiology. We repeat, that muriatic acid gas is not a disinfectant, (except perhaps in cases where it forms some inert substance by chemical union, as it would with ammoniacal gas, if it were dispersed in the air;) —and that where manufactories are pouring out their volumes of it into the atmosphere of towns, that atmosphere becomes infected; and in proportion to the degree of the atmospheric infection, is that town insalubrious, and that vitiated air injurious to health. Those, then, who fancy that it may contribute to the preservation of public health, from purifying the air, are labouring under a delusion, and imagine so in opposition to the plainest rules of hygeia, and the most common principles of physiology.

It is utterly impossible that muriatic acid gas can purify the air while it remains muriatic acid gas, for pure air is healthy air, and can be breathed; but muriatic acid gas is unhealthy air, and cannot be breathed without danger. The only way in which it can purify the air when it abounds with some noxious matter, or disinfect it, is by producing a solid or non-aerial substance, through the union of its atoms with those of the other infectant; or by being converted into, or by producing a certain proportion of oxygen gas, neither of which it has hitherto been known to do. It must produce some chemical or physical change on the infecting substance, and either neutralize it by making a new compound (not an aerial one), or convert it into oxygen. In order to do this with certainty, and not to act empirically, the nature of the infectant should be known, and its chemical or physical qualities perfectly ascertained.

We again repeat, that muriatic acid gas is not a *disinfectant*. It may correct in some measure the bad smell in any place, arising from putrid animal substances, by chemical action on the animal matter itself, or the gases emanating from decomposition, which we shall more fully consider in a future essay on the hygeia of public cemeteries and butcheries; but still that place is infected either with the putrid animal effluvia, or that of muriatic acid, and it only becomes healthy, when, by currents of fresh air, those infectious gases are expelled, and a pure atmosphere occupies their place. It may be a sort of useful auxiliary, or intermediate agent, in assisting to clear a room of some kind of noxious air, by render-

ing it easier to expel both these unhealthy gases by currents of pure air, and substituting the pure air which disinfects it and makes it habitable. Its light specific gravity, approaching near to that of common air, will in this respect be one great means by which this is effected, for then aerial currents will experience less difficulty, and have less labour in forcing the impure air away and dispersing it. These may be some of the benefits of muriatic acid gas; but they do not entitle it to be accounted a disinfectant.

The quantity of pure gas consumed when death is occasioned, varies with the species of animal, seeming to depend on the capacity of the lungs. In the case of mice the mercury rises a very little, but with a bird it attained several inches; and with mice it appears to rise higher, as their chest is larger.

Experiments with Muriatic Acid Gas diluted with Atmospheric Air in various proportions.

Exp. I.—The proportion of one of muriatic acid gas and five of atmosphere (two in ten) well agitated, affects the breathing immediately, and kills in about three minutes. The animal (a mouse) struggled much and panted. Between the second and third minute it fell down in convulsions, breathing most laboriously, and shortly afterwards expired.

Exp. II.—The proportion of one gas and twenty air kills in about twenty minutes or half an hour. The effects produced are chiefly on the respiratory organs, and through them on the body. Immediately the breathing becomes laboured, and the animal (a mouse) gasps: its chest and abdomen heave violently and rapidly, which continues for above two-thirds of the time without any struggling, but apparently with prostration of strength. After this time its weakness increases and it is unable to stand, —it expires slightly convulsed.

In receivers filled with air and muriatic acid gas, mixed in the same proportions as those used for vegetables, the animals lived several hours; the duration of time increased as the gas was more and more diluted with the air, up to one in 1500,—when the experiment was so protracted, that we could not positively attribute the death which ensued to the gas; for mice pine under confinement and die. In the majority of these instances, no convulsions were perceptible, and the air, mixed with very minute quantities of the gas, seemed to produce a soporific effect. In the majority of cases, too, the animals were affected immediately on immersion; respiration was quickened and laboured; all the mucous membranes that are visible, as that of the eyes, that forming the outlets of the nose and mouth, were affected, and were obviously uneasy and painful, for the animal frequently rubbed them.

Observations and Deductions.

These experiments prove that muriatic acid gas and common air mixed together, are destructive to animal life, in proportion to the quantity of muriatic acid gas there existing; that is, the more muriatic acid gas is in a given space or quantity mixed with common air, the more destructive it is to animal existence. In all these fatal instances, all the gas was far from being consumed, for its presence was always detected by the test; and even the external parts and fluids of the animal equally tinged the paper.

Though it ceases to kill (at least within any reasonably short space of time) when mixed with 1500 parts of common air, yet it does not cease to be injurious to health. I myself have been a sufferer from its noxious agency on the respiratory organs. While engaged in making a series of experiments on muriatic acid gas, the apparatus for my conveniency was brought into my sitting room. Here the air became infected with a quantity of the muriatic acid gas, which had escaped during our manipulations from the defective luting, and from a retort containing salt and sulphuric acid accidentally left in the room. My breathing was affected; a sense of weight in my chest was experienced; pain and heaviness across my forehead; and, in short, I was labouring under a low incipient inflammation of the mucous membranes of the lungs. No sooner was this removed, than my throat was inflamed and swollen; but I do not know that this latter malady is attributable to the injurious stimulating qualities of the atmosphere, vitiated with the muriatic acid gas. It will be remarked that, with some of the mice, sleep was frequently induced, even at night, during the hours of which these animals are in general most lively, and even

when attempts were made to prevent it. A similar effect was produced on one who assisted me, and had breathed this vitiated air for some time,—his nights, contrary to usual habit, being passed in sound sleep, and drowsiness being felt before retiring to bed. This may be an occasional effect of air impregnated with muriatic acid, and, in all probability, arises from the circulation of partially blackened blood, which, arriving at the brain, produces torpor and sleep.

From its effects on the visible parts of the mucous membranes, we may conclude that similar changes take place on the remainder. In the pure gas the conjunctiva of the eye assumed a dull cast, and became opaque and thickened, so that in consequence of it some of the animals were blinded; but as the gas was diluted, these unhappy consequences were diminished. The eyes appeared to be frequently uneasy, painful, and irritated.

Though it does not, when much diluted, kill within a few hours, which was the greatest length of time we extended the experiments, yet it does not cease to be injurious to health. It will always be somewhat noxious, for it is unrespirable air; for though, after its being mixed with 1500 of common air and only one of gas, it produces no striking or perceptible effect; it will, by being a source of constant irritation and an improper stimulus, be liable to produce slow and gradual or chronic inflammations, or diseases of the lungs and windpipe. That portion of the mucous membrane lining the stomach and intestines, can be distressed or even inflamed by this kind of vitiated air, more particularly with those subject to mucoid diseases in the abdominal region.

The specific gravity and want of chemical affinity of this elastic fluid causing it to separate and fall, must make its effect still greater by reducing the proportion of atmosphere, and thus concentrating the gas.—In conclusion, it now must be admitted by all, that, however much diluted, it will never constitute an atmosphere from which the healthy organs of respiration can derive any advantage; since a certain proportioned mixture of oxygen is the proper food for the lungs.

SALINE INJECTIONS IN CHOLERA.

CASES TREATED IN THE MARYLEBONE INFIRMARY.—COMMUNICATED BY DR. HOPE.

CASE I.—*Eight pints injected in three hours—Remarkable temporary benefit—Ultimate failure.*

W. EDWARDS, æt. 48, thin, a gardener, admitted June 5th, at 10 o'clock, P.M. into the Marylebone Infirmary, under Dr. Hope. Skin deep livid and blue; cold, and covered with clammy perspiration. Tongue very cool; great thirst; pulse imperceptible at the wrist; at the carotids 100, very weak, irregular. Countenance collapsed; eyes sunk; deep livid areola; cornea dull; lips purple; violent cramps of the limbs; voice a whisper; rice-water vomiting and purging; urine free.

Has had slight diarrhœa for a week. Eight hours before admission the vomiting and purging became violent, but have now abated.

Baln. aeris calid. Vini Gallici ʒij. omni decimâ horæ parte. Injectio salina venis quam primùm.

℞ Hydr. subm. gr. viij. Opii gr. j. M. ft. pil. ij. statim sumend.

℞ Hydr. subm. gr. iij. Opii gr. ⅓, omni horâ ad sextam vicem.

2 o'clock.—The median basilic vein was opened, and two pints of the following fluid were slowly injected.

℞ Sodæ muriat. ʒij. Albuminis ovi ½. Aquæ Oij.

Radial pulse became perceptible in a few minutes; and, in an hour, when the two pints had been injected, the heat, colour, voice, and pulse, were in a great measure restored. In less than half an hour he relapsed into a worse state than before.

Rep. Injectio.

At 4, P.M. another vein was opened, and Ovj. were slowly injected, the strength of the solution being gradually increased to ʒv. of the salt to water Oj. During the introduction of the fluid the pulse gradually rose to a regular, firm, and steady beat, and the general heat was restored. During the operation the patient complained at intervals of pain in the head and loins; and finally, the lividity disappeared, the face became flushed, the eyes suffused, and he made

some violent muscular efforts. At 5½ o'clock the excitement subsided into stupor, respiration became hurried and stertorous; the pulse failed, and became 180; the lividity reappeared; and he expired at 8 o'clock, P.M.

SECTIO.—*Brain.* Great venous congestion, with serous effusion under the arachnoid.

Chest.—Venous conjestion less marked, and the blood less black and consistent, than in other corresponding cases.

Abdomen.—Stomach almost universally of mottled red colour and slimy. Isolated glands of the duodenum slightly enlarged; pink vascularity. An enormous quantity of thin, flaky, rice-gruel fluid, in nearly the whole tract of the intestines: where it was most abundant the mucous membrane was blanched perfectly white, while the fluid itself was pinkish; where the accumulation was less considerable the mucous and other intestinal coats were of uniform pale pink colour.

REMARKS.—This was an extreme case, and authorized the immediate use of the saline injection. Becoming still more desperate after the first injection, it was deemed justifiable to increase the strength of the solution, in the hope that a smaller quantity might expeditiously produce the desired effect. It becomes a question whether the strength was not ultimately too great, so as to have given rise to the cerebral irritation. Hence, in future injections, it might be desirable to keep the strength and quantity of the solution injected at any one time within such bounds as not to irritate the brain, by the stimulus either of plethora or of a too acrid fluid.

CASE II.—*Seven pints injected at thrice in five hours—Calomel and Opium also—Recovery.*

Ann Fletcher, æt. forty-five, a charwoman, accustomed to spirits, was admitted June 8th, eleven o'clock A.M., into the Marylebone Infirmary, under Dr. Hope. Previous health good; supped on the 7th on lobster currie; vomiting and purging, with epigastric pain and cramps, commenced at three o'clock A.M. Now, extremities cold and livid blue; pulse 120, barely perceptible; tongue brown, furred, moist, and cold; purging and vomiting of rice-gruel fluid; urine scanty; eyes sunk; livid areola. Pain over abdomen, but more particularly at epigastrium,—increased by pressure; cramps of arms and legs; voice feeble; vertigo; imperfect vision.

Baln. Aeris Calid.

℞ Mur. Sodæ, ʒss.; Carb. Sodæ, ℈j; Potas. Oxymur. gr. vij. ex aquâ statim et omni bihorio.

℞ Cal. gr. vij.; Opii, gr. j½. ft. pil. stat. sumend.

℞ Pil. Cal. gr. ij. c. Opii, gr. ss. om. horâ sumend. Sinapism abdom. et spinæ.

Half-past three o'clock.—Pulse not countable; skin very blue; countenance more collapsed.

Quarter to four o'clock.—Oij. of the following fluid were injected.

Sodæ Mur. ʒij.; Sodæ Carb. ℈ij.; Aq. Destill. Oiij.

Pulse greatly improved; she spontaneously expressed herself stronger, and much better in every respect; said she could "speak better." Pulse 114, firm and regular; giddiness nearly gone, and sight improved.

Half-past four o'clock.—Relapse; pulse scarcely perceptible. Injection of Oij. was repeated; after introduction of Oss. pulse became distinct, but on suspending the injection for a minute it flagged again. The same experiment was tried after the injection of each succeeding half-pint, or thereabouts, and with the same result, except that the interval during which the pulse beat firmly became progressively longer; and after the last half-pint the beat became permanently firm and regular.

Nine o'clock P.M.—Purging continues; rice-water stools; skin cold and clammy; pulse scarcely perceptible. Three pints of the saline mixture were injected into another vein between eight and nine o'clock; felt better after it, and much inclined to sleep; pulse improved.

June 9th, eight A.M.—Has passed a quiet night; slept well; expresses herself much better; countenance and colour natural; pulse 96, regular and steady. About three pints of rice-water fluid discharged from bowels during the night; passes urine freely.

Olei Terebinth. ʒj.; T. Opii, ʒj.; Mucilag. ℥iv. ft. enema st. inject. Omit. Pil. Cal. c. Opio et Pulv. Salin.

℞ Sodæ Carb. ℈j. o. horâ sumend.

10th.—Still purged; some feculent matter in last stool; otherwise well.

11th.—Had four bilious stools during the night; complains much of sickness and pain at scrobiculus cordis shooting through to the back; has vomited a quantity of bilious matter.

℞ Cal. gr. xv. statim; Olei Ricini, ℥ss. post horas tertias; Emplast. Lyttæ Epigastrio.

12th.—Nine bilious stools, of dark green colour; no sickness nor pain in abdomen; pulse 90, full, soft, good strength.

Mist. Cardiacæ, ℥j. 6tis h.; beef-tea; arrow root.

13th.—No complaint; three bilious but more feculent stools.

Remarks.—This patient, though far advanced in what is called the third stage, was not in circumstances so extreme as the preceding; and being younger, she was perhaps possessed of more constitutional stamina. The saline injection, however, was not resorted to until it was obvious, according to the antecedent experience of those present, that she was inevitably sinking. It was determined that the saline injection should not supersede the calomel and opium in small and repeated doses, previously found eminently successful in analogous cases in the institution; for it is possible, that though the injection may rescue the patient from immediate danger, it may only place him in the state of one affected with a less advanced degree of cholera, and not effect such a salutary change of action as to cure the disease. How far the restoration of the patient was attributable to a conjunction of the remedies, extensive comparative observation can alone decide; but it is probable that such a conjunction will ultimately be found to constitute the most rational and efficient treatment—each remedy being adapted to meet suitable indications. The writer has now seen several severe cases in which the saline treatment, with the exception of injection, did not produce the slightest beneficial effect.

[Two more cases of cholera have been admitted into the St. Marylebone Infirmary, under the care of Dr. Sims, during the present week, in which injection of fluid into the veins has been practised. We shall give the histories of these cases in our next number.—Ed. Gaz.]

CHOLERA—SALINE INJECTIONS—DEATH.

To the Editor of the London Medical Gazette.

Sir,

I offer the subjoined case for insertion in your journal, having excluded from the narrative all the minor details, as its interest hinges solely on the treatment which was adopted. The patient was selected from several others, as a proper one for again putting to the test the plan of the saline injections, which proved, in the present instance, as ineffectual as other modes of treatment which have been adopted in similar aggravated cases.

I am, sir,
Your obedient servant,
Charles Gaselee.

June 11th, 1832,
211, High-Street, Southwark.

Henry Mackenzie, a blind pauper boy, æt. 15, was attacked with diarrhœa, which was soon followed by vomiting, in the middle of the night of Tuesday, June 5th, the cramps being slight. When I first saw him at 10 a.m. on the following morning, the alvine discharges had ceased, and he had taken, about an hour previously,

Cal. et Opii aa. gr. j. formâ pilulæ.

10, a.m.—The disease is well characterized; collapse confirmed; no pulse in radial or brachial arteries, and the carotids are scarcely perceptible. Extremities livid; lips and ears remarkably so. Thirst excessive; and restlessness urgent. Tongue of a deadly coldness; voice choleric; vomiting is kept up by the fluids which he takes; mind unimpaired. How long this state had existed did not appear.

Warm bottles were applied, and the following injection was thrown slowly into a vein in the arm:—

℞ Aquæ tepidæ ℔ij. Muriat. Sodæ ℥j. Carbon. Sodæ ℈j. M.

The internal treatment consisted in the exhibition of Carb. sod. and Acid. tart. in effervescence, with brandy occasionally.

12 o'clock.—The only sensible effect produced by the foregoing treatment is,

that the respiration is more frequent. No improvement.

2½, P.M.—Is more restless; a damp perspiration is beginning to exude; thirst excessive; is evidently losing ground; no evacuations, except that the vomiting continues occasionally.

Repet. Injectio Salina ut antea.

The only apparent effect was that which was noticed before; namely, that the respiration was quickened, and became more laborious: he also complained of a sharp temporary pain on the crown of the head, to which he had alluded before.

It is needless to add more. The disease did not receive the slightest mitigation; and, at 5 o'clock, it only remained to replace him in the "shell" in which, forty-eight hours previously, he had extended himself for the amusement of his companions,—then, happily, unconscious that it would so soon become his proper receptacle.

CHOLERA AT CAWSTON.

To the Editor of the London Medical Gazette.

Great Yarmouth, June 11, 1832.

Sir,

In the little town of Cawston, situated within eleven miles of the poor, crowded, and closely-built city of Norwich, the cholera has made its appearance in a formidable shape; while in the last mentioned place not a case has been heard of, and the disease has not been seen any where else in this part of the country.

Cawston presents nothing peculiar in its locality, or in the habits of its people, amounting to eleven hundred. It is a retired village, apart from any high road. Mr. Grey reports thirty-seven cases of cholera, and one hundred and four of diarrhœa with sickness, as having occurred among this number of inhabitants between the first and tenth of June. I saw two of the first description, and one before leaving Cawston which was likely to become such. As some interest may attach to the appearance of the disease in this isolated situation, after passing over, if we may so speak, places that might rather have invited, and better have fostered it; and lest any doubt may exist upon the subject, I beg leave to offer the following case, being one to which I was a witness, and which I am authorized by Mr. Grey, the resident surgeon, to draw up as follows:—

— Dewing, aged thirty-six, a weaver, earning two shillings per week, upon which he and his wife subsisted, was attacked at six o'clock on Saturday, June 9th, with cramps, vomiting, and purging. He is now, at seven, covered with a copious perspiration, which turns litmus paper red; surface moderately warm on the central parts; extremities, tips of the ears, nose, and lips, cold and livid; skin on the fingers and toes shrivelled; tongue moderately warm, white, and flabby looking; countenance and manner anxious and appealing; pulse 90, small and feeble; abdomen drawn in; some tenderness upon pressure. In the morning he passed eight watery dejections, resembling whey, with flocculi suspended in them; since six o'clock, the time when he was seized with cramps, he has had no evacuation from the rectum, but the bladder has been once or twice emptied. Seen at ten o'clock, after a bleeding of ten ounces; coldness and debility extending from extremities upwards; the abdomen only is now warm; no pulsation at wrist; the heart is heard to act sharply but feebly, without any impulse; breathing not laboured; cramps and other symptoms aggravated, with the exception of the vomiting and purging, which have not returned. At twelve o'clock the collapse was making rapid advance; extreme distress; constant call for drink; was rolling about in the bed; the legs and arms were affected with succession of cramps. From this state he rapidly sunk, and died at one o'clock this morning *.

Mr. Grey gave him, at six o'clock, an emetic, and afterwards a grain of opium and five of camphor, repeated every hour. At twelve o'clock, in attempting to give him a suppository of camphor and opium, upon opening the sphincter he had a copious dejection of the character described.

Another case remained under treatment when I left Cawston; the patient had repeated dejections and discharges from the stomach of the rice-water appearance; the latter seemed to be relieved by the saline doses of Dr. Stevens. Of the result Mr. Grey has kindly pro-

* The *inspectio cadaveris* is here given: the appearances were those usually found in malignant cholera.—E. G.

mised to inform me, and if agreeable I shall communicate it to the Gazette.

I have the honour to be, sir,

Your obedient servant,

WILLIAM TRAVERS COX, M.D.

ANALYSES & NOTICES OF BOOKS.

" L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Two Lectures on the Primary and Secondary Treatment of Burns. By HENRY EARLE, F.R.S. &c.

THE publication of these lectures in so compendious and popular a form, is a benefit conferred on the community. Both the profession and the public at large wanted some manual of this sort to regulate their conduct in the many emergencies which often suddenly arise, and are attended with some of the most disastrous consequences; and perhaps if the Society for the Diffusion of Useful Knowledge had taken it into their plan to circulate a few treatises like this, they would have done more good than by several of their recent tracts, including their "Physician" with the odd brochure on "Cholera." With regard to burns, it is well known that numerous and the most opposite plans of treatment have been advocated from time to time, and that unprofessional people entertain the most singular notions relative to the nature of the injuries inflicted by fire; nay, persons of superior mind and education frequently find themselves totally at a loss how to proceed on the occurrence of such accidents. It was Mr. Earle himself, we think, who told us a most interesting story illustrative of this point. The child of a respectable gentleman had met with a severe burn, and a considerable time elapsed before the practitioner reached the house: in the interval, the parents were in the most distressing state of uncertainty as to what ought to be done. The celebrated Miss Edgeworth was an inmate of the family, and most inquisitive was she as to the line of conduct which should be pursued under such circumstances, and narrowly did she watch the progress of the cure. In a few months after, the medical gentleman (Mr. Earle) received a copy of her "Harry and Lucy," in which she introduced the anecdote of Harry being burnt in extinguishing the fire at a cottager's dwelling, and thus conveyed to her readers a knowledge of the proper mode of treating burns immediately on the receipt of the injury.

Mr. Earle's method of cure in burn cases is, of course, modified by circumstances; but it is clear that he has a strong leaning to the cold water plan, as the most rational and judicious in all ordinary conjunctures. The management of deformities, arising from the same source, is also embraced in the pamphlet before us, of which we are the less anxious to give a fuller account, as the work itself is so accessible and so well worth having.

A Practical Compendium on Midwifery; being the Courses of Lectures on Midwifery and on the Diseases of Women and Infants, delivered at St. Bartholomew's Hospital by the late Robert Gooch, M.D. Edited by GEORGE SKINNER, M.R.C.S.

"MAGNI hominis umbra." We have here, not only a posthumous work, but one that, we are almost sure, was never originally intended for publication. Mr. Skinner attended the lectures as a pupil, which he has now presented to the public. We think, however, that he has not done an ungracious thing in this matter. He appears to have a due regard for the credit and reputation of his late eminent preceptor, and the information which he has husbanded was certainly well worthy of being preserved. To the obvious condition of editing a work of this description Mr. S. seems reconciled; for the errors, whatever they may be, will be not unjustly charged to his score, while the excellencies must all go to the account of Gooch. The volume, which is of a convenient size, is well got out, and will be found a useful manual of inceptor accoucheurs.

On the Diseased States of the Kidney, &c. Parts I. and II. By J. C. GREGORY, M.D.—[Edinb. Med. and Surg. Journ.]

THE interesting observations made by Dr. J. C. Gregory, of Edinburgh, on the kidneys and their secretions in health and disease, are contained in two papers

inserted in Nos. 109 and 110 of our northern contemporary.

The questions discussed are very important both in a pathological and in a practical point of view; but such, nevertheless, as have not excited so much attention as they deserve. That a connexion between certain diseased conditions of the system, and particular changes in the urine, subsists, has long been known; but the precise nature of those changes, and the specific condition of the kidneys themselves, or of other parts which give rise to them, are questions of more recent inquiry, and such as are not yet fully elucidated. Drs. Blackall and Bright, but particularly the latter, have shown, that a peculiar organic change in the kidney may be generally ascertained even during life, owing to its connexion with a coagulable state of urine, accompanied by a diminution in its specific gravity. The universality of this inference, however, has been called in question by Dr. Graves, of Dublin, who, in some papers inserted in this journal, (vol. vii.) advocated the opinion, that the occurrence of a pale state of the urine, with a small portion of albumen, and an unusually low specific gravity—indicating a diminished proportion of urea, may depend upon the presence of chronic hepatitis. Some of the points connected with these investigations have been taken up by Dr. J. C. Gregory, and treated in a manner exhibiting at once minute, extensive, and patient research, with a cautious precision in the inferences which are deduced. The papers are very elaborate, and contain several tables, together with the details of not fewer than eighty cases; while, therefore, we are compelled by lack of space to omit these, we think it of importance to our readers that they should be made acquainted with the principal results to which they lead, namely, the connexion of a low specific gravity and the presence of albumen in the urine, with peculiar organic disease of the kidney, and, that this may be the better understood, we shall make some extracts from the general observations appended by the author to the data which make up the bulk of the paper.

"The low specific gravity, and the albuminous impregnation of the urine, appear to me to be of so much importance as diagnostic signs of this affection, that I have thought it right to draw up in a tabular form the results of the examination of the urine in fifty well-marked cases, in order to give as condensed and clear a view as possible of the changes which this secretion undergoes in the disease which forms the subject of these remarks.

"It will be found that this table furnishes an average of 1011.88 for the density of the urine in the fatal cases, and of 1014.48 in those which were dismissed relieved or free from complaint. The general average of the whole fifty cases, therefore, amounts only to 1013.18; and this low density is the more remarkable when it is considered, that, to avoid the source of error already mentioned, the specific gravity in a large proportion of the cases was taken when the quantity of the urine was considerably below the natural standard, which circumstance would necessarily have had the effect, if the secretions had been in a healthy state, of greatly increasing the general average. I may mention, in corroboration of the mean density resulting from this table, that there is a very remarkable coincidence between it and that furnished by the cases published by Dr. Christison. On taking the average of six cases in which the specific gravity was ascertained while the urine was not above the natural standard in quantity, I find that the result is precisely the same—namely, 1013 and a fraction. A seventh case, to which Dr. Christison has alluded, is not included in this average, because it appears in the foregoing table, having been fully detailed in the first part of this paper. Dr. Bostock ascertained the density of the urine in nineteen of the cases published by Dr. Bright. In some of these it was taken several times—in one six times; and in stating the average of sixteen of these cases to be 1017, Dr. Bostock has included in his calculation all the trials of the urine of the same person. But, by first taking the average of each case in which the specific gravity was ascertained oftener than once, and then taking the general average of these nineteen cases, I find that it amounts only to 1014 and a fraction—a result approaching very nearly to that given by the preceding table.

"From this remarkable correspondence in the averages of so great a number of cases, in which the density was ascertained by different observers,

it appears to be a general fact, that the specific gravity of the urine in this disease is decidedly lower than in the healthy state of this secretion; and that this low density, especially when combined with scantiness and coagulability of the urine, is a very characteristic sign, if not pathognomonic of the existence of this peculiar organic alteration of structure in the kidneys.

"Along with these observations on the specific gravity, a series of experiments was instituted in regard to the action on healthy urine, of the various tests and chemical reagents employed to detect the presence of albumen in compound animal fluids. My limits will not permit me to enter at present into any detail of these experiments; but I may mention a few of the leading facts and the principal results. The tests of the state of the urine employed in the cases of this disease which have been detailed or alluded to in this paper, were heat, corrosive sublimate, ferrocyanate of potass aided by a few drops of acetic acid, nitric and muriatic acids, and alcohol. In a considerable number of the cases each of these tests was tried in succession, and I have not yet met with a single well-marked example of this disease in which, when coagulation was produced by heat, the additional tests did not produce a similar effect. This, as will presently appear, is a point of some importance in reference to the action of the same tests on healthy urine. Indeed, from the result of my own observations, I should be disposed to recommend, that when it is wished to ascertain the presence of albumen in suspected urine, one or more of the other tests should be added to that of heat, in order to guard against a possible source of error. For this purpose, I prefer the ferrocyanate of potass, or nitric or muriatic acid, to corrosive sublimate. This last is perhaps the most delicate test we possess of the presence of albumen; but from this very circumstance it is less eligible as a test in suspected cases of this disease, because we have found that it very frequently causes in healthy urine a copious precipitate, differing in no respect from that which it produces in the urine of kidney disease, especially when a saturated solution is employed.

"The general, although not quite the uniform, result of 480 trials, is, that in healthy urine heat carried to the boiling point produces no effect, while corrosive sublimate, on the other hand, causes a more or less copious flocculent precipitate, according to the strength of the solution and the degree of concentration of the urine. But in a considerable number of cases in which the precipitate, thrown down by corrosive sublimate, was very copious and precisely similar in all its characters to that caused by the same test in albuminous urine, or in perfectly healthy urine to which a small portion of the serum of the blood has been added, the other tests above-mentioned produced no effect whatever. Nor have I yet met with one instance in which they caused a precipitate when there had been no effect from heat, even although that caused by corrosive sublimate had been very copious. This, coupled with what has been said above, seems to me sufficient to point out the importance of these supplementary tests, and the advantages they appear to possess in this respect over the more delicate tests of corrosive sublimate.

"I have stated, that this precipitation of healthy urine by corrosive sublimate, though very general, was not universal in the trials we have made, nor was it constant in all the trials of the urine of the same person. It appeared in some cases to be distinctly connected with slight derangements of the digestion, and in two or three it could be produced at will by certain articles of diet, particularly heavy unfermented bread, dumplings, pastry, and other preparations of flour containing suet or butter. In these cases, however, the urine was at the same time coagulable by heat. This albuminous impregnation of the secretion, in persons otherwise in good health, it is important to remark, was not attended in any case by a reduction of its usual density; it was very transient, and, where it was induced by certain articles of diet, it generally disappeared after some hours. In some of those cases in which corrosive sublimate caused a precipitate, a slight haze was also produced by heat, and this was generally observed where the precipitate from corrosive sublimate was most abundant.

"In regard to the source of the albuminous impregnation of the urine in this disease of the kidney, I must be very brief. Dr. Christison has considered the question, whether the secretion of albumen is vicarious of the se-

cretion of urea? and has shewn satisfactorily, that where the urine was very pale, and of very low specific gravity, and where, consequently, the proportion of urea and salts was very small, the quantity of albumen present was also small; while in the cases where the urea was still considerable in quantity, the proportion of albumen was also considerable. Similar observations were made in several of the cases detailed in this paper; and Dr. Bostock has stated, as the result of his experiments generally, "that the quantity of albumen in the urine bore no exact relation to the total amount of its solid contents, or to that of the urea in particular. It appears pretty evident, therefore, that in these cases, at least, the albumen contained in the urine was not a substitute for the urea; and the important fact ascertained by Dr. Christison, that in this disease urea may be detected in the blood, is sufficient to prove that the constituent elements still unite to form this principle, although its elimination by the kidneys is impeded or obstructed.

"The presence of the red globules of the blood in the urine, in a good many of the cases detailed, would seem clearly to indicate that the albuminous impregnation of the urine depends merely upon the transudation of a portion of the serum of the blood unchanged in its passage through the kidneys, especially in the earlier stages of the disease, during which the various shades of colour caused by the presence of the red globules in greater or less proportion, are most generally observed. That this is at least a principal source of the albumen, is rendered still more probable by the circumstance, first observed in one instance by Dr. Bostock, that where the urine is coagulable, as in Dr. Bright's cases, on which he made his experiments, the serum of the blood has a low specific gravity, and is deficient in its proper proportion of albumen.

"Whether, under any circumstances, or at any period of this kidney disease, the albumen in the urine may proceed from any other source, or is, in fact, vicarious of the secretion of urea, I cannot take upon me to determine. Dr. Prout considers urea as an albuminous product, and one of two principles into which that substance is capable of being decomposed, as he hopes at some future time to be able to demonstrate. My limits will not permit me to enter at all upon this very important subject; but I may state that the extraordinary relation which Dr. Prout has found to subsist between the elementary composition of the proximate principles of healthy urine, and that of the substances which this secretion contains in disease, seems to throw considerable light on this obscure point, and in a singular manner to confirm some of his very ingenious views."

With the talent for this kind of investigation, and with the opportunities which his situation as one of the physicians to the Infirmary afford him, we anticipate much instruction from the continuation of Dr. Gregory's researches, nor doubt that he will prove himself a worthy scion of the stock from which he springs, supporting the reputation of a name associated in the minds of many of our readers with some of their earliest and most pleasing retrospections.

MEDICAL GAZETTE.

Saturday, June 16, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

NEW LUNACY BILL.

How many of our readers are aware of the state of the law with regard to lunatics? If we may judge from what we hear when we mix in medical society, very few know any thing about the matter; most persons, indeed, have some vague idea that an act was lately passed connected with the subject, but they opine that it concerns only the "mad doctors," and touches not them. This, however, is a mistake. The Bill which passed in 1828, though professing to be merely to "improve the care and treatment of insane persons," delegates to a commission powers of a nature so extensive that no medical man can safely flatter himself with being beyond their reach. We are convinced that it is only because our brethren were not aware of the full bearing of some of its provi-

sions that they shewed so much indifference upon the subject formerly; and in truth, the time and attention of the medical practitioner are so much engrossed by the immediate objects of his daily and hourly duties, that he has neither leisure nor inclination to attend to questions of legislation, unless on some very special occasion, as that of the Anatomy Bill, which appeals to the feelings or the interests of every one who has the slightest connexion with medical science. Now, admitting as we do that the extension of legal protection to the practice of dissection is more important in a scientific point of view, we must at the same time assure our readers that the business of the practitioner, as regards his comfort, or even his character, are much more immediately implicated in the Lunacy Bill. A sketch of its history will make this apparent.

In 1827, a committee of the House of Commons was appointed to examine into the condition of the *pauper* lunatics in licensed houses, and much of the evidence on this inquiry, it will probably be remembered, was published in the daily papers. The inquiry on the occasion alluded to, was strictly confined to the treatment of this class of lunatics in certain establishments in the metropolitan district. In the course of the following year, a measure was brought before parliament, under the title of "A Bill for regulating the Care and Treatment of Insane Persons," which embraced, not only the objects for which the inquiry was professedly instituted—viz. the treatment of *pauper* lunatics—but enactments were made which applied to lunatics resident in private asylums, and which were founded on data as to the condition of such institutions obtained twelve years previously. These enactments appeared to us, at the time, to be framed without a sufficient knowledge of the circumstances, and with too great a disregard of the interests of those who had devoted their lives and embarked their capital in this very responsible and important branch of the healing art; nor has the result given us any reason to change that opinion: on the contrary, we know that great, and even ruinous loss, has in some instances accrued. The circumstance which has mainly led to this, and the one of which we chiefly disapprove, is that the Bill, in spirit and in detail, evinced an unaccountable feeling of suspicion towards the medical profession—a feeling which caused the establishments for insane persons to be placed under a surveillance of extreme and unnecessary severity.

Nevertheless, Mr. Gordon's Bill passed the Commons. The Peers, however, though they did not, as we could have wished, adopt the simple and effective Bill proposed by Sir George Tuthill, modified and improved that of the lower house to a very considerable extent, and, among other changes, placed the administration of the act in the hands of the Chancellor instead of the Secretary of State, who could not be supposed to understand much about questions of such intricacy and delicacy as must have been constantly submitted to him.

The act was passed in haste, and obviously in a very crude state. It was to remain in force for three years, or until the close of the then current session; and it is the fact of the period of its probation being just about to expire which has induced us again to take up the subject at the present moment. In a spirit of fairness which does him credit, Mr. Gordon some time ago made a draft of a new Bill, and circulated it, for comment, among some of the best informed practitioners interested in the questions to which it related; but, strange to say, many of the most wholesome and necessary suggestions

which were thus offered met with a cold reception, and were ultimately rejected: the bill, as it now stands, exhibiting proofs of increased, rather than of diminished suspicion of the medical character. So strikingly, indeed, was this the case, that an eminent physician (Sir George Tuthill) framed and published the draft of a counter-bill, with a letter addressed to the Lord Chancellor, and we think that every liberal and right-thinking man will concur in the feelings and views which he expresses.

"The object of my Bill (says Sir George) is to afford a complete protection to the insane, without adding to the keen suffering of those who are dearest to them, and without degrading those who must be concerned in directing the management of them.

"A principle of my Bill is an honourable secrecy; of Mr. Gordon's, a painful publicity. Mine endeavours to uphold the honour, the integrity, and the dignity of my profession; that of Mr. Gordon has the effect of degrading it. His is a severe inquisition into private calamity; and mine is a careful concealment of it. In their anxiety to secure the liberty of the subject, the framers of Mr. Gordon's Bill have forgotten that the affections which bind us to each other are dearer than liberty itself. The feelings of the parent, the husband, the child, are with me sacred in their agony; by Mr. Gordon's Bill, I am convinced that he never saw the wounds open which I so often witness, and does not know concealment to be their only balm."

Sir George Tuthill farther remarks, and the subject is of importance, because it is now again to be discussed and more permanently settled—"Mr. Gordon's Bill asks for powers with which, I believe, no commissioners have ever before been invested; by which the accused is, as it were, twice tried, and all his means of defence can be wrung from him by the prosecutor before he enters Westminster Hall." Those powers, however, were granted, and, unless we are egregiously mistaken, they constitute an anomaly in British legislation, and are at variance with those privileges which have hitherto been regarded as essential to the boasted freedom of Englishmen. Not only are the accused placed in a situation to be "twice tried," but in one where defence becomes difficult, or impossible; and where, indeed, the attempt at vindication may afford the subject of prosecution and the means of conviction. It is within our knowledge that physicians have been peremptorily summoned before the commissioners (whose call they are compelled to answer, in season and out of season, under a penalty of fifty pounds), examined concerning charges made against them, but without the nature of those charges being made known to them, and without their being confronted with their accusers—the inquiry being conducted with closed doors, and the answers taken down by a short-hand writer! If this be suffered to continue the law of the land, then is the reputation of every medical man who but looks upon an insane person, at the mercy of any discarded servant,—of any intriguing relative of the lunatic,—or even of the maniac himself—should he be, as many are, at once cunning and malicious. The objections to the details, in fact, are numberless, and we beg to refer our readers, for further observations upon them, to our leading articles of February 5th and 12th of last year, in which they will find the subject very fully discussed.

Among the commissioners are many men of high character, and they also embrace in their number several eminent and justly-esteemed members of our own profession; nor do we intend for a moment to charge them either with inefficiency or intentional rigour; but the machinery of the Bill under which they work is in several respects bad, and such as we are sure they cannot themselves approve. Some of

those gentlemen give their services gratuitously—others are paid; and we need scarcely say that the latter are and will be the most useful and attentive. It is only consistent with the nature of things that it should be so. Zeal may lead a man to accept an appointment in the hope of being useful, but it is mere affectation to argue, that where neither fame nor profit is to be obtained, the zeal is not marvellously apt to cool: accordingly we can already perceive some symptoms of this kind. Would it not be thought absolutely necessary for every commissioner, before he concurs in a general report to parliament on the state of madhouses in his particular district, first to have inspected every such establishment? Yet we are assured that not one half of the lay commissioners of the metropolis have done so. Nay, we believe that even the prime mover in the act, although he has been nearly four years a commissioner, has not yet made the tour of the London district. He has taken great pains with one large establishment—that which first elicited the late inquiry—and we believe has improved it very much; but how can any commissioner personally answer for the general efficiency of the act, unless he has himself seen and examined all the institutions within his district?

There are certain provisions of the present Bill incompatible with the privacy which every man of delicate feelings would desire to be observed towards a relation near and dear to him, while labouring under the most humiliating of human calamities;—observe we say privacy, not concealment. All the necessary security against abuses might be obtained, without the exposures to which the present arrangements give occasion—at least such is our opinion. In truth, the framers of the Bill legislated on retrospective information, taking as the grounds of their proceedings the general parliamentary inquiry which had been made into the state of madhouses in 1815, without duly considering the improvements which had taken place in the interval; and what is more singular, without consulting practical persons upon the subject—much less those whose interests were immediately and deeply involved. Indeed, so literally was this the case, that the Bill was introduced, and had proceeded to its second stage, before any of the proprietors of licensed houses were made aware that it contained provisions for any other purpose than to regulate the management of pauper lunatics. As soon as its nature became known, efforts were made to have the objectionable parts omitted or modified; notwithstanding which it passed *sub silentio*, without comment or discussion; so absolutely wanting under the present, or rather the late system, was any means of effectually representing the medical profession in the Commons House of Parliament. In the House of Lords it was petitioned against: the Peers not only listened to the objections, but maturely considered them, and, as we have said, greatly improved the Bill. The objections then urged, and which still remain in their full force against this Bill, are—that it vests all the real authority and administration of the act in the hands of commissioners, only one in four, or at most one in three, of whom are medical men, and who are therefore always exposed to the risk of being overruled by their lay brethren; that it deputes powers unprecedented in legislation, and which, if abused or injudiciously acted upon, might lead to the ruin of any medical man who should, though but inadvertently, infringe one of the numerous and complicated clauses* of

* Lord Eldon observed in discussing one provision, that *twenty misdemeanors* might be committed under that particular clause. See Medical Gazette, Feb. 5, 1831.

the act. Then the construction of the commission is essentially bad; it consists of certain members who are paid, and certain members who are not—so that the parties are not on a level as to the independence of their services; it consists of noblemen, members of parliament, magistrates and physicians—so that they are not on a footing of equality either as to rank or acquaintance with the subject; and feeling as we do at least as much respect for the intellect of the latter as the former classes of the commission, we cannot avoid the suspicion, that the desire to oblige those who have more or less of patronage to bestow, and who besides honour the Board with their gratuitous attendance, must be rather inimical to absolute freedom of discussion, or independence of decision. When we consider the nature of the duties which the commissioners have to perform, we feel perfectly convinced that they would be best discharged by a majority of physicians, with a minority of lay members and magistrates; but there ought to be no noblemen among them, or, at least, none who do not receive a salary, and to whom such remuneration is not a sufficient object to secure attention to his duties. At present the medical commissioners are so small in number, that they may at any time be outvoted, and that by men who cannot possibly be so well able as they to judge of affairs relating to the "care and treatment of lunatics."

Medical men ought to use all their influence, by private explanation and public petition, to bring the members of both Houses to a better understanding of this important subject as speedily as possible;—a little more delay and the time for exertion will have gone by. For ourselves, we have done our duty in repeatedly calling attention to the subject, and we shall conclude as we did on a former occasion, by declaring our belief that, "should the bill pass in its present form, and in the full vigour of its imbecility, it will do more to injure the condition of lunatics, and to aggravate the perplexities of their friends, than did all the worst abuses which it is intended to obviate."

ANATOMY BILL.

To-night (Friday) the second reading of this tedious piece of legislation is to be moved in the House of Lords. We are most anxious to learn what may have been the progress of opinion among their Lordships on the subject, since the time they rejected Mr. Warburton's first bill.

BARON CUVIER'S SUCCESSOR.

M. de Blainville, Professor of Physiology in the College of France, has been appointed the successor of Cuvier at the Jardin du Roi.

PROFESSIONAL POINT OF HONOUR.

It is said that the French Government last week made a most disgraceful proposal to the medical men of Paris—to give an account of such patients of theirs as had received wounds in the late sanguinary tumults. It is needless to add, that it was rejected with indignation.

ROYAL INSTITUTION.

Friday, June 8th.

Mr. D. O. Edwards on Baron Heurteloup's latest Improvements in Lithrotrity.

In introducing this, which was understood to be the principal part of his subject, Mr. Edwards took occasion to review the progress of the various expedients adopted, from the earliest periods, for relieving stone in the bladder. With regard to *lithotomy*, which was very early practised, it was curious, as the lecturer observed, how few modifications have been effected in that method during the lapse of ages. The proportion of deaths to recoveries, in cases of lithotomy, has been always considerable; and even at the present day it is usually reckoned at one in seven. In the Norfolk and Norwich hospital, during the 44 years between 1772 and 1816, 506 persons were cut for stone,

of whom 70 died, being about one in seven and a quarter; and Norwich has always justly been had in high reputation for the lithotomic skill of its surgeons. Cheselden lost 20 out of 213, in St. Thomas's and the Westminster hospitals; and Mr. Lynn, whose cleverness as a lithotomist has never been surpassed, told Mr. Edwards the other day, that at the commencement of his professional career, he performed 30 operations successively without losing a patient, but immediately afterwards had a run of seven fatal cases; and he moreover allows that the mortality in his practice of the operation, during the last fifty years, has not been less than one in eight.

In the year 1813, one Gruithuisen, an obscure Bavarian surgeon, demonstrated the possibility of introducing a straight instrument into the human bladder, and subsequently proposed certain methods of destroying the stone within the organ. One of these methods was the application of a sort of hydrostatical force to the stone through a double canula; another, through the same channel, to apply the influence of a voltaic battery; but his third method was the most rational and important; it was the mechanical perforation and breaking up of the stone. But all his plans were confined to theory.

Mr. Elderton, of Northampton, was the next in point of time who invented an instrument to act upon the calculus in the bladder; but after some trials his device was abandoned. M. Civiale thought of a purse in which to catch the stone, and act upon it with chemical solvents; but this project was also abandoned, the *materiel* for such a purse being unattainable. M. Leroi d'Etiolles was of opinion that *curved* sounds only could be introduced into the bladder, and consequently all *his* projects for perforating calculi proved impracticable. M. Amusat, at length, completely removed this impediment; he demonstrated by dissection the practicability of reducing the male urethra to a right line.

Both Amusat and Le Roi now devised straight instruments for destroying calculi; but for M. Civiale was reserved the high gratification of first breaking up the stone in the living bladder. This was accomplished in January 1824. M. Civiale's bold example was presently followed by Heurteloup, Leroi, and Amusat; and since that time several hundred lithrotritic operations have been performed.

Mr. Edwards now proceeded to describe the lithotritic process as usually practised anterior to Baron Heurteloup's ingenious modifications, and this led him at some length into a popular demonstration of the bearings of the human bladder, and the nature of urinary calculi. Our space will not permit us to follow him in this part of his discourse; nor do we think it necessary to more than allude to most of the Baron's instruments, including the surgical syringe, the *perce-pierre*, the *mandrin-à-virgule*, the *evideur-à forceps*, the improved perforator, the *percuteur droit*, and the *brise-coque*,—all of which, together with the "rectangle bed," have been already fully described in the pages of this journal. (See the Baron's papers, with woodcuts, which are indispensable in these descriptions, in our fifth and sixth volumes.) But we shall give Mr. Edwards's account of the most recent, and, as we agree with him in thinking, the most important of all the Baron Heurteloup's lithotritic inventions; we allude to the *percuteur courbe à marteau* and the *magazin*:—

"The application, however, of the *brise-coque* to large, flat, and oval stones, was found to be not unattended with danger, and the active imagination of Baron Heurteloup was long employed in the invention of a means of destroying these last bodies. He has succeeded, at length, in devising an instrument, which may be called his masterpiece, on account of its simplicity and beauty, and which, after a little modification, is likely to supersede all the other lithotritic apparatus. It has been named by the Baron, the *percuteur courbe à marteau*."

"This instrument consists of a sound, straight to a certain point, and having a definite curve at its extremity. This form enables it, like the recto-curvilenear sound, to move freely in the bladder, and to enter with ease over a tumid prostate. Instead of being a hollow canula, it consists of three bars of iron arranged laterally, the centre bar sliding backwards and forwards between the two side ones. The beak is divided transversely, and the anterior half is moved in unison with the central bar. The edges, at which these two parts come in contact, are serrated, and closed together like a suture.

"At the manual extremity are fixed two nuts, the one connected with the internal, the other with the external bars; by the respective movement of these nuts, impelled by the hand, the instrument opens or shuts. The simplicity of the instrument enables it to be moved with facility in all directions, and to catch the stone, whether that body be situated laterally or centrically. When the calculus is once seized, the grasp of the instrument is secured by a screw, and the *armure* is then placed into the *point fixé*, and a succession of smart blows are struck on the end of the central stem, the impulse of which is conveyed to the beak, and cracks the stone. The fragments are subsequently comminuted and expelled, as in the other instances.

"In ordinary cases the detritus is carried away in the stream of urine expelled by the natural efforts of the patient. But when the bladder is paralyzed, or the prostate enlarged, or the stream of urine deprived, from any other cause, of its prosilient force, the na-

tural efforts become inadequate to the expulsion of the fragments, and artificial means are rendered necessary.

"The first resource of lithotritists under these circumstances was to introduce a catheter with large eyes, so as to facilitate the escape of the detritus through its area. But the entanglement of the fragments in these apertures, which occasionally occurred, was a dangerous circumstance, and in many instances laceration of the urethra was the consequence. Baron Heurteloup has availed himself of this very circumstance to invent a new instrument, which has proved very effectual in removing the fragments. He has constructed a straight recto-curvilinear catheter, of uniform diameter, and which may vary, according to the size of the urethra. About an inch from its point are two large oval foramina, placed directly opposite each other. Their being thus placed is a matter of importance, because, when they are withdrawn into the neck of the bladder, a stoppage or obstruction in the stream of urine is an indication of the entanglement of a fragment. An iron stilette exactly fitting the area is introduced, and, being impelled forcibly, crushes the fragments. Some of the remaining molecules are pressed into the thimble-like extremity of the tube, while the rest escape into the bladder, and are carried away with the water. This terminal part of the instrument the Baron has named the '*magazin*,' and it is attached to the main instrument by means of a screw. When the catheter is curvilinear, the stilette is hinged in a peculiar manner, to adapt itself to the curve of the canula; but its strength is not on that account diminished. Several fragments may be destroyed in this way without withdrawing the instrument."

When Mr. Edwards had concluded his lecture, which was listened to with the greatest attention by a large audience, Mr. Faraday came forward, and, after pointing out some peculiarities in a new form of gun, announced that this was the last evening of meeting for the season. He said, that whatever difficulties the Institution had to contend with in other respects, those meetings had given to all parties the highest satisfaction, and he anticipated still greater successes, he would venture to say, when, in the course of some months, they should be once more resumed.

Baron Heurteloup soon after obeyed a general call, and delivered, with the aid of an interpreter, a short, but very animated and interesting, demonstration of his *hammering* process. He used a real calculus of the hardest description; and it was really surprising to observe the facility and apparent safety with which, by a few smart blows, he succeeded in breaking it up.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

GENERAL DISPENSARY.

Irreducible Omental Hernia, with symptoms resembling those of Strangulation.

Sarah Evans, aged 50, was admitted under the care of Dr. Lambe, May 19, 1832. For about a fortnight previous to her application she had been suffering severe colicky pains in the bowels, which of late had very much increased in severity: she had a double inguinal rupture; that on the left side was reducible, the other had not been returned for several years. May 25th, Mr. Coulson was requested to see the patient, in consequence of symptoms of strangulation having supervened. She was vomiting excessively, the abdomen was distended and painful, pulse small and feeble; there was hiccough, and the bowels had not been opened for three or four days. On examining the tumor in the right groin, a hardened mass was found, having the feel of a string of hardened glands: it was not painful on pressure. A truss with a *flat* pad had been worn on the rupture many years, but of late the hernia had become so painful that she was obliged to leave it off. Judging from the symptoms that a knuckle of intestine might have got under this hard mass, ice was ordered to be applied in a bladder to the tumor; and it was recommended, if she were not relieved in a few hours, that the operation should be performed. Her symptoms increased towards night, and the friends called in an eminent medical man, who advised the immediate performance of the operation. The patient, however, begged that nothing might be done until the morning. The ice was constantly kept to the hernia. Towards morning she slept, and on awakening the sickness and vomiting had left her, the pulse was full and strong (108), and the bowels were soon after freely opened. She continued improving for two or three days, after which all the former symptoms returned with increased severity. The operation was not again proposed, but the usual remedies for subduing inflammation were tried, but without success, and the patient expired on Tuesday, June 5th.

Permission could only be obtained to examine the tumor in the groin. It contained a small portion of omentum, which was firmly united with the front of the sac; the posterior surface was free, and the finger could be easily introduced between it and the corresponding portion of the sac in the abdomen. The two rings were approximated to each other. The sac was very much thickened. The omentum was of a dark colour externally, but, when cut into, presented no unhealthy appearance. Mr. Coulson said that the form of the pad which the patient had been wearing appeared to him objectionable. In irreducible omental

hernia, the pad which answers best is one hollowed or concave in the centre, the concavity corresponding to the mass of omentum which is not reduced. In this case a flat pad had been worn for a long time, which, though it prevented any further descent, pressed on the omentum which was already down, and at last occasioned so much pain as to compel the woman to discontinue its use. A patient who has been in the habit of wearing a truss for a long time, and suddenly leaves it off when the complaint exists, is almost in greater danger of having his hernia strangulated than if he had not worn a truss at all. And in such cases the strangulation is sometimes caused by the neck of the sac itself. Mr. C. had a striking case of this kind a short time ago. A man who had been in the habit of wearing a truss for an oblique inguinal hernia discontinued its use, during which time the gut came down, and could not be returned. The operation was performed, and before the hernial sac was opened, the finger could be readily passed anterior to it beyond the internal ring; the intestine, however, could not be returned. But when the sac was laid open, a director only could be introduced between the intestine and the thickened neck of the sack, upon the division of which the hernia was easily reduced. A striking feature in this case was the similarity of the woman's symptoms with those of strangulated hernia. When a patient, who has an irreducible hernia, is attacked with inflammation of the bowels or with ileus, it is sometimes very difficult to distinguish between the symptoms of these latter affections and those of strangulation of the gut. At the early stage of this woman's complaint, the performance of the operation was urged, but the progress of the case led to the inference (as was afterwards proved by dissection) that the rupture had nothing to do with the symptoms of the disease.

Impetiginous Ulceration of the Legs cured by the Application of the Oxyde of Bismuth.

William Andrews, aged sixty-eight, was admitted under the care of Mr. Coulson, with impetiginous ulceration of the left leg. The limb was affected two-thirds of its extent, and the complaint had existed for the last twelve months. The smarting and itching had latterly become very distressing, and the skin was very much inflamed. The patient was ordered to sprinkle some oxyde of bismuth on the limb night and morning, and to take some aperient medicine occasionally. The patient was also directed to wash the limb with warm water every time prior to the application of the powder.

The complaint rapidly subsided under this treatment, and in less than a fortnight was completely cured. The oxyde of bismuth has been tried in several cases of extensive impetiginous ulcerations in this institution, with great benefit.

Extensive Ulceration of the Integuments cured by the Application of the Mel Æruginis.

James Lindsey, aged thirty-four, was admitted December 12, 1831, under the care of Mr. Coulson, with extensive ulceration of the integuments covering the upper and anterior part of the abdomen. States that about six months previous to his admission an abscess formed in the situation just mentioned, the origin of which he attributes to a blow. Ulceration followed, which resisted every means that were employed to arrest it. After the lapse of five months he applied at this institution. There was a large ulcer, five inches in breadth and three in length, situated between the umbilicus and scrobiculus cordis, with irregular edges and a copious unhealthy discharge. He was in a very weak and enfeebled state. Ordered to use the chloride of lime wash three or four times a day, and to dress the wound with simple cerate; quinine mixture every day at noon. He improved but slowly under this mode of treatment. March 7th he was ordered to touch first the edges, and after a few days a greater extent of the sores, with the mel æruginis, which greatly hastened the cure. The sore is at present, May 29th, nearly healed, and his general health as good as ever.

LONDON HOSPITAL.

Hydrophobia from the Bite of a Fox.

Edward Boutle, aged 46, was admitted into the hospital about nine o'clock on Sunday evening, June 10th, labouring under symptoms of hydrophobia. He was a man of intemperate habits, living in Wapping, and kept a favourite tame fox, with which he was accustomed to play. About six weeks ago the animal appeared unwell, and bit him slightly in the palm and back of his right hand. He took little notice of the bite, which got well in a short time; the animal, however, died two days afterwards, for which he was unable to account, but suspected that it had been poisoned. Saturday afternoon, June 9th, he complained of feeling dull and heavy, and of an aching pain in his fingers, which afterwards extended up the arm, and he felt an uneasiness just above the inner condyle and in the axilla. During the night he was restless and unable to sleep; the following morning (Sunday), in attempting to take a cup of tea, after swallowing about half he threw it from him, and complained of its choking him. A constriction and uneasiness about the throat came on; he was unwilling to swallow any thing; and being seen by a medical man, he was sent to the hospital in the evening.

When admitted his look was wild, his respiration difficult, and he could not bear any one to approach him. He ran about the garden, and had a great horror of the warm air of the hospital. He was taken into

one of the wards at a window, as he refused to pass through the passages; he was afraid of the slightest breath of wind, as when the door of the room was opened, or a person breathing upon him. His pulse was 98. Upon a glass of water being presented to him, he was thrown into a violent paroxysm. He complained of the uneasiness in his arm and pain in the shoulder. Ten grains of the carbonate of ammonia with one grain of opium, in pills, were proposed to be given him every hour, but he dared not attempt to swallow them. An enema containing laudanum was ordered to be administered as soon as he should consent to it. A blister was applied in the course of the spine.

He passed a very distressing night; the paroxysms became more frequent and violent; he would lie down, and in about a minute start from the bed in painful convulsions, and continue walking about the room. He was perfectly sensible, and would talk; he said he did not think that his present state had any thing to do with the bite, but he believed that he should not recover.

June 11th, 9 A.M.—He was now quite furious, rolling upon the floor and bed, threatening to destroy himself, and it was with difficulty that five men could restrain him. He was spitting his saliva violently from his mouth, and was in the greatest possible distress. He now consented to have an enema. An ounce of laudanum in some gruel was thrown up, and retained; in about an hour afterwards, an enema, with ℥ij. of laudanum was given, and in half an hour was repeated. After the clyster he became more composed and quiet. He had afterwards two enemas, containing four drachms each.

12 o'clock.—An ointment, consisting of two ounces of lard with two drachms of acetate of morphia, was rubbed upon the spine, but appeared rather to aggravate the spasms. He was now becoming weaker; the paroxysms were frequent, but less violent; he was delirious, and did not know his wife. Pulse 160. From this time he gradually got worse, and died quite exhausted a little after two o'clock, about forty-three hours after the symptoms first appeared.

Examination of the body twenty-four hours after death.—There was serous effusion between the arachnoid membrane and pia mater; the substance of the brain was increased in vascularity; there was also a considerable quantity of serum under the theca vertebralis. The superior cervical ganglion of the sympathetic, the eighth pair of nerves and phrenic nerves, were perfectly healthy in their appearance. The nerves were minutely traced to that part of the skin which was the seat of the bite, but nothing unusual was observed. The mucous membrane of the pharynx and larynx was remarkably vascular. The lungs were healthy, but rather gorged with blood.

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

June 1832.	THERMOMETER.		BAROMETER.	
Thursday . 7	from 43 to	59	29·49 to	29·51
Friday. . . 8	40	62	29·57	29·69
Saturday . 9	44	65	29·76	29.70
Sunday . . 10	40	63	29·75	29.83
Monday. . 11	41	67	29·81	29·70
Tuesday . 12	43	69	29·60	29·46
Wednesday 13	54	70	29·46	29·49

Prevailing winds S.W. and S.E.

Except the 13th, cloudy, with rain every day; thunder at times on the 7th, 9th, and 10th.

Rain fallen, 1 inch and ·6 of an inch.

CHARLES HENRY ADAMS.

DR. ELLIOTSON'S LECTURES.

THE publication of Dr. Elliotson's Lectures is suspended for a short time, at that gentleman's request. Dr. Elliotson informs us that certain engagements render it impossible for him, *at the present moment*, to revise the proofs for the Gazette; and we have thought it better that a little delay should take place, rather than deprive our readers of the advantage derived from the corrections of the learned lecturer.

IMPORTANT PRESENTATIONS AT COURT.

AT the Levee, on Wednesday, Sir Charles Aldis presented his book on Diseased Glands and the Use of Mercury! At the Drawing-room, on Thursday, Sir Charles Aldis presented his son, Dr. Aldis!!

NOTICES.

Dr. F. will perceive that his question —as to whether saline injections had been tried in London?—is fully answered by the contents of our last two or three numbers. He states that he recommended a trial of this method when in London, in March; but was prevented, "by some unlucky obstacle," from putting it in practice. Dr. F. advises the simultaneous employment of a galvanic aura, arising from 60 to 120, or more, four-inch plates.

"Y. P."—The treatment adopted seems to have been so judicious as to leave but little more to be done;—if the digestive system could be got into better order, it might be worth while to try carbonate of iron in large doses.

ERRATA.

In Mr. Gaskoin's paper, in our last No. p. 317, in the second column, line 14, *after* "for twenty minutes," *insert* "night and morning."

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 23, 1832.

CROONIAN LECTURES,

Delivered at the Royal College of Physicians,

By Dr. Roupell,

May 1832.

Lecture III.

On the Treatment of Disease.

In completing the sketch which I proposed to make of pathology, I come now to the consideration of that branch of my subject which has reference to the treatment of disease.

It is a matter of dissatisfaction to reflect upon the uncertainty of many of the grounds which regulate our rules of practice, and to think how little is known of the actual mode of operation by which the benefit derived from the administration of our remedies intrinsically depends.

The first step towards attaining this end is obviously to ascertain the exact nature of the complaint we may be required to oppose; to know accurately how to form our diagnosis; to fix not only the seat, but the nature and extent of the injury, previously to attempting its removal. Self-evident as this proposition may appear, and to require no discussion or proof, still we see its necessity denied, and the utility of the actual knowledge of disease questioned even by practitioners of undoubted talent. We would not certainly withhold a remedy because we knew not the processes by which it restrained excited actions, and because we employed it *empirically;* but we surely regret at the time that we resort to such a means of cure that we cannot explain the principle upon which we proceed, or the actions which thus we may be able to restrain.

Experience proves certainly that the treatment least capable of justification in the knowledge of disease is sometimes crowned with success, and that the treatment opposed directly to the apparent pathological state—that a course least authorized by our knowledge of the state of parts—may in some cases be sufficient to lead to a successful termination of a case; and that the symptoms may in some complaints be adequate guides; but a very brief inquiry into the causes and origin of symptoms will at once convince us how uncertain are the signs which they afford, and what nice discrimination is required to estimate their value; to enable us to disregard urgent and prominent signs; to lay our stress upon comparatively trifling indications. Experience in a variety of complaints has now satisfied us that many parts may be essentially and seriously disordered, yet the symptoms which may present themselves would lead us far from the right object of our inquiry, and examinations not unfrequently develop extensive disease not suspected even during life.

Upon what base, then, must our proceeding be directed—our course in ascertaining disease be founded—if we are deprived of the more positive indications; if we are thus restrained from drawing our conclusions from outward marks? It is to the *functions* of the different organs that we are to look for the proper explanation of disorders, and must study in the local or general phenomena of the uses of parts the real seats of derangement. But not only is it important to ascertain the actual *seat* of disease—its nature should be ascertained: this we can only learn by pathological anatomy, the use of which has been denied.

The various theories of disease have influenced very materially the treatment which has been at different periods adopted, and at the present day the same disease in the hands of different practitioners might probably be variously treated. The idea of all disorders originating from, and being maintained by, an increase in power and activity of the vessels of one part, and being dependant upon an increase of strength, leads obviously to the employment of one mode of practice—that, simply, of depletion. The opposite notions of Dr. Browne, whose theory attributed all to debility (but which at the pre-

sent day meets with but few supporters), influenced even more materially and injudiciously the regulation; while the fact that such opposite systems should for a time prevail, would shew that the seat and actual nature of disease was but imperfectly known or understood; and the feeling of the present day, to consider effect simply, without yet arranging diseases under any one decided order, proves the conviction of previously prevailing notions to be unsatisfactory in their explanation.

The observation that during the continuance of one malady another seldom appears, and that on the accession of a second disorder a primary one would for a time cease, led to the idea that the action of one was incompatible with that of another—that two could not simultaneously exist; and that the impressions induced by some specific disorders remain for a time in the system, has conduced to the most brilliant discovery of moderating a more serious disorder by the action of a less, and affords us the means of accounting for the action of some of our most powerful remedies; not that simple excitement is sufficient entirely to account for the destruction of certain poisons in the system, as mercury is supposed to destroy the syphilitic virus by simple excitement; this poison does not appear to be thus destroyed: (a simple fever would not prevent the action of syphilis:) some actual change in the state of the fluids, inconsistent with its presence or activity, must be induced.

Among the theories of the present day may be noticed that of Dr. Hahneman, which, disregarding all established modes of treatment, opposing all accustomed practice, of correcting fluxion by astringents, or irritation by sedatives, administers those remedies as a cure for the disease exactly calculated to produce it, and by this augmentation of the original affection considers that a state is induced more favourable to the subsidence of the first—a doctrine, however, which assumes that the medicines act in the same way, whether in large or small doses, and that the action of these is the same, both during a healthy and disordered state—propositions, both of which are sufficiently controverted, and militate against this mode of explaining the effects of Dr. Hahnemann's administration of remedies, if, indeed, it produces any effect at all. The other theory, which directs us to look to individual systems and organs to ascertain their exact state, and to combat by such means as experience has pointed out the local affection, is the most valuable—is the right mode of directing our means of remedying, as well as in ascertaining the nature of the ailment. Therapeutics, then, strictly speaking, is the art of modifying the intimate action of organs. This can only be securely based upon the knowledge of the laws which regulate and excite these processes.

If the study of these laws which preside over the *theory* of disease presents us with difficulties, the appreciation of the right modes of treatment is not the less matter of perplexity. The knowledge, however, of the processes of disease which pathological anatomy teaches us, and the views which I have been induced to adopt of the nature of the changes to which the different parts of our system are subject, and the means by which we ascend to the origin of morbid alterations, is at least in this one view important—that it points out the proper direction of our remedies; and, furthermore, is calculated to afford us, when completely investigated, the means of accounting for the effects we know to result from the exhibition of medicinal substances, and eventually to lead us to the scientific appropriation of the various principles of medicines.

We are taught by this view to direct our treatment to each individual organ, to supply such substances as may be deficient in the system, and to correct others injuriously prevailing. In the consideration of specific diseases we learn that the certain course of some cannot be opposed successfully by any attempts at checking their progress; and in other diseases (at present incurable) we know how to avoid *increasing* the evil, while we may entertain a rational hope eventually to be able to overcome them by means as yet undiscovered.

We must admit that our therapeutic means are founded upon *experience*, the results of which are handed down to us from the ancient physicians.

We may certainly *reason* upon these grounds for particular modes of practice, and question the propriety of the methods employed; but our reasoning must not be allowed to militate against facts. Founding, then, our treatment on experience, we learn by analogy, in similar cases to direct a corresponding procedure; and we know too little as yet of the analysis of the fluids, or their state at various periods, to attempt by chemistry the sole prevention of disease. When the means by which the natural restoration to health from disease, and the resources which nature has herself at command, are well directed, it must be our object to allow them to take place; but when we perceive that the powers of the organs are unable themselves to restore the proper equilibrium, then to interpose our art, and to administer our remedies. The chemical theories have till now furnished us with uncertain indications; but in some instances we find them firmly established, and capable of directing the appropriate remedy and treatment of disease.

The practice of our art is founded upon certain known and acknowledged

truths, which time has sanctioned, and we direct our proceedings by the acquired knowledge of the actual nature of the disease, its form, and character; we are guided by the degree of strength of the patient, the intensity of the disease, its date, the symptoms, its seat, or its complications. Of these conditions, the character of the disease—its type, is the most important; and we know thus, to bleed in inflammations—to administer mercury in various forms of syphilitic affections—to administer bark in intermittents—and, when organs have been injured, to enjoin rest, and to return displaced parts to their natural position. How but by pathological anatomy can we know the exact nature of complaints, the parts they involve, and be satisfied of their essential nature? The character of a disease may, however, be itself inflammatory, and yet, under ordinary circumstances, may not require active depletion, some having a natural tendency to go through a favourable series of changes; thus we see little active treatment necessary in many specific diseases, as small-pox, measles, erysipelas, and others. An intermittent type, if clearly marked, affords us useful and valuable indications; and in this periodical return, rather than in the actual nature of the symptoms, we often learn to direct a treatment. Headache, diarrhœa, and a variety of other symptoms, have thus yielded to the sulphate of quinine, and epileptic attacks, assuming a regular period for accession, have ceased on the employment of this remedy. The degree of power in the constitution must always be weighed in the consideration of our treatment; and these vary in every individual; indeed, on this has been founded a division into sthenic or asthenic—active and passive states. This is, indeed, one of the most important points to be decided, and which most influences our minds in the course to be adopted.

It becomes, then, a matter of some importance to ascertain the signs of strength: the individual endowed with the greatest strength, according to the ordinary acceptation of the term, will by no means always be the one usually so accounted in our point of view. Muscular power is strength, but we rather look to the power of bearing depletion; and clearly those of the greatest muscular development are far from being those who require, or bear, the largest bleedings, or most active treatment. It were superfluous to specify the external indications of strength or weakness, or to mention the signs by which we may distinguish real from apparent debility; these signs will principally be furnished by our knowledge of the nature of the disease, and the history. When we have ascertained an organ, situated within the body, to be the seat of inflammation, we are not deceived by the feebleness of the pulse from active measures; we know that a weak and irregular pulse may be present in inflammation of the brain; or that there may be no force in the pulsation at the wrist in gastritis, or inflamed liver: we may appreciate also real or apparent strength by the action of remedial means, the rising of the pulse under bleeding, or the greater debility from stimulants, which, under other circumstances, would, for a time at least, give additional power. This point, however, is one far from being clearly definable, but receives its most real elucidation by the knowledge of the complaint, and the history of the disease. The heat of skin—the colour of the cheeks—the power at times of muscular movements—the colour of the urine, whether pale or red—have been in turn assumed as indices, but afford us but uncertain criteria; and it must be confessed, that experience alone can give the knowledge which should guide us to administer stimulants in some complaints, as fever—and undoubtedly at some periods of this state stimulants are beneficial,—or when to urge still further the necessary depletions.

The injury which had been observed to result from loss of blood late in diseases, no doubt gave rise to the rule laid down by Hippocrates, that after a week, bleeding should not be practised — a rule advantageously every day departed from; but it is obvious that caution is requisite on this point, and that the consideration of the duration of the complaint will influence materially our employment of debilitating remedies.

The *complications* of diseases occasionally embarrass us; and we feel much difficulty in some circumstances where opposite modes of treatment are indicated: thus in syphilis combined with scurvy, the administration of mercury would be fatal; and we find in some cases individual peculiarities, which render persons susceptible to disease, the remedies for which their constitutions are unable to bear.

The effect of the state of the atmosphere on the body at particular seasons, influences materially our treatment of disease: the well-known fact is exemplified in the difference in a large town and in the country; and during some years the particular character impressed on the usual disorders confirms the truth of the statement: an accurate observance of the progress of any one complaint, which may generally or epidemically prevail, will be found essentially to vary at different periods during its prevalence.

I shall now proceed to make some mention of the means we possess generally of combating disease.

Therapeutics include all the means which can be employed to arrest disease—to prevent the proclivity to disorder; and includes all the regulations respecting air, exercise,

regimen, as well as the administration of remedies; and has thus led to the division into prophylactics and therapeutics.

Prophylactics, or the means by which the health is maintained, embrace the regulation of diet, are very valuable assistants in the cure of disease, and are, in fact, in many cases, the chief means employed; and by regulating all those sources of excitement which our accustomed *habits* in *health* would occasion in disease, we allow the system to restore the balance of its power, when the disease is slight and its tendency is to recovery.

The principal differences in alimentary substances is the quantity of nitrogen and carbon in their composition, both in addition containing oxygen and hydrogen; by regulating these substances we have it in our power to provoke or diminish the activity of the system, and to alter not only the quantity, but the quality of the chyle.

The rigid enforcement, however, of severe rules of diet, and the strict abstinence enforced by the French physicians, I felt assured was productive of serious injury, especially at the Hospital for Children, their rapid digestion requiring support even during ailments of an active inflammatory character, so that strict allusion to rules of diet is not so absolutely necessary for them. An entire abstinence, positively no food at all, was insisted on in former days; six days' fasting was in some diseases enjoined: this was a great measure of combatting disease among the ancients, but the active measures we now can adopt render such modes of reducing strength less necessary.

But although the necessity for enjoining caution with regard to food is obvious, experience shews that too rigid an attention to this point may be productive even of injury: in the periods of complaint, when a stimulating diet would be injurious, the stomach naturally rejects it, and the repugnance to the stronger and more alkalescent kinds is insuperable; but towards the close of fever, and even during the continuance of the slighter attacks, the rigid interdiction of even the lighter kinds of animal food is not absolutely essential. In chronic complaints, the attention to a plan of diet is of the greatest importance, and gout and phthisis may be materially benefited by attention to these particulars; indeed, an early attention to the mode of life may alter entirely, especially if aided by climate, the proclivity to these diseases. Diets are divided into cooling, relaxing, and tonic; substances of varied *chemical* composition are assigned to each of these, and would point out the means by which they effect the alteration in the system: the cooling diet is composed of acids; the relaxing contains mucilaginous substances; and the tonic or strengthening those matters in the highest degree azotized.

By attention to the regulation of alimentary substances, we may diminish essentially the formation of animal heat, and influence by the ingesta the chemical processes which take place in our frame.

Of the mode of action of our therapeutical measures, much doubt at the present day exists; we observe their effects, and class them accordingly, but little is known of the means by which these effects are produced: we have evacuants, astringents, tonics, stimulants, sedatives, and specific remedies: the action of these again is but relative, stimulants proving in some cases sedatives, and our sedatives are capable of occasioning the highest state of excitement. The system of plants does not afford us much assistance in the investigation of disease, although no doubt the circulation of injurious fluids produces some of the disorders to which they are subject, while a predisposition—an hereditary state—appears in some to continue in all descendants from particular stocks, and leads to the eventual extinction of certain species. The observation of the effects of poisons on the class of vegetables presents us with some interesting and curious facts, clearly proving, that by admixture with the circulating liquids, an injurious effect in their minuter vessels may be the result. One source of active injury to them is shewn in the animalculæ, noticed by Dr. Farre, in their circulating fluids. Of this mode of inducing disease in our own frame we are not assured; though in some animals, as in sheep, we see the presence of such beings in the fluids contained in the vessels of the liver.

I have already alluded to the extreme sensibility of plants to the agency of muriatic and sulphuretted hydrogen gases, and mentioned the minute portion of arsenic which was capable of producing injury to them, and may observe that the experiments of Mr. Marcet shew the deleterious nature of many substances on the organization of plants, which are known to be capable of exerting similarly hurtful impressions upon our own; to account for these effects, a power similar to the nervous system in man has been presumed; but it seems more rational to refer the injurious effects which result to the combination of these substances with the circulating fluids, than to create a separate system, of which we see no proofs, to account for purely physical changes. May we not more rationally conclude, that the injurious effects exerted by these substances on our own frames are owing to combinations in the system which we possess corresponding to the one affected in plants, and that the indications which the nervous system presents are the *impressions* exerted by the fluids thus changed on their extremities of the nerves, or by the impaired or over excited activity of the state necessary to, or which occasions, *sensation?* The different effects of poisonous agents on

plants shew a difference in their mode of action: irritants destroyed them, by withering and drying the leaf, combining with the moisture, and interfering with the respiration; narcotics occasioned the drooping of the leaves, by destroying the power of the foot-stalk; a small quantity of prussic acid applied to the sensitive plant impaired its sensibility; and nux vomica, opium, belladonna, cocculus indicus, and other substances deleterious to animal life, have been proved equally destructive to the vegetable kingdom.

Should the theory, which I mentioned in my last lecture, of the possible formation of the secretions, prove true, and that the excretory organs are so composed as to allow such particular secretions to pass their vessels, and eventually be discharged, we might find in a similar provision the explanation of the acknowledged and obvious fact, that particular substances affect different viscera, as we know cantharides to act specifically on the urinary and genital organs; nux vomica to induce an injurious effect on the spine, or on the nerves which are derived from that source; while the infusion of tobacco influences the nerves which supply the heart, destroying the irritability of its muscular fibre. Any ideas, however, on this subject are yet far from a satisfactory explanation; but we know that poisonous substances do enter the blood; that some produce no injury without they are mingled with that fluid; that the circumstances which accelerate or retard their progress through the vessels, quicken or impede their action; and, judging by analogy, from the effects observed in plants we may hope to be able eventually to explain the means by which they produce their effects; to ascertain the physiology of the vascular system, and be justified in looking to changes in the circulating medium, for the *cure*, as well as the *origin*, of disease. That centuries yet to come may find mankind but little practically advanced, is possible, when we consider how much thought has been expended on a subject, which, till now, has been but little advanced; yet in the present state of chemical knowledge our prospects of advancing in this inquiry are daily and hourly increasing.

In the administration of remedies, *a circumstance* which modifies their action, both curious and not yet sufficiently elucidated, is the different effect occasioned by the form and quantity of these substances: the contrary operation of large and small doses of calomel affords common illustration; and is also shown in the enormous doses of tartarized antimony which have been given with comparatively little effect: a difference of the power of absorption we must presume to be the cause. For the reason of the different agencies of different medicines; of the actual power by which some pass through the vessels, but without exciting local action, while others occasion the most violent inflammation, or active effusion of their fluids, to what can we look but to the chemical composition, and the combination they may form with the fluids in the alimentary canal? The extraordinary accounts of the minute doses of the homœopathic physicians, to a certain degree is capable of explanation, inasmuch as we see the most deadly poisons, and some of those most readily absorbed, remain for hours without exciting any active symptoms, if in a state unfavourable for the passage of them through the absorbing vessels: the minute quantities of some animal and vegetable matters, capable of affecting our sensations, is shewn in odoriferous substances, when the real quantity given off must be less than the minutest dose administered by Dr. Hahneman: how infinitely small must the quantity of musk really be inhaled by a single inspiration, and capable of producing distressing effects, and even fainting, when we hear that a portion of musk left for twenty years, and giving off during the whole of that time abundant exhalations, is calculated to have undergone division into three hundred and twenty quadrillions of particles; and a mass of assafœtida exposed to the air for seven years, and filling the surrounding atmosphere with its effluvia, was ascertained to have lost but a single grain.

That, to a certain degree, substances become more readily admissible into the vessels, and consequently more active, cannot be denied; and by the actual mechanical obstruction of the larger particles of some substances, when introduced into the circulation, we find a mode of exciting disease; tubercles having been thus formed when mercury was introduced into the vessels, and M. Magendie occasioned inflammation of the lungs, by mixing oily substances with the blood.

To make some mention of the means by which we have it in our power to control morbid actions, as is well known, we possess very extensive means of acting on, exciting, or depressing, the various functions.

Of these means, the antiphlogistic are the most important, powerful, and best understood in their application. In the abstraction of blood and diminution of its fibrine, by a variety of evacuants, and diluting with watery substances, we can ably combat inflammation.

The signs by which bleeding is considered necessary, and its repetition called for, are well known; and also that the same signs which indicate its *necessity* in the one instance, are proofs in the other of its over employment. But on this subject I shall not enlarge.

The exact means by which our tonic remedies act, how the peculiar bracing effects are produced, is yet to be sought for. We

have evidence now of the introduction of these substances into the system, and we have to balance the *probability* of their local action on the parts with which they first come in contact, and the constitutional sympathetic action, against their absorption and effect by universal contact. Is the tissue with which the tonic comes immediately in contact, the part alone affected? Do the bitter substances confine their action solely to the mucous membrane of the stomach and intestines? Is the connexion between the stomach and the other parts so entirely united as to enable us to receive implicitly the explanation generally given, that the tissue of the stomach and œsophagus is rendered more dense and firm, and that the ulterior effect results from this impression?

The effect of stimulating remedies on the system is very marked, and certainly, in some cases, clearly proves the sympathetic action of the brain with the stomach, by the sudden insensibility which ensues on the injection of a large quantity of alcohol, and the return of consciousness on its removal; but this substance also proves that, carried into the system, it occasions more or less general excitement.

The administration of our remedial means is usually to meet indications by the specific action of remedies; and one advantage, at least, of Dr. Hahneman's theory will be to investigate the effects of medicines on the frame which are infinitely diversified; but we employ those chiefly in medicine whose action is marked and decided.

Some are capable of producing a specific action on the digestive organs; exciting vomiting, or restraining the active secretions from the bowels.

Other medicines exert their influence on the heart, amongst which is digitalis, a medicine of the highest value, though much is to be desired as to its proper administration.

But the action of the greatest number of medicinal substances act specially on the various secerning organs; thus the excited action of the kidneys and alimentary canal, and skin—thus shewing a special determination and an interference with the action of the minuter vessels, which then afford them the means of escape, or, by modifying the changes thus performed, give rise to the symptoms of irritation.

In considering the therapeutical agents which act upon the absorbent system, the efficacy of several is known; and we feel the assurance that chronic disease is not beyond the assistance of art, and the belief that many substances of great efficacy are yet within the reach of discovery. Many substances act upon the brain—disturb the functions of the cerebrum; various stimulants excite, and narcotics, again, produce a contrary effect; but how this effect is induced, or why opposite results occasionally occur—why a sedative to one is to another the highest excitant—we know not. The distended state of the vessels of the head shews their actual condition; but through what influence this effect is produced, we are yet in doubt. The experiments of Dr. Macartney prove that the presence of oil of tobacco in the brain did not occasion the symptoms of poisoning; which were, however, instantaneous on the application of this substance to the tongue, referring to the minuter extremities the recipient power of the nervous system; and on the nervous extremities, by means of the circulating fluids, I imagine these effects are to be accounted for.

That prussic acid acts in this manner, appears more rational, I conceive, than by a simple impression on the extremities of the nerves; especially seeing that this substance produces no effect when actually in contact with the brain, that its action is prevented by tying the vessels of the part to which it may be applied, and that the division of the nerves has not interfered with its action.

With these observations I shall conclude the subject I have chosen for these lectures—one much involved in doubt and obscurity. I have viewed the changes of structure which result from disease, as the same processes by which nutrition is performed; I have considered and stated my reasons for believing that many of these owe their origin to the state of the fluids contained in the vessels themselves; and have expressed my belief that in actions thus disturbed of the solids and fluids some injuries to the system are occasioned, and the correction of others effected, but at the same time feel my inability to do justice to the subject, and am fully aware of the authority which I thus have ventured to oppose; I entertain also a deep sense of the indulgence which I have met with, and the attention with which I have been heard, while thus declaring an individual opinion.

MEDICAL EVIDENCE

IN

A TRIAL FOR CHILD-MURDER,

At the last Warwickshire Assizes.

FROM THE NOTES OF PROFESSOR AMOS.

Rex v. *Eliza Maria Jones.*

THE prisoner was committed on a Coroner's warrant, and stood indicted for murder. She had been delivered of a child in a privy; concealed the fact; and, upon discovery, affirmed that the occurrence was accidental. The following was the principal evidence given at the trial.

John Mouchet Baynham, examined by Mr. Amos.—I am a surgeon. I saw a dead child on the 21st of October last, at about eleven

o'clock in the morning; I saw it in an out-house at Mrs. Spinks's. The colour of the skin was generally green, with the exception of the face and the scalp, which had a remarkably fresh appearance; it had been washed. I have a reason now to suppose that the green was the result of putrefaction; at the time I entertained no distinct opinion. The commencement of putrefaction would depend on many circumstances; a material difference is observed in the period of putrefaction of different parts,—the face and scalp not putrefying as early as other parts. I think the labour had been quick, because of the absence of the tumefaction of the scalp which is observed in protracted cases. In an ordinary labour I do not think the appearances would have been the same as in this case; I have never seen the scalp in a less state of tumefaction. The navel-string had not been tied; its length was about five inches; I am of opinion it had been cut; I form that opinion after a very careful examination of the divided surface: if the navel string had been broken, in all probability it would have broken at one of its extremities, and generally at that farthest from the child; from that circumstance, and the state of the divided surface, I think it was not broken. The state of the lungs induced me to entertain the opinion that the child had been born alive; I was satisfied that they had been inflated, by the appearance of them. I tried whether they would float on water; they were unusually fresh; they floated in water; they were somewhat of a pink colour, with white edges. My opinion of the floating of the lungs is, that it may be taken, when there are no symptoms of putrefaction, as a test that the child has breathed. If the child had breathed when its head was out of the mother, it is not probable that the child would have been born dead when the birth was complete. I examined a vessel which is closely connected with the heart; it is called the arterial duct; it was not closed, it was somewhat contracted, which is not seen before birth—it was corrugated; it is the duct through which the circulation is carried on before birth. I observed also contraction of the bladder, which did not contain a tea-spoonful of urine. From all these appearances I am of opinion that the child was born alive. It is very common for women on the point of delivery to feel a desire to go to stool. Consistently with all appearances the child might have died from suffocation in the soil.

By the Judge.—I examined the child on the afternoon of the 21st of October; I first saw the child on the morning of that day. Supposing the child to have been born on the morning of the 21st, I cannot account for the appearance of putrefaction; it was not the appearance which a still-born child would have; in a still-born child the discolouration would not be of the same kind, and there would be separation of the outer skin.

Dr. Darwall examined by Mr. Reynolds.—I am a physician. I saw the child at a public-house, I believe in Litchfield-Street, in the morning; I have heard Mr. Baynham's evidence; I concur in the general appearances described; I draw the conclusion from those appearances that the child was born alive; I can conceive no other cause of those appearances of discolouration than putrefaction. I cannot account for the appearance of putrefaction so soon; the discolouration would not be peculiar to a still-born child; I should have been able to draw no conclusion as to the child being born alive or dead from that discolouration alone; I can merely conjecture that some of the gases evolved in the privy might have produced such a discolouration.

Cross-examined by Mr. Humfry.—I think the child was born alive, but that its respiration had been imperfect. Putrefaction, even incipient, would have rendered every other test more uncertain. The right lung was more inflated than the left; a partial respiration might take place before the body was born—even before the head was completely excluded. Supposing the child to have lived at all, it must have lived for a very short time—as short as a second—as long as one or two respirations at the very outside. The bladder being empty is not an infallible test that the child had been born alive. If one or two respirations had taken place, it might have followed that no more might have taken place before the child was completely born. After breathing once or twice, I conceive that it might happen that the child might fall upon the ground and be suffocated by the natural discharges, or by the efforts of the mother to complete her delivery. Supposing the child to have been born while the mother was on the privy, it would be more likely to be destroyed by falling into the place, than with the natural discharges. If it fell upon the ground, it might be suffocated in the natural discharges. This would not be a solitary case of delivery taking place while in the position of going to stool. There were no marks of violence on the child; it is consistent with every thing I saw, that the child, after breathing once or twice, might have fallen on the ground and been suffocated, and the navel-string might have been afterwards divided. I think the navel-string was not broken by the fall of the child, but by a cutting instrument, and this either before the falling into the privy, or afterwards. There are instances of the child falling into a privy and being suffocated, without there being the slightest reason to criminate the mother; there are several instances.

Re-examined.—There is always some discharge at the delivery. If the woman

had been delivered on the floor, there would have been some discharge on the floor. When sitting down on the privy, I think a woman could not have been delivered, and cut at the time the navel-string. In order for a woman to be delivered on the privy, the pelvis must have been large in proportion to the head of the child. The child was full-grown; there was nothing particular in the size of the head; I did not examine the person of the prisoner. My conclusion, from what I saw of the child, is that it had had one or two gasps; it might have lived more than a second. The lungs were cut; there was crepitation, which would be the case from natural air, but not from putrefaction. I think that there would be crepitation if air had been forced into the lungs. It would depend very much on the density of the stuff into which the child fell, whether or not it would breathe in that situation. I do not think it would necessarily follow that there should be any mark on the neck, if the mother had pressed it so much as to occasion death.

Mr. Baynham, by the Judge.—The length of navel-string varies very much—from twelve to thirty-five inches; the usual length is eighteen inches.

Mr. Ingleby examined.—I am a surgeon. On the 22d I was called by Mr. Doughty to see the prisoner. I examined her person; she had all the marks of having been recently delivered. There was milk in the breasts. I think the delivery was rapid, on account of a very great laceration; I think she had not been delivered more than twenty-four hours. She stated that she was quite innocent of the charge, and was willing to be examined.

By the Judge—There was a very extensive laceration of the parts.

Epinetus Spinks examined.—I live in Lancaster-Street. On Thursday morning, the 20th of October, I saw the prisoner about twenty minutes past six; she came out of a back door of Mary Page's house. She was dressed. She made a moaning or groaning noise. She went towards the privy. I cannot say whether or not she went into the privy, on account of a brewhouse. I did not see her come back. I saw her again about ten minutes or a quarter of an hour afterwards; she was going towards the privy; she had a lighted candle and a mop. At eight o'clock that night I searched the privy; I found a child in the night-soil under the seat; it appeared to be with its face downwards; it was over head; there was a large quantity of the soil that stuck to the top of its head when I drew it out. I put the child on the floor, and it was washed. Mr. Baynham had it next morning.

Mrs. Page examined.—The prisoner came to my house on the 15th of October. She said she had been unwell for eleven months. She said her legs and feet had been swelled. She said she should want to stay a fortnight, as she should be able to go to service in a fortnight.

The prisoner was *acquitted* of the capital offence, as well as, subsequently, of the charge of concealment of the birth.

VENOUS PULSATION—SINGULAR EFFECT OF CASTOR OIL.

To the Editor of the London Medical Gazette.

Wolverhampton, June 14, 1832.

SIR,

HAVING never met with an account of a similar phenomenon, and considering it confirmatory of Harvey's opinion, that the capillary and venous circulations are produced by the *vis à tergo* of the heart, I send you the following note of a case for publication in the Gazette, should you esteem it worthy of that honour.

E. B., æt. 30, was admitted a patient at the Wolverhampton Dispensary, January 17th, 1832. She had been unwell for more than a week with feverish feelings, lowness of spirits, cough, and flying pains in different parts of her body. Three days ago she took a strong purgative, which has weakened her very greatly, without any relief. She was seen by the house-surgeon, and bled; blood neither cupped nor buffed. I found her the next day with a flushed and anxious face; tongue furred, dry, and aphthous at its edges; bowels open; urine high-coloured; shooting pains in hypogastrium, which was tender to the touch, and also over the chest and upper part of the abdomen; nausea and flatulence; cough troublesome; expectoration viscid, aerated, reddish, containing some pure blood. Chest sounded well on percussion; respiration puerile, very laborious and rapid; pulse frequent, of moderate strength. She is in the fifth month of her second pregnancy, having had a previous miscarriage. Considering her case to be one of fever, with incipient pneumonia in both lungs, I ordered fifteen leeches to the chest, and saline draughts, with opium. The leech-bites bled so profusely during the night as to reduce her to the greatest

state of exhaustion; and the bleeding was only at last arrested at some of the orifices by the aid of hot knitting-pins. The blood as it oozed was very thin and bright-coloured; she still had some cough, but all her pain was gone. Her strength was recruited by cordials and nourishment, but she miscarried on the 21st, though without much loss of blood.

I did not see her again till the 24th, when she was sitting up, and I observed that the veins on the back of her hands, which were greatly distended, pulsated with great violence. The skin being very pellucid, the pulsation was very distinct in the small venous ramifications of the fingers, in which the blood was of the arterial colour; the larger veins being darker, but not so blue as usual. The pulsation extended beyond the middle of the fore-arm, and was not stopped by pressure upon the veins at any point above, though it was by pressure below the pulsating parts, whether on the fingers or backs of the hands. It was contemporaneous with the real pulse, which was hard and rapid, and not very compressible. It continued more or less marked for three days, and ceased on the evening of the 26th, when the pulse also lost its force and velocity. A few days afterwards her legs became œdematous, but this symptom soon subsided, leaving her only troubled with a cough, which she lost in a few weeks, and she is now quite well.

I can only attribute the venous pulsation in this case to the excessive reaction of the heart, pushing the thin and impoverished blood through the capillaries straight on into the veins; the impelling force of the heart being more than enough to overcome the retarding power of the capillaries.

Since the foregoing, I have never met with a case of exhaustion from loss of blood followed by such violent reaction; but I should expect that medical men, engaged in the obstetric branch of the profession, would often meet with similar cases.

It is perhaps not unworthy of remark, that when this patient takes castor oil it does not act as a purgative, but exudes from every part of her body.—I remain, sir,

Your obedient servant,

T. Ogier Ward, M.B. Oxon.

MIDWIFERY.

To the Editor of the London Medical Gazette.

Sir,

To relieve the tedium of waiting the issue of a protracted case of midwifery, I take leave briefly to notice the subject of a paper in your Gazette of Saturday last, "on a peculiar cause of laceration of the perineum," in which the author states that he has not found mentioned by authors a similar case to that which has recently fell under his notice.

Truly such a state of things was not likely to have been made the subject of an *extraordinary* communication, though it has no doubt been a matter of consideration, and duly explained in lectures, being by no means an unfrequent occurrence. Nevertheless, it must be confessed, Dr. M.'s paper may prove valuable to the junior practitioners in the obstetric art, and would form a very appropriate portion of a midwifery lecture, though necessarily with some modifications in the representations of the supposed serious results from a continuance of the obstruction described.

The case mentioned was certainly an extreme one, taking into consideration the tumefied state of the scalp, and the consequent impression on the integuments by the contracted vaginal aperture; but I submit that the extenuated, or thin margin, was rather favourable than not, because the fissure would probably have been to no great extent if a laceration had taken place.

In such a state of the aperture, there would have been great difficulty, or, perhaps, an impossibility of applying the forceps, but the vectis, perhaps, might have been used, had the state of the patient rendered an operation necessary, as regarded exhaustion or cerebral affection; at all events, I think the *necessity of craniotomy* would not have suggested itself to the minds of many practitioners, and assuredly the life of the child could hardly have been considered in jeopardy in this case. The doctor does not state precisely the length of time the pressure existed, or the previous state of the vaginal cavity, as regarded the secretions; whether the parts were heated,

and consequently dry, rendering them, of course, less yielding.

I must now be allowed to state a *rather* more uncommon state of the vaginal aperture than that from contraction or extenuation, occurring, no doubt, to others, and which perhaps may be mentioned by authors. To render the description clear, it is necessary to make a comparison, and that is, of the vaginal margin to the hem of linen cloth, through which a small cord has been drawn. This state of parts I have repeatedly met with, and it has appeared to have been the chief cause of protracted expulsion; and in one instance the apparent cause of laceration of the perineum.

There are cases, however, I am fully persuaded, where no care or precaution could have prevented laceration, though a proper support would no doubt prevent an extension of the laceration. As a trifling adjunct to Dr. M.'s paper, I beg to suggest, what, perhaps, my junior brethren have all taken into consideration before, that they should interfere as little as possible by examinations during *first* labours, and likewise forego the repeated applications of *dry* napkins to the perineum, and not allow their hot hands to be *continually* applied to the distended perineum.

Dr. M. no doubt, would have bled his patient, and given opium, had not the expulsion of the head taken place, and had the excessive irritability of his patient, with other urgent symptoms, continued.—I am, sir,

Yours respectfully,
G. Fincham.

5, Spring-Gardens,
June 12th, 1832.

SALINE TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

In your number for the 12th of November last, you inserted a paper of mine, in which I endeavoured to draw the attention of the profession to the employment of diuretics in the treatment of cholera. Indeed (preposterous as it may appear to many), from the appearances on dissection, from the favourable symptom of a re-establishment of the secretion of urine—from the disease in preference attacking drunkards, whose urinary organs have long been urged to extraordinary exertions, and have in consequence gradually lost their energy, and from common cholera appearing after hot weather, when the saline refuse, instead of being eliminated by urine, which then is scanty, is directed to the skin, and, on a decrease of temperature, thrown back on the circulation, while the kidneys, through inactivity, have become torpid; I have often felt tempted to consider epidemic cholera as tantamount to epidemic ischury. Thus biassed, I could not avoid attributing the recent success of the saline treatment to the effects of small doses of soluble alkaline salts on the urinary system, rather than to a direct change in the qualities of the blood, according to the doctrine of Dr. Stevens and others.

From the cases reported in your last number (236), in which saline injections were employed, it appears to me that where the saline substances proceeded exclusively to the bowels, they failed; where they produced an effect on the kidneys, recovery was the result. It is remarkable, too, that the injections in Case I. are stated not to have produced any change in the consistence and colour of the blood, but to have, in a most expeditious manner, found their way into the alimentary canal. That the diaphoresis, increased temperature, and fresh supply of fluids thus produced, must prove of great temporary advantage, I have no doubt; still I feel inclined to ascribe the final success of these injections to their diuretic effect; for where this did not take place, their usefulness seems to have been temporary only. The muriate of soda, therefore, had better not be employed in doses that may prove purgative, whether from their largeness or frequent repetition; and it strikes me, that remedies, when administered in this way, should be used in smaller doses than usual.

Far from wishing to detract from the credit due to Dr. Stevens, for having principally been instrumental in bringing into vogue the most satisfactory treatment hitherto tried, you will allow that it is of some importance to investigate in what manner remedies effect a cure; and my aim in addressing you is

to promote further inquiry into the truth of the above opinion as to the operation of salines in cholera, as well as the febrile diseases in which they have been found useful, and where the critical urinary discharge would seem of the greatest moment, on account of its ridding the system of substances of a saline or alkaline nature, or elements forming other combinations inimical to animal organization.

That the constitution of the blood is altered, in different morbid states of the body, seems evident, and that the urinary system is destined to eject the most noxious of its abnormal ingredients, is, from its functions during life, highly probable. The diuretic effects of remedies I would likewise explain, not so much by a specific stimulus they present to the kidneys, as by their either supplying a superabundance of fluid, or containing vegetable aroma and essential oil, or saline or other substances, (when from the nature of the dose these do not prove purgative or diaphoretic,) which it is the office of those organs to separate from the circulating mass, in some instances, according to the experiments of Drs. Wollaston, Marcet, and others, through channels immediately connected with the *primæ viæ*, so as to prevent their entering the circulation at large.

Few will doubt that there is a *vis medicatrix*—a kind of presiding power in the animal organizm, appointed to watch over its preservation, and consequently over its functions; and that this power, for which the nerves seem to serve as conductors, is particularly perceptible in the various organs of secretion, which appear endowed with an elective faculty of abstracting, or else elaborating, from the blood, one bile, another semen, a third milk, &c. is an opinion which furnishes a simple, but not the less plausible, explanation of that function.

Viewed in this light, the remedial agency of diuretics might in many cases be accounted for: for instance, that of colchicum in gout, (a disease which seems the effect of an accumulation of azote, hydrogen, &c. in the system, caused by a diet abounding in those elements); of taraxacum in icterus; dulcamara and viola tricolor in cutaneous eruptions; water-cresses, cochlearia, &c. in scurvy; cantharides (by Hippocrates) in icterus. Mason Good quotes an author, who states that an epidemic itch, after having resisted all other usual means, was cured by irritating diuretics.

Should you find room for the above remarks, I shall be obliged by your inserting them, and remain, sir,

Your most obedient servant,

E. Stanley, M.D.

June 16, 1832.

INJECTIONS INTO THE VEINS IN CHOLERA.

(Papers communicated to the Central Board of Health.)

Sir,

Since my last communication, I have subjected to the influence of venous saline injection a few more cases of cholera, which occurred during my week of hospital attendance. A few considerations in the treatment have suggested themselves, which you may deem not entirely devoid of interest. With such an impression, I have been induced to send you the following brief statement of cases, with remarks.

Case I.—*Forty Pounds injected in twenty hours—Apparent Convalescence for two days—Ultimate Death**.

A prostitute of the very lowest grade was brought to the hospital on the 28th May, at half past 1, affected with cholera: the case was one of more than ordinary severity. I not being present, she was subjected to the ordinary saline treatment recommended by Dr. Stevens, till four o'clock. As soon as I arrived I found her sinking rapidly, tortured with the most fearful spasms, distressing retching, severe diarrhœa, extremities cold, pulse gone, eyes sunk, and other symptoms of confirmed collapse. Her body exhaled a most disagreeable odour, which increased with her distemper, so much so that the windows, constantly open, scarcely admitted pure air enough to render the apartment where she lay tolerable to the attendants. The saline injection was instantly thrown into the veins, which was followed by the most agreeable consequences, having entirely removed the insupportable load from her chest, which was a source of much anguish. After eight pounds were injected, she experienced entire relief.

Diarrhœa had all along been very profuse, but now it became excessive, running through the bed, all over the floor, in consequence of which the rallied powers again gave way, requiring to be stayed by a repetition of the injection. So rapidly did she sink, that it

* The writers of these papers are not answerable for the headings put to the cases.—Ed. Gaz.

became necessary to inject with more than usual speed, which, for reasons afterwards to be noticed, should always, if possible, be avoided. About 20 pounds were injected on three different occasions within 40 hours, by which time the symptoms of cholera were removed, the diarrhœa considerably lessened: the stools were now tingedwith an increasing flow of bile; and the urinary secretion was restored. For two days she seemed to be tolerably well. Previous to the attack of cholera, she had been under treatment for diseased liver, and was also subject to pectoral complaints: the symptoms of these were now aggravated, for which she was bled, leeched, and blistered, with but little effect. Her stools now became of a dark olive colour, and, like the increasing fetor of her person, were very offensive. The whole surface assumed a leaden hue, was wan and clayey, and she gradually sunk, and expired without a struggle.

Case II.—*One Hundred and Thirty-two Ounces injected during the first two hours—Injections repeated—Temporary Improvement Ultimate Death.*

On the 27th of May a middle-aged female was brought to the hospital in *articulo mortis*. She had been seized with cholera early in the morning. The diarrhœa and vomiting, which had been very profuse, had so thickened the vital fluid, by draining off the serum, that by noon the exhaling vessels, gorged with their own thickened contents, ceased to furnish that fluid which constitutes the dejections so characteristic of the disease. When she was lifted from the cot in which she had been carried to the hospital, her head dropped on her chest, her arms hung dangling and lifeless from her trunk, the whole surface was blue, and her entire aspect was that of one whose spirit had just fled. Unconscious of existence, she was laid on the heated mattrass. I instantly opened a vein in the right arm, and threw in 132 ounces of saline fluid, keeping the temperature above 105°. At first it was rapidly introduced, but as soon as symptoms of resuscitation appeared, I proceeded more slowly, and, excluding some considerable delays, accomplished the whole in about two hours.

Having finished, I felt much gratified with the result: the poor woman was in a short space restored to the entire use of her senses. But though the result was much more fortunate than I had any reason to expect, I entertained not the most sanguine hopes for her ultimate recovery: for though her other symptoms were good, the radial pulsation continued very feeble. Diarrhœa returned; and notwithstanding the free application of stimuli, externally and internally, again she became pulseless, and by midnight she was as low as ever. Eighty ounces of saline injection again restored her, and by two in the morning (May 28) she was greatly improved. Respiration not in the least laborious, though quicker than natural. Pulse 120, small but distinct; countenance natural, red; tongue moist and warm; temperature of surface restored: skin moist, &c. &c. Towards morning diarrhœa returned; she continued slowly to decline, in spite of every remedy; the surface became cold and clammy, the pulse ceased, and she complained of extreme debility. So I was compelled again to have recourse to the only thing that produced any impression, and at 4 A.M. four pounds nine ounces were injected, which drew from her the most grateful acknowledgments of relief. Mercurials, tonics, and stimulants, with effervescing draughts, were administered throughout the day, and before night she had passed five bilious stools, and micturated freely two or three times. Symptoms of cholera were now gone; she passed the night easy, and next morning (May 29) the report is—has no complaint except debility and considerable thirst. Pulse 104; tongue dry and red. Breakfasted with relish; bowels free; dejections very dark; passes urine freely. During the forenoon seemed absent. In the afternoon she became incoherent. During the night she gradually sunk, and died next morning, without any symptom of collapse or of any local disease.

Case III.—*Eight Pounds injected in half an hour—Injections repeated—Recovery.*

A middle-aged man, of sober, industrious habits, in whose family several cases of cholera had occurred, was, on the 27th May, at 9 P.M. seized with the first symptoms of that disease, and was subjected to its ordinary treatment seven hours previous to his coming under my care. On the morning of the 28th, at half-past five, his countenance was pallid, his eyes sunk, his voice choleric in the highest degree. Pulse 118, fluttering. Every feature sharp, and extremities cold. Had had saline injections per anum, laxatives, opiates, stimulants, hot applications, sinapisms, &c. &c.; and notwithstanding sunk rapidly, particularly during the hour previous to injecting the veins, an operation which was unavoidably delayed, I being engaged with another patient. In half an hour eight pounds were injected, with entire relief to every unpleasant feeling, if we except the irritation arising from the sinapisms. He was ordered to take hourly small doses of calomel and opium, and beef-tea enemata, with muriate of soda. Diarrhœa continued copious and frequent, and by 2 P.M. seemed sinking fast; had almost lost his sight. Recommenced the venous injection; his sight soon began to improve; and before a pound was injected, it was quite restored. About six pounds expelled

all his uneasiness, and from that moment he continued to improve, using only beef-tea, mercurials, and effervescing draughts. Early next morning his stools became bilious, the secretion of urine was restored, and he was convalescent on the third day.

Remarks.—Here are three cases having different features: the first proving fatal from organic disease, the second from the effects of the most inveterate diarrhœa, and the third recovered by the prompt and easy application of venous injection.

With regard to the cause of death in the first case I have nothing to say, it being now generally admitted that the individual labouring under chronic disease, like the drunkard or the debauchee, is very liable to be assailed by cholera, and that in consequence of such an attack, the dormant disease develops itself more actively, and very often proves fatal, or issues in confirmed bad health. With regard to the second cause of death—viz. the diarrhœa and its consequences, this is a subject which appears of most interesting importance.

The watery diarrhœa is evidently derived from the extremities of the minute vessels ramifying on the stomach and intestinal tube: these are, in some way or other, forcibly dilated, affording exit to the serum and crassamentum of the blood; nay, in the advanced stage of aggravated cases, even of the red particles, which last symptom, accordingly, always affords an unfavourable prognosis. Now, whether this effusion is only sympathetic of hepatic obstruction, or is an idiopathic disease produced by the invisible meteorological cause, may be subject of dispute. At all events, if we consider that those individuals who seem most liable to the attacks of this terrible disease, are those whose diet and habits of life are very apt to derange the functions of the liver—that pain is often very acute in the hepatic region—that the secretion of bile is among the first that ceases—that its restoration is hailed as a happy omen—that it continues morbid, and a source of great uneasiness long after the stage of collapse is past—that hepatitis very frequently supervenes during reaction—that a diseased state of the organ is very generally met with after death—that medicines, such as mercurials, which act on this viscus, are of much utility—that it is always gorged with blood;—also consider that the nature of the hepatic circulation is such as renders it very susceptible of obstruction, more especially if the blood is reduced to a viscid state by the abstraction of a portion of its serum, and the nervous energy subdued by the influence of any sedative agent; and, besides, that the effusion of serum in cholera is confined to those viscera which return their blood through the veins, in which viscera venous congestion is always strongly marked;—all these circumstances certainly render it probable that hepatic obstruction does exist, preventing the return of the blood from the bowels, producing in these the usual consequences of obstructed circulation—the effusion of serum. In other parts of the body the blood returns to the heart, flowing from branches to trunks, and when it reaches the centre of circulation, if thickened from the draining effects of diarrhœa, it collects; thence producing the insupportable load so symptomatic of cholera; but so soon as saline fluid is introduced along the veins, and directly mixed with it, then free circulation is restored, and the anguish is removed. The blood thus restored to a fit state for circulation makes its circuit, and congestion is removed from every viscus except the liver. Where the circulation is widely different, the thick grumous blood gorges its venous ramifications. The obstruction thus produced must continue until, by slow degrees, it is penetrated by the more fluid circulating mass. Until this is accomplished, diarrhœa must continue: the symptoms of its restoration are a cessation of the serous diarrhœa, and a return of the biliary secretion.

But in the second case noticed above, such obstruction seemed entirely removed, and the secretion eventually restored, and yet the patient died. Even so it happened; but then we must take into account the circumstance that the diarrhœa being profuse, and often renewed, had carried away a large proportion of the more substantial principles of the blood, which, though not immediately necessary to existence, yet the deficiency is soon felt, and if not supplied, the vital fluid may speedily fall short of its intentions. But not only was such a pernicious change effected on the blood, but the fearful extremity to which the poor woman was reduced must have proved an almost irrecoverable shock to the nervous system, and from the abstraction of energy favoured the deposition of fibrine which was found in the heart, which was doubtless the cause of the very feeble pulse, which induced me to form an unfavourable prognosis of the case.

If the view I have taken of the cause of the diarrhœa is correct, it points out the need there is for the early use of the saline injection, the benign effects of which are clearly demonstrated in the third case. It is not improbable that the quantity of salt formerly recommended may be too large, particularly if we require to repeat the injection: circumstances which I may notice on some future occasion have induced me to think so, and accordingly, in each succeeding injection, I diminish the quantity of saline ingredients.

I may mention, though I have no permission for doing so, that solutions of quinine and of morphia, pure water, and even blood,

have been all used by some medical gentlemen, but not with such encouragement as induced them to persevere. Albumen has also been used in the saline mixture, but given up, though I would certainly be disposed to continue its use in extreme cases, when the diarrhœa has continued profuse, and has returned after each injection.

I regret my letter has turned out so voluminous; I trust, however, I may be excused for such indulgence, and have the honour to be, sir,

Your most obedient servant,
T. Latta, M.D.

Leith, June 13, 1832.

To W. Maclean, Esq.

Case IV.—*Four Pints injected in an hour and a half—Temporary Improvement—Relapse—Injection repeated, until seven pints in the whole had been thrown into the vein—Ultimate Death.*

Sir,

I beg, for the information of the Board of Health, to state that I was called on Saturday morning last to visit an old man, living on the banks of the Thames, labouring under an attack of cholera morbus. I was sent for about 8 o'clock, and I then found that he had been taken ill five hours before. He had now passed into the second stage, the cramps had began to subside, the pulse at the wrist fluttering and indistinct, and the whole surface cold and clammy. Seeing that the man was almost in *articulo mortis*, and that no remedy with which I was acquainted, and of which I had any experience, was at all likely to benefit him, I at once decided on submitting him to the operation of the hot-air bath, and to the injection of saline substances into the blood. While these things were preparing, I gave him a drachm of carbonate of soda in brandy and water, and injected two drachms, with the same quantity of muriate of soda, dissolved in hot water, into the bowels, taking care that it should not return. At 9 o'clock I commenced throwing into the cephalic vein of the right arm a solution, containing two drachms of carbonate of soda, and one scruple of the muriate of soda, in four pints of water at 112°. I continued gradually injecting until the whole quantity was thrown up, which occupied me until half-past ten. During the operation a most perceptible alteration took place in the appearance of the countenance; the cheeks and lips, which had before been blue, cold, and bedewed with a clammy moisture, now began to recover their usual tint; and the face, as well as the surface over the whole body, lost that deadly cold and inelastic feel, so peculiar to an attack of cholera, and a more diffuse warmth and natural appearance were manifested. This favourable change lasted but a short time. Liq. ammonia, brandy, and soda, were repeated as frequently as the patient could be prevailed on to swallow, and the injection was continued until about seven pints were thrown into the vein. No inconvenience seemed to arise from charging the circulation with this immense addition of fluid. A slight difficulty and hurried breathing came on, after I had thrown up nearly a quart of the solution pretty quickly, and when I thought the man was nearly gone. He rallied for a short time, and expired about twelve o'clock, nine hours after the attack. I may just add, that this man had been labouring under an irritable state of the bowels for many years, and that he could never leave home for a few hours without seeking some place to relieve them.

I remain, sir,
Your most obedient servant,
Francis Godrich.

Brompton District,
19th June, 1832.

To W. Maclean, Esq.

LETTER ON SALINE INJECTIONS IN A CASE OF CHOLERA.

Sir,

Circumstances have prevented me from transmitting an account of the fatal case of cholera, referred to in my letter to you of the 27th ult. sooner. Death took place, in the case alluded to, ten hours after the circulation had been restored by means of copious saline injections into the veins; the patient previously suffering severely from symptoms characteristic of fatal abdominal derangement, and these attributable, I believe, to the means employed to rescue him (and which succeeded) from death, in another shape.

In the space of two or three hours after the venous injection had produced the astonishing effects already repeatedly mentioned, the patient appeared to do well; after which, however, he began to complain of distention and uneasiness in the bowels; this was followed by great restlessness and distress, until death closed the agonizing scene.

The symptoms in this case were exactly similar to those which I have observed when complete loss of functional power in the intestinal canal had taken place, as a cause or effect of obstruction, or from mortification, or perforation of the intestines.

A difference of opinion existed in the minds of my able and respectable colleagues of the Leith Cholera Hospital, as to the immediate cause of death in this case, before an examination of the body took place; and

my talented friend, Dr. Mackintosh, of Edinburgh, whose zeal in the cause of medical science is scarcely equalled, certainly not surpassed, by any man alive, thought it probable that we should find a ruptured blood-vessel in the abdominal cavity. But upon dissection all the viscera were found sound. The intestines were enormously distended; the bladder was firmly contracted; there was no effusion of any kind into the cavity of the abdomen; the veins were perhaps more turgid than natural, but not so as to attract particular attention. Upwards of seven pounds of fluid, of the kind usually evacuated in cholera, were found in the bowels. The distention of the abdomen, the supervention of distressing symptoms, and ultimately the death of the patient, was, I have no doubt, the effect of total want of functional power in the intestinal canal—a condition which I have sometimes observed to take place, although I do not remember to have seen it particularly noticed by medical writers, during the progress of other diseases.

My conclusion in regard to the case of cholera now under consideration is, that paralysis of the intestinal canal, occasioned, or increased, by the copious venous injection, was the cause of death—an opinion to which I shall have occasion to revert, in explaining other conclusions I am induced to draw from our late experience of this most extraordinary disease, and which I shall speak of fully in a future communication. In the meantime I may remark, that the failures we have witnessed, in my opinion, afford no real objection to the practice of treating cholera by venous injection—a method of medical treatment which, as I said before, will I predict lead to important changes in the practice of medicine, and will entitle Dr. Latta's name to be placed amongst the number of those (alas! how few) who have really contributed to the improvement of the healing art.

In a late number of the Medical Gazette, a writer, in speaking of the Leith and Edinburgh cholera cases that have been treated by venous injection, seems to imagine that the patients who died did so in consequence of phlebitis: that is not the fact; there has not been a single case of death from that cause*.

I have the honour to be, sir, yours, &c.

ROBERT LEWINS.

Leith, June 14, 1832.

To W. Maclean, Esq.

* We were informed that phlebitis had supervened in some cases, by a physician from Edinburgh who had witnessed the practice of saline injections.—ED. GAZ.

MEMORANDA OF FIVE CASES OF CHOLERA IN WHICH SALINE INJECTIONS WERE USED.

Rochester, June 17, 1832.

SIR,

I BEG to acknowledge the receipt of your letter of the 17th instant, and I feel highly gratified to find that my communication has been so favourably received by the Board; since which time I have had considerable experience in the employment of the saline injection by the veins. The first two cases selected for trial had been in a state of the most perfect collapse for about five hours, and both passing their stools involuntarily; which constant drain from the bowels I have ever found a fatal symptom under every mode of treatment yet employed.

In the first case, upon injecting four pints into the veins at the bend of the arm, the pulse became perceptible, with a general sensation of heat over the body. The injection was continued, and two additional pints thrown up very gradually, when the pulse still continued to rise, and the countenance to assume its natural hue (which was before quite blue). Upon such unexpected results, another case in a worse state was immediately selected, and the same process adopted, and with the same results. Severe rigors took place in both these cases a few hours after throwing up the fluid; but still the pulse continued for about three hours after the injection, after which collapse again took place in both about the same time. The injection was again had recourse to, and with the same decided benefit. This plan was followed up in both cases for about twelve hours, when I was obliged to leave, requesting that during my absence similar treatment might be persisted in during the night, but was sorry to find, upon my return to the ship next morning, that one of my patients had just died and the other dying. In the first, 305 oz. were thrown up, and in the other about 190 oz. During all this time, an immense serous discharge continued to run from the bowels. Three additional cases were selected yesterday, and this plan of treatment adopted, and, I am happy to say, with most decided benefit; they are all at this moment convalescents. In neither of the cases, however, had that insensible serous discharge taken place from the bowels, although the collapse was equally perfect, nor was it necessary to inject more than from four to six pounds in each. In one of the cases sleep took place during the process. Upon the whole, I am inclined to place more dependence upon this remedy than upon any other mode of treatment yet recommended; and I have no hesitation in saying, that if you see your patient soon after the collapse takes place, and before the insensible *draining* takes place from the

bowels, you have an almost certain remedy within your power. This plan of treatment I intend to follow up for the present, as altogether the most favourable I have yet observed. The number of cases that have occurred in both convict ships since the commencement, 56; deaths, none; cured, 13.

It was reported yesterday that cholera was prevailing at Sheerness, and that three deaths had taken place.—I have the honour to be,

Your very obedient humble servant,
JOHN ANDERSON, M. D.
Surgeon R. N.

To W. Maclean, Esq. Secretary to the Central Board of Health.

CASES OF CHOLERA

WHICH WERE TREATED BY

SALINE INJECTIONS INTO THE VEINS,

At the St. Marylebone Infirmary,

BY DR. SIMS.

CASE I.—*Eight Pints of Saline Fluid injected in eight hours—Recovery.*

WILLIAM CAPES, æt. forty-five, a shoemaker, admitted into the Infirmary June 11th.

Twelve noon.—Pulse 90, small, feeble; tongue cold to the touch; surface of extremities cold and blue; lies with his mouth open, in a half-stupid state; can be roused to answer questions with great difficulty; voice feeble.

He has been ill for some days; about thirty-six hours ago was seized with violent vomiting, purging, and cramps.

Utatur Balneo Aeris Calidi; Inhaletur Vapor. Sp. Vini Gallici; Pulv. Salini, Dris. Stevens.

One P.M.—Two pints of the following solution, 108° F., were injected into the median basilic vein:—

℞ Sodæ Muriatis, ʒij.; Sodæ Carbon. ℈ij.; Aq. Dest. Oiij. et ℥xii. Solve.

After the injection his pulse much improved; his countenance was less sunk; slight perspiration, and the livid colour of the skin was much diminished. He appeared in all respects better, and expressed himself as follows:—"I feel more lively; I pay more attention to what the gentlemen say to me. The impression died away from my ear before, and I thought no more of it." He says that since he was first attacked, he has forgotten the questions put to him as soon as uttered. Two small motions with flocculi.

Four P.M.—Pulse fails; increase of stupor; roused with difficulty; face more livid; one motion, with slight feculent odour.

Three pints of the saline solution, 115° F. injected into the vein.

After the injection, the pulse again rose in strength and volume, 98; countenance improved; still lies in a half comatose state; mouth and eyes half open.

Half-past 8 P.M.—Pulse 116, very feeble; skin cold, livid, with clammy perspiration; death-like expression of countenance; he appears to be rapidly sinking.

Weak brandy and water at intervals.

Another vein was opened, and three pints of fluid injected of double the strength of the former.

℞ Sodæ Mur. ℥ss. Sodæ Carb. ʒiv. Aquæ Oiij. et ℥xii. solve.

He again improved; pulse 100, increased in volume, but not so sensibly as after the first injection; much inclination to sleep; general perspiration; urine passed freely.

12, Midnight.—Pulse continues good; stupor increased; copious dejections of rice-water fluid, with flocculi, and some feculent matter.

Admov. nuchæ Emp. Cantharidis.

June 12th, 8 A.M.—Has passed a quiet night, slept well, and expresses himself as feeling much better; skin of extremities still livid, clammy, inelastic; pulse 100, tolerably firm and regular; soon becomes drowsy after being roused by questions; three copious motions during the night, of the same character as the last.

1 P.M.—Pulse 120, fails again; skin colder; copious perspiration; drowsiness continues, but answers questions distinctly when roused; tongue dry, lightish brown; involuntary motions; it was remarked today that the action of the bowels speedily succeeded taking the powders.

Abradatur capillitium. Lotio frigida capiti applicetur. Sumat. Hydr. Subm. gr. v. 2da quâque horâ. Omittantur Pulveres salini.

13th, 1 P.M.—Pulse 80, firm, regular; stupor much diminished; tongue cleaning at the edges; skin nearly natural; thirst abated; no motion from the bowels since last report; has passed water with consciousness; expresses himself as unable to collect his thoughts, but he answers questions pertinently.

Sumat. Haust. Sennæ Co. ʒtiis horis usque ad alvi solutionem. Habeat statim Enema purgans. Omittatur Hydr. Submurias.

Vespere.—Bowels acted after the first senna draught; has had three feculent motions of dark grey colour; pulse continues regular; skin rather cool.

14th.—Has slept the whole of the night; one small motion.

Sumat. statim Hydr. Submur. gr. viij.
Rep. Haust. Sennæ Co. Rep. Enema purgans.

Vespere.—Has been much inclined to sleep throughout the day; perfectly rational when roused; bowels slightly open; motions darker; pulse rather lower than in the morning.

℞ Conf. Aromat. ʒj. Tinct. Cardam. Co. ℥ss. Mist. Camph. ℥iiiss. Aq. Menthæ Pip. ℥iv. M. Sumat. ℥iss. 4tis horis.

15th.—Still sleeps heavily; more easily roused; pulse improved. Has had a small dark-coloured feculent motion; tongue moist. He took a senna draught in the morning.

Sumat. statim Hydr. Submur. gr. x. Habeat Enema purgans. Sumat Misturam 8va quâque horâ.

16th.—Copious feculent motions; urine free; pulse 78; improved in all respects. He has slept much less during the last twenty-four hours.

17th.—Convalescent.

19th.—Tongue clean; motions natural; sits up; appetite good; no complaint but weakness.

Case II.—*Attack of Cholera during convalescence from Chronic Dysentery — Injection into the Veins practised after the failure of other remedies — Some attempt at rallying, followed by relapse, and Death in five hours.*

Joseph Pritchard, æt. 71, admitted June 12th.

6½ P.M.—Countenance sunk, remarkably deathlike; dark areola round the eyes; pulse 120, feeble; tongue moist, icy cold; surface generally cold, that of the extremities of a livid colour; voice weak. He complains much of headache.

He had been a patient in the infirmary several months with chronic dysentery, and was considered convalescent, though still in a very weak state. He says that he was disturbed early in the morning with purging, which soon much increased in frequency and quantity; has vomited once; passed urine half an hour before admission; no cramp; coldness came on three hours ago.

He was put into the hot-air bath; brandy and water was given him; and he took the saline powders, according to the plan of Dr. Stevens.

7½ P.M.—Heat restored by the bath, followed by copious perspiration and several watery motions, containing flocculi; pulse irregular and weaker.

℞ Sodæ Mur. ʒij. Sodæ Carbon. ℈ij. Aquæ destil. Oiij. et ℥xij. Solve.

Three pints and three ounces of this fluid were injected into the median basilic vein.

Sumat. Hydr. Subm. gr. v. 3tiis horis.

℞ Ammon. Carb. ʒj. Conf. Aromat. ʒj. Tinct. Cardam. Co. ℥ss. Mist. Camphor. ℥viiss. M. Sumat ℥iss. 3tiis horis.

After the injection, his pulse became firmer and more regular, and he expressed himself as better; tongue warmer. From this time he gradually sunk.

Died soon after midnight.

Dissection—12 *hours after death.*

Head.—A quantity of blood and serous fluid escaped on opening the skull. Blood-vessels of the membranes and of the substance of the brain congested; those of the pia mater and the filamentous tissue of the arachnoid in a greater degree: the pia mater was readily peeled from the surface of the brain. The fluid had issued from the dilated ventricles and from between the membranes. On the upper part of the middle lobe of the right hemisphere was a space about the size of a half-crown, redder than the rest: here there appeared a small quantity of blood uncoagulated, between the pia mater and the grey substance. Medullary matter firm; cineritious, soft. The carotid arteries and ramifications loaded with white matter; the others not so.

Abdomen.—Mucous membrane of the stomach very much thickened and granular. Mucous membrane of small intestines slightly thickened and highly vascular in some parts. Colon contracted; vestiges of ulceration in the rectum. The intestinal tube contained the fluids so generally found in cholera, and the small intestines were in some parts lined with a paste-like substance.

Liver.—Several deep furrows on the anterior surface; lower margin nutmeg colour. Gall bladder filled with dark bile. Spleen distended and red; its coat was thickened. Bladder contracted.

Inferences.—In the case of Capes the remedy appears to have had no injurious effect. Before the first injection of fluid into the vein he was sinking rapidly. During and immediately after the injection, the action of the vascular and nervous systems was restored, and his mental energy returned. Before the third injection he was nearly dead; after it he became re-animated.

The drowsiness which was present before the injection, and continued for

three or four days, was not greater than in cases of cholera where the remedy has not been used.

The saline compound of Stevens appeared to have an injurious effect in this case. The other parts of the treatment were as simple as possible: he did not take the smallest dose of opium, and the quantity of stimulants introduced into the stomach was very small.

The fatal case of Pritchard was under treatment about five hours: there was no evidence during life that the injection produced any injury. He continued to complain of pain in the head, but it was better after the injection. From the report of the dissection of the brain, the practical pathologist may be able to judge whether or not the appearances noticed are ordinarily found in a septuaginarian with diseased cerebral arteries, who, during the last few hours of his life, suffered a severe and rapid disease, subsequent to a long and debilitating chronic affection of the alimentary canal.

The first case is in favour of the saline injection; the second case shews nothing against it.

June 20, 1832.

PROPOSAL TO INJECT SERUM.

To the Editor of the London Medical Gazette.

Sir,

Having lately read in the Gazette the plan of saline injection in cholera, as recommended by Dr. Latta, and having further seen the details of some cases in which it had been decidedly successful, and others in which it had been productive of temporary benefit, it occurred to me, that if the crude materials injected were attended by such astonishing and happy results, something still nearer resembling the part of the blood lost in the evacuations would probably crown with more complete and general success the humane endeavours of our professional brethren. I therefore take the liberty, if you think them worthy of notice, of suggesting the following hints:—1st, that if possible, when a person is seized with cholera, an animal, say a sheep, should be procured and bled, and the blood set aside, that the serum may separate, and this *diluted* be then injected into the veins. I am aware that the serum may be altered in its properties, but surely it is more like the serum of the blood whilst in the vessels than a solution of any salts can be. 2. That if this is impracticable, the animal should be kept in readiness, and bled at the moment of transfusion, and the blood thrown in diluted with water, of a proper temperature, injecting at intervals portions of undiluted blood, which may tend to give a more powerful and natural stimulus to the system. Of course the blood employed should be *arterial*, and not *venous*, or at all events, a mixture of the two, as venous blood would only, in all probability, add to the mischief, as may easily be imagined from the actual state of the blood in cholera. If it be objected to the first hint that it would require too long a time for the process of separation to take place, it may be necessary to answer that it should be resorted to as early in the disease as possible, so that, as a last resource, it may be in readiness if all other means fail. I need not add that the serum should be warmed; and, therefore, I recommend it to be used diluted, as otherwise, of course, it would be coagulated. Perhaps even then it may still coagulate, but I have not the means of determining this point at present, as I write in great haste. The plans may be varied, and no doubt, if feasible, will be improved; but I could not allow even the *chance* of a useful suggestion to be lost, because I had not more time for experiments, or for further consideration of the subject.

I am, sir,
Your very obedient, obliged servant,
R.

Peckham, June 12th, 1832.

[Three cases in which transfusion of blood was adopted by M. Scoutetten, of Metz, without success, will be found in our number for December 17, 1831. —Ed. Gaz.]

SOUNDS OF THE HEART.

Dr. David Badham in Reply to "Auscultator."

To the Editor of the London Medical Gazette.

Sir,

I apprehend "Auscultator" to be quite wrong in his interpretation of the cardiac sounds in the case which I had

related, and from which I had deduced general conclusions that still appear to me uninvalidated. It is true that I argued very much from a single case, but it is to be remarked, that a single case, in a subject of controversy, is sometimes very important, because if phenomena, accurately observed, be altogether irreconcileable with a prevailing theory, the pretensions of that theory must necessarily be questioned. I shall be most happy to have an interview with my ingenious antagonist, if he shall think proper to reveal himself, and the result will probably be that one of us will become the proselyte of the other. In the meantime I stick to my text, and proceed to read to the medical public another homily on a subject in which I take considerable interest, and which, in fact, is to the physiologist sufficiently interesting to justify any degree of attention.

"After the ventricular contraction," says Auscultator, "producing (the first) sound, impulse, and pulse, and followed by its corresponding second sound, there succeeds a second ventricular contraction, so feeble as not to produce either impulse, pulse, or sound, but sufficient to expel a small quantity of blood from the ventricles, and thus give rise to a diastole. As the first sound is naturally more suppressed than the second, the first, when both are diminished in an equal degree, may become extinct, while the second alone remains audible; and such, I have little doubt, was the case in the instance to which Dr. Badham refers." It is clear from this passage, which I quote in "Auscultator's" own words, that the correctness or incorrectness of his explanation must depend entirely upon the circumstance of the two normal sounds of the heart "being diminished in an equal degree;" but here I can most positively state that such was not only not the fact, but that one at least of the cardiac sounds in question was augmented, as, indeed, one would have anticipated from the nature of the organic lesion by which the heart was affected; for the natural sounds of this viscus are increased in intensity by dilatation of its walls; and, therefore, in the case to which "Auscultator" refers, and where this morbid condition was actually present, these sounds could not have been "diminished in an equal degree, as he has little doubt they were." Again, allowing it to be an ascertained law in acoustics, that when two sounds, a graver and a more acute, are equally diminished, the graver also first ceases to be heard, how, I would be glad to learn, can "Auscultator" explain, on his own supposition, the identity between the second and the several reduplicated sounds which were heard in this case? The second sound, agreeably to Dr. Hope's doctrine, arises from a full ventricular diastole—the reduplicated sounds from exceedingly imperfect and limited expansions of the ventricles: how, then, come they to be so similar in kind, period, and intensity, as to prohibit the distinguishing of either from the other? To which objections I would further urge, that "exceedingly febrile contractions" are perhaps less likely to occur in active aneurism than in any other condition of the heart; nor can I conceive any ventricular systole so slight as not to produce—it may be a very feeble, but still a perceptible pulse. The blood is an incompressible fluid; the arteries are yielding tubes; any sudden addition, therefore, to the mass which they already contain must enlarge their capacities, and lead to the production of pulse.

The following abstract of another case, while it appears to me to confirm the accuracy of Laennec's statement respecting the sounds and motions of the heart, presents phenomena which are irreconcileable with any of the more recent speculations of physiologists on this subject.

An old woman, aged 75, was admitted during the winter 1830, into the Glasgow Royal Infirmary, under the care of the Professor of Medicine. She was labouring under ascites and general dropsy, from an organic affection of the heart. The ventricles of this organ were hypertrophous, (active aneurism) and there was besides valvular disease. One remarkable circumstance in the stethoscopic investigation of these organic mischiefs was the existence of a double bellows-sound, occurring at the inferior portion of the sternum, to the left of that bone, and heard loudest over the superior part of the epigastrium. The first sound of the heart's pulsation, which is admitted on all hands to be ventricular, was here converted into the morbid sound just mentioned, which was of long duration. This should have been followed, in health, by a second

sound, clearer in its kind, and of shorter persistence; in place of which, a second "bruit de soufflet" supervened, not so prolonged, indeed, as the first, but equally characteristic. Now, had our investigation of this case stopped here, it would have been impossible to meet an objection that might have been urged against Laennec's theory of the succession of the cardiac sounds; for in the first place, some apparent testimony would have been afforded to the idea that the second normal sound of the heart had been merely converted into the second bruit de soufflet, as this correctly represented its natural period of persistence, and occurred in the natural order of succession; and, secondly, it was easy to shew that the bruit de soufflet in question proceeded from the ventricular parietes, or cavities, since it was loudest over these cavities, and very faintly heard as we receded from their situation towards that of the auricles. If, then, the second sound of the heart had been merely converted into the second bruit de soufflet, as this last was ventricular, it would have followed that the former was also ventricular. But the fallacy of such a conclusion became at once evident on our applying the stethoscope higher up on the chest over the auricles, when the clearer of the two normal sounds was instantly recognized; shewing, of course—

1. That it had not been superseded by, or converted into, a bruit de soufflet, but had been merely marked by the greater intensity of this morbid sound, from the instrument having in the first instance been placed over the site of the ventricles.

2. It was further apparent, that a sound heard just over the auricles, and only in that situation, could not be any other than auricular, and hence it was impossible not to conclude, in direct opposition to the theories of Drs. Hope and Corrigan, "that both sounds of the heart are not ventricular, but that one proceeds from these cavities, while the other proceeds from the auricles. Again, as the clear sound, which I have called auricular, was short, and as the second bruit de soufflet, was also short, as their time of persistence was the same, and as it is the clearer sound of the heart which occurs second in order, which order here obtained in the occurrence of the morbid sound, it cannot be doubted, I think, that the two were synchronous, and if this be admitted, it remains that we consider the auricular sound of the heart as second in succession.

The diagnosis of this case, as far as the valvular disease was concerned, was sufficiently obvious; some "patency" existed from an imperfect occlusion of both ventriculo-arterial orifices, admitting some regurgitation of blood into the ventricular cavities. Several organic causes might indifferently produce this result: which was the real and efficient one here, it would be in vain, before death, to speculate upon.

I may mention, in conclusion, that two or three of the students, not previously instructed in the use of the cylinder, instantly recognised the peculiarities of this case, on its being properly applied to the chest.

I remain, sir,
Your obedient servant,
David Badham, M.B.

42, Half Moon Street,
May 31, 1832.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

A Treatise on the Injuries, the Diseases, and the Distortions of the Spine; founded on an Essay to which the Jacksonian Prize, for the Year 1826, *was adjudged by the Royal College of Surgeons.* By R. A. Stafford, Member of the Royal College of Surgeons, &c. &c.

This is an excellent book, and upon a very important subject, of which the author takes a more extensive and instructive view than is to be found in most of the works on the Spine recently published.

Mr. Stafford considers, first, malformations of the spine—particularly spina bifida; next follows concussion, and then fractures and dislocations. After which we have an account of diseases of the vertebræ and intervertebral substances, including rickets, mollities ossium, angular curvature of the spine, and lumbar abscess. Now these numerous and formidable diseases are well and succinctly described, and this portion

of the Treatise will be found, by the junior members of the profession more especially, to contain all that it is of importance for them to be acquainted with on the points to which it relates. But of the chapter on distortions of the spine we shall speak more in detail, because the subject is not yet so generally understood as it ought to be, and because Mr. Stafford has handled it in a very satisfactory manner. There are three distinct species of curvature—forwards, backwards, and to one side. To these, that the enumeration may be complete, must be added such curvatures as result from rickets and diseases of the bone; but it is to the former classes that we wish at present to direct attention.

Distortions of the Cervical Vertebræ may arise from various causes: first, from paralysis, general or partial, of the muscles of one side, those of the other side of the neck remaining healthy; secondly, from the habit of constantly holding the head to one side; thirdly, from disorder of the digestive organs; and fourthly, from inflammation of the muscles of one side, by which they have become contracted. These varieties give occasion to the following remarks:—

"Total or partial paralysis of the muscles on one side of the neck is not an infrequent affection, and it usually may be found to have taken place in infancy, after, perhaps, a fit of convulsions. It does not, however, always appear suddenly; it sometimes comes on very gradually, without any apparent cause, when from some reason or other (most probably from disorder of the digestive organs), the nervous influence, and with it the strength of the muscles, is lost, by which the balance of power on one side is destroyed, and the head is pulled to the other by the action of the healthy muscles. Hence the side to which the head inclines is not the one affected, but that opposite.

"The total or partial paralysis of the muscles accounts for the various affections of the neck we daily meet with, such as incapability of keeping the head in the erect position, so that it lolls to one or the other side; involuntary convulsive twitching, &c. &c.

"The first of these affections, which usually has its origin in infancy, and it appears probable from the birth, is owing to a complete paralysis of the muscles of the atlas and dentatus, and weakness of the muscles which maintain the head in the erect position; and thus the head falls upon the shoulder.

"The other affections of the cervical vertebræ may be produced from a variety of causes, and one of the chief among them, I have reason to believe, originates from a disordered state of the digestive organs. During infancy it often happens that the stomach and bowels are extremely deranged, at which time both the brain and spine sympathize with them. As an instance of this, we frequently see convulsions produced from irritation of the stomach. Strabismus, or squinting, also continually originates from a derangement of the alimentary canal, which, when removed, the irregular action of the muscles ceases. If, then, convulsions can be produced, and the powers of the muscles of the eye be affected by a disordered state of the digestive organs, why should not those of the cervical region also? The irritation produced from teething also may be considered to be one of the chief causes of these maladies. Children are continually thrown into convulsions from the pain they suffer from cutting the teeth, when they are seized with one or other of these affections, from which they seldom recover. I have seen several cases of this description, all of which can be traced from childhood. In one instance the individual has lost all power over the motion of the head, which falls on one or other of the shoulders; in another, the muscles which rotate the head are partially paralysed on one side, so that, although the sufferer can bring the head to a fixed point, it is immediately drawn on one side by the opposite muscles, and thus it is kept in constant motion; in a third, there is a constant convulsive twitching on one side; and in a fourth, the cervical vertebræ are contorted, as in wry neck.

"Independently, however, of paralysis of the muscles, the habit of leaning the head on one side, no doubt tends to throw the cervical vertebræ out of their equilibrium. This habit, like many others, of course brings the muscles of one side more into action than those of the other, by which the neck is gradually contorted.

"Sudden inflammation of the muscles on one side of the neck might also cause their contraction, and produce

permanent wry neck. Rheumatism would have this effect, and in the advanced periods of life when it shews itself, more particularly so.

"Besides these causes, I have reason to believe that the muscles are very often seized with a kind of tetanic affection, by which they become contracted, and remaining so, the cervical vertebræ suffer distortion."

In reference to the above quotation, we must take leave to express some doubt as to the correctness of the reasons assigned for enumerating derangement of the digestive organs among the causes of spinal curvature in the neck. At least we think that more satisfactory evidence than any yet adduced would require to be brought forward before the question can be looked upon as settled.

With regard to the treatment, Mr. Stafford observes, that if the muscles be paralyzed but little can be done; and the only thing which remains is to fix the parts by machinery. Those distortions, again, which are dependent upon contraction of the muscles, do sometimes admit of relief; and here the business of the surgeon is to restore the power of the muscles by bringing them again into action. In addition to friction, shampooing, &c. "a weight hanging laterally over the shoulder, on the concave side of the curvature, might be attached by means of a girdle round the head, and made to weigh down that side of the neck rather beyond even the distortion; whereby the muscles on the convex side would naturally act against it by the endeavour to keep the head in the centre of gravity, and thus the cervical vertebræ, in the course of time, if not wholly, might be partly brought back to their places."

The stoop, or semicircular curve forwards, next occupies attention, and is described as originating in two different causes: 1st, from a general weakness of the whole frame; and 2dly, from "bad habits." Of course it is of importance to discover on which of these any particular case depends. The weakly overgrown schoolboy is in vain corrected, or laughed at, for stooping, when this depends upon the weakened condition of his muscles; and the growing stoop of age is another illustration of the effects of debility, though here we suspect the vertebral column ought to bear its share of the blame, as well as its investing muscles. Any habit which calls into play one set of muscles more than is their due, ought as speedily as possible to be broken through; but to enter into any details on this point, would be a work of supererogation.

"For the reasons just mentioned, it would be a good plan to make children play at soldiers, and let the one who is disposed to stoop carry the drum in the same manner as the drummer of a regiment. I have little hesitation in saying that the habit would soon be cured. Another very good exercise also would be, to make the individual play the cymbals: he would be forced to extend his arms in the air and look upwards, by which the head and trunk would be thrown backwards, whilst the muscles of the shoulders would be in constant action. To prove the truth of this assertion, and to show the utility of the exercise, we have only to observe the two blacks who play this instrument in the Rotunda at Vauxhall. It would be almost impossible to point out two men who carry themselves more upright."

The lateral curvature occupies a considerable portion of the volume, and is very satisfactorily treated of. The causes enumerated as producing it are, first, weakness of the spinal column and its appendages; secondly, the habit of making too frequent use of any particular member of one side; and, thirdly, such positions of the body as tend to bring the trunk out of the perpendicular. The account of these, which follows in detail, contains little of absolute novelty, but a very perspicuous account of the more approved views on the subject. The practical part we shall extract, as a very fair specimen of the author's opinions, and as likely to be interesting to our readers.

"Having pointed out these alterations of structure of the spine, we are naturally led to inquire how such changes are to be obviated, and whether they will admit in all cases of a remedy? This must depend both upon the extent of the curvature, and the length of time it has existed. If it be but newly formed, and is only slight, then much may be done to bring back the vertebræ into their natural situation; but if it has lasted a long time, and the curvature is great, it is very doubtful, as it is more than probable that anchylosis has taken place, when it would be highly injudicious to tear

the adhesion asunder. Besides which, also, so much mischief must necessarily have been already done to the bones, or intervertebral substances, that it would be impossible, and, even if it could be done, imprudent, to restore them. Before, therefore, an attempt be made to treat a distortion of the spine, it will be necessary to institute these inquiries.

"In the treatment of distortions of the spine which are unconnected with rickets and the softening of bone, our attention should be directed to the state of the muscles and ligaments. As I have just stated, those on the concave side, immediately connected with the spine, are contracted; whilst those on the convex side are elongated; and thus both are rendered incapable of a proper action: we ought, therefore, to employ those means which will restore the healthy functions of these parts; and this only can be done by bringing them into action. The late Mr. Shaw, in his valuable work on this subject, has most ably pointed out the necessity of the proper exercise of a part for the due performance of its healthy functions; and he has shewn that, by the disuse of the muscles and ligaments, they become wasted and powerless. Daily experience proves this fact to be correct; for instance, we see in diseases of the hip-joint the muscles of the leg wasted and flaccid from want of use, and in fractures, and various other diseases, the same effect produced.

"Before, however, I proceed to treat of the remedies calculated to produce the above effect, I feel it necessary to state that we must bear in mind, that there are very few cases of distortion which do not arise either from weakness of the vertebral column itself, or from weakness of the muscles destined to maintain it in the erect position; that is to say, in the former the spine yields under the weight it has to sustain; and in the latter, from weariness and aching of the muscles, the patient falls into the habit of leaning to one side or the other for relief, and thus throws the spine out of the centre of gravity. Attention, therefore, to the general health of the patient, is of the utmost importance; for as long as the frame is in a debilitated state, we can expect but little benefit from any auxiliary means we may employ to restore it to its natural form. The food should be of the most nourishing quality, avoiding any thing, such as pastry, &c. which may disorder the stomach; the meals should be taken, three in a day, at stated periods, equi-distant from one another, and the bowels should be regulated. The patient should be enjoined to be as much in the air as his strength and the state of the disease will admit of, and tonics may in many cases be administered with advantage.

"According to the pathology of lateral curvature, the treatment must chiefly consist in restoring the muscles and ligaments of the spine to their proper functions, which can only be done by bringing them into use. For this purpose various means have been recommended, and the methods which have been employed are certain exercises by which they are immediately brought into action. Before, however, we proceed to adopt these measures, we must inquire into the cause of this distortion. If it arises from weakness of the spine alone, our chief care should be to relieve it of the weight it has to sustain, which can only be done by lying down. In such a case, however, it would be highly injudicious to confine the patient entirely to the horizontal position, as then the health would suffer, and the muscles, from want of use, would become debilitated, and incapable of supporting the body, by which the very thing we wish to avoid would be accomplished. It would be advisable, therefore, that moderate exercise should be taken, as much as the patient is able to bear, but the greatest part of the day should be spent in lying on the inclined plane, or using such exercises as are compatible with the horizontal position, and which would tend to restore the spine to its normal form.

"When lateral curvature originates from bad habits, such as standing upon one leg; the constant lifting up of one shoulder more than the other, &c., they should be corrected. Before we proceed to the treatment, it is of importance to find out which of these habits gave origin to the deformity, as our means must be directed accordingly; for upon this depends where the primary curve first occurred.

"If a child has been habituated to stand on one leg, the primary curve would of course begin in the lumbar region; but if, on the contrary, it has used the one shoulder more than the other, for instance, to relieve itself from

the shoulder-strap which is constantly slipping off, then, if I mistake not, the primary curve is first established in the dorsal and cervical region. It appears to me, therefore, of consequence to know its origin, as the means we should employ when the primary curve begins in the lumbar region would not be exactly the same as that in the dorsal.

" If it is observed that a child stands on one leg, and inclines to one side, while repeating its lesson, it is a clear proof that the muscles which are destined to keep the body erect, are wearied, and that they are too weak to perform their office. Under these circumstances, it will not be sufficient to correct the child, and make it stand on both feet equally—we must prevent the tendency to such a habit, by not placing it in such a situation as to acquire it. The child should be allowed to sit in a chair with a back to it to lean against, by which it will be supported, or to lie down. If, also, it is observed that a child, who is accustomed to sit upon a stool, falls into the same habit, the same simple means must be employed. We may be assured that it suffers uneasiness or pain, or it would not resort to such means to relieve itself.

" It frequently happens that the habits just alluded to become confirmed, and that the spine has already suffered more or less distortion before we discover it. In such a case, the most simple means of remedying the evil is to make the little patient use the limb on the opposite side; for instance, if the mischief has arisen from standing on one leg, the more the other is used the better, and the best method of gaining this, is to introduce such games for the amusement of children as will effect this object. The favourite game called hop-scotch, is one well adapted for this purpose. Should the habit have been standing on the right leg, which is most frequently the case, then the parent should encourage the child in this game to hop on the left, or *vice versâ*.

" Another simple and admirable plan of exercising the spine, from whatever habit the curvature arises, is, that the child should stand laterally on a semicircular piece of wood made with boards, like the bottom of a rocking-horse, only much higher, so as to allow of a rope being attached on each side to its upper extremity. He should stand with his feet from half a yard to a yard asunder upon this machine, and take hold of the rope with each hand, and thus rock himself backwards and forwards. It will be seen by this movement, that not only the muscles belonging to the lumbar region, but those also of the dorsal, will be brought into action. Hence they will act laterally both on the primary and secondary curve.

" The curvature, from the habit of using the muscles of the shoulder on one side more than the other, will be best counteracted by exercising those of the opposite side. If, from the endeavour a child may make to relieve itself by constantly elevating the shoulder, from the irksome sensation of the shoulder-strap slipping off, the right arm has been most raised, then those games should be played which will bring into action the muscles of the left: for example, the game of battledore and shuttlecock may be played with the left hand, and, in short, any exercise which will bring into use the left shoulder. To relate all the exercises that may be taken in this way would be useless. When the principle is once understood, common sense will at once point them out."

The preceding recommendations are intended as applicable to the cure of slight curvatures; but failing these, recourse must be had to gymnastics, of which ample details are given in various treatises on that subject; and we would particularly recommend to those interested in the best mode of applying them, to peruse the observations of the late Mr. Shaw, in his work on Distortions. Stretching the spine is also adverted to, particularly with reference to the benefit derived from it in the hands of Delpech and Jalade-Lafond.

" The methods they have adopted are two,—a particular kind of machine to be used as a chair, and another as a bed. The patients sit in the former of these, being firmly fixed by a belt strapped round the body above the hips to the seat, and being gradually drawn up by pullies and other machinery attached to the neck and beneath the axillæ on each side; and in the latter they lie on the back, being fixed much in the same manner, only in the horizontal position, and by ropes and pullies are thus extended. The last of these two machines is the best, because it has not to act against the weight of the head and trunk."

The diseases of the medulla and its membranes are spoken of in a separate chapter, and conclude the volume.

A Practical Treatise on the Forms, Causes, Sanability, and Treatment, of Pulmonary Consumption. By Edward Blackmore, M.D., &c.

It is but too true, as Dr. Blackmore very sensibly observes, that much precious time and many invaluable opportunities have been lost by our practical physicians, who have neglected to systematize their knowledge, through an inherent dislike of abstract reasoning, and entertaining a most erroneous impression with regard to theory. Many labour under great mistakes touching the nature of *experience*, "an oracle," as Bishop Berkeley remarks, "to how many inquirers dumb!" "Men do not consider with sufficient attention what it is that constitutes experience," observes Dr. Whately; "so that frequently one man shall have credit for much experience, and another, who perhaps possesses as much, or more, shall be underrated as wanting it. Time alone does not constitute experience; many years may have passed over a man's head without his even having had the same opportunities of acquiring it as another much younger; and again, the longest practice in conducting business in one way, does not confer any experience towards conducting it in another." But not to take up more room with the opinions of eminent men on this point, we must allow that facts in the history of the medical art particularly prove them to be correct. How few Morgagnis, Heberdens, and Gregories have we had to supply us with even the ground-work of a philosophy of medicine! The prefaces and commentaries of the venerable father of pathology in Italy are indeed fine specimens of reasoning and observation, such as are rarely seen in modern works.

In the introductory remarks prefixed to the treatise before us, Dr. Blackmore shows that he understands well the task which he has undertaken. He gives an interesting summary of the views of preceding eminent writers on the subject of phthisis, and accompanies it with many acute observations of his own. On the classification of the species of consumption he is most anxious to be exact, and attaches a due degree of importance to organic lesions as proper to be taken into account along with the purely external symptoms, according to which some writers (Dr. T. Young and Dr. Duncan for example) have thought it right that the various forms should be distinguished. The fact is, that it is impossible to overlook organic lesions in the lungs now as they used formerly to be neglected: the stethoscope has opened such a new world in the pathology of the chest, that it is difficult any longer to say where external symptoms end and internal changes commence. With this proviso, the position of Dr. Young seems to be characterized by the usual enlightened sagacity of that eminent philosopher: and we cannot entertain a rational doubt, that in a new classification of forms of phthisis, he would have availed himself of the *external* symptoms detectable by the stethoscope.

We like the philosophic method adopted by Dr. Blackmore. He displays both his learning and experience in a very conspicuous manner, and has produced, we think, so far as it goes, (for there is to be another volume) the most comprehensive and clear treatise that we have yet seen on this difficult subject. The work is essentially *eclectic*, and necessarily so, as it was expressly designed to be practical; and when finished, it will be found to be, we will venture to say, a complete Cyclopædia of what is known about phthisis. The author has consulted all sorts of authorities, and has drawn amply upon the rich stores contained in many recent, and among them several periodical works—not neglecting, however, our valuable old English writers, whom Dr. B. justly conceives to be too much *shelved*, out of deference to the modern French school of pathology.

We shall be anxious to see the completion of the present work: what we have before us of it mainly consists of a single chapter, which embraces the *four* forms of pulmonary consumption recognized by Dr. Blackmore: and perhaps we cannot do better than give the author's summary account of those forms. "The *first* form comprises the acute hectical florid consumption, ordinarily from tubercles; it is seen chiefly in scrofulous subjects. The *second* form is also seen in the scrofulous: it is sin-

gularly rapid in its progress, latent as it regards the extent of the disorganization in the lungs, with irregular symptoms of an asthenic character, exhibiting the "typhous aspect" of Dr. Rush. It is connected with the softened ulcerous lung. The *third* form is the catarrhal phthisis of various authors from chronic inflammation or ulceration in the bronchial membrane; the cases under this form will be placed in two divisions, the genuine or primary, and the spurious or secondary, where the pulmonary affection is symptomatic on disease in some other part. They are generally chronic in their course, and characterized in their origin or progress, by the symptoms of inflammation in the lung or its investing membranes. The *fourth* form is also chronic, remittent, and inflammatory; related to induration, tubercles, and ulceration in the lung."

It will be observed that the first two forms in this classification are constitutional—the last two local; so that perhaps, pathologically speaking, there should properly be but two forms instead of four. But we think with Dr. Blackmore that there can be no very valid objection brought against his fourfold division, inasmuch as it is so materially subservient to practice, each form requiring a different mode of treatment. Of the treatment of phthisis, however, (which is to form the subject of chapter 4) we must await Dr. B.'s views till the publication of his second and last volume.

MEDICAL GAZETTE.

Saturday, June 23, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

LEGITIMATE OBJECTS OF MEDICAL REFORM.

It is not easy, in these busy times of ferment and innovation, to avoid the occurrence of an occasional thought relative to the possible changes which may take place ere long in the external relations of the profession; nor do we see why the consideration of such a subject should not rather be courted than shunned. Whatever may be the final issue, we have too much faith in the general good sense—in the common discretion of the people of this country, to suppose that any body of legislators should be found so low and stupidly senseless as to prejudice the interests of British medicine. The changes, if any, which may be effected in it, will, we have little doubt, rather consist in the enlargement than the contraction of present privileges: for heaven knows, and every thinking person must candidly acknowledge, that from time immemorial the legislature of England has been but a sorry stepmother in matters relating to the rights of the profession,—allowing, for example, to every vile pretender the utmost degree of liberty and indulgence in his proceedings, or, what comes to the same thing, (if, indeed, it be not much worse, involving as it does a disgraceful mockery,) vesting in the governing medical bodies certain most limited powers of restraining quackery, and at the same time rendering those powers useless, if not odious, by the illiberal interpretation which it ever puts upon their exercise. If to this we add the absurd and barbarous obstacles thrown in the way of the only proper mode of professional education, we need scarcely go further to shew in what respect the aid of reform may be beneficially invoked in behalf of our medical institutions.

And to this extent, we doubt not, will every respectable member of the profession be ready to announce himself a medical reformer. There is nothing in the end proposed, wild, visionary, or revolutionary: it is a purely rational object, the consequences of which cannot but be as beneficial as they are extensive; it is, in short, a plain and perfectly intelligible purpose, implying nothing more nor less than the protection of the public and the

profession from the mischievous doings of impostors.

Nor are the means of accomplishing this so desirable an end in any wise difficult to be encompassed. They are obviously reducible to these three short items:—1, The infliction of positive penalties upon notorious quacks; 2, Extension of the powers of the existing governing medical bodies—qualified with certain material arrangements in their present economy; and 3, A reformed system of medical education, including every facility for the unimpeded prosecution of professional study.

Upwards of two centuries ago, Lord Bacon had occasion to remark on the "weakness and credulity of the multitude, who will oftener prefer a mountebank or a witch before a learned physician:" were his lordship to revisit the earth, he would surely have ample reason to admire the steadiness with which the multitude, both high and low, have adhered to their antique preference in this matter; and while he would discover with delight, that through the mighty impulse given by his philosophy, all classes had profited, acquiring an acuteness of detecting imposture proportioned to the general diffusion of knowledge, yet his mortification would be no less manifested in perceiving that this acuteness existed only in regard to the other branches of knowledge; while in the concerns of the medical art "the multitude" were as much as ever the prey of "mountebanks and witches." Nay, it is quite clear that this national hallucination will go on if not put a stop to by legislatorial interference: long since has England been accounted the hot-bed of quackery, and we have become the laughing-stock of all Europe. What with the licensing of quack medicines by government, and the open patronage bestowed on impostors by the higher ranks of society, it is clearly hopeless to await the operation of reason among the multitude, and positive legislation alone seems adequate to the extinction of the dangerous nuisance. There is something very singular in the condition of the medical art as regards the abuse in question. The Church has its jurisdiction and its standard of orthodoxy perfect, by which at least the disguise of pretenders is stripped off, and it is demonstrated to the world what manner of persons they are. The Law has its tests and its qualifications, by which the admission of notorious empirics among its members is precluded: it must be practised openly, and with the recognition of the proper authorities; and with all this, we find that several energetic steps have been taken of late to secure its still further immunity from intrusion: while Medicine, that art on the proper handling of which life and death are dependent, is, through a mistaken weakness, if not from sheer ignorance or connivance, suffered to fall into the hands of the most ignorant and unprincipled of wretches, who either know not of, or care not for, the *brutum fulmen* provided for their misdeeds in the charters of certain (so called) *regulating* establishments.

Nearly akin to the quacks of whom we speak, is a small, but *select* class of persons, who cry out that medicine is even *too much* restricted at present, and that "free trade" should be the order of the day; but (observe!) who under the mask of resisting monopoly, have the modest assurance to propose a system of traffic of their own, and to put forth a tariff for diplomas and titles at *reduced prices.* But the silly impertinence of these *soi-disant* medical reformers would be utterly beneath notice, were it not for some degree of ludicrousness which is about it.

Here we have positively the very crew, with their famous commander at their head, that attempted to get up a Collegium Wakleyanum a couple of years ago, once more trying to re-

suscitate that defunct chimæra! Not content with the powers that be, nor the light in which they are estimated by the said powers, they must forsooth be admitted to a "rivalry" with existing institutions, the charters of which "they will not seek to expunge." Admirable forbearance! The crew, in short, only seek "a charter" for themselves—a Refuge which they are to govern by their own laws; and they have the unparalleled simplicity to address the public upon the subject, as if any thing coming from the very dregs of the profession could meet with even a momentary notice. Surely, also, they forget the damning name under whose auspices they have embarked in the project. How long shall the existing establishments be without the power of restraining this sort of gentry? How long shall the respectability of the profession be compromised by such antics? How long shall common decency be offended?

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ANATOMY BILL.

The bill is committed at last in the Lords, after having passed the second reading on Tuesday evening, by most miraculous good luck. It was carried by a majority of *five* (out of 25) that it should not be thrown out altogether! Lord Wynford was the proposer of the *amendment* for its rejection; and in the speech which he made, discovered how perfectly innocent he was of the evidence given before the House of Commons some years since in committee. A conversation which he had with Colonel Despard many years ago, when that worthy was under sentence of death, seems to have made a deeper impression upon his Lordship's mind than any thing that he ever heard since. The Colonel, it appears, could not think without horror of his being anatomized after death. We believe, by the way, that the sentence went no farther than *decapitation*. But his Lordship did not tell us *why* Col. Despard disliked dissection: a hundred to one it was because it was vulgar, and considered as an additional punishment, authorized by the law to be performed on murderers only. The condemned criminals, however, (Messrs. Burke, Bishop, Williams, &c.) generally take a far more philosophical view of the matter, and never fail to express their utter indifference at that part of their punishment. Yet horrible as his Lordship seems to think dissection, he had no dislike to its being performed on all condemned felons and suicides; and thus he would supply the schools, and save the bodies of the poor. So much for another specimen of this sort of spurious and half-learned humanity. With the exception of a few words from Lord Brougham, rather deprecatory of discussion, there was nothing in the debate, if debate it may be called, worthy of more particular notice.

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SALINE INJECTIONS IN CHOLERA.

In the present number will be found some very interesting additions to the mass of information which we have previously laid before our readers on the important subject of saline injections into the veins in cholera—a practice first recommended in the pages of this journal. Mr. Smart, of Cranborne, alluded to the subject in a paper dated November 14th; and in our number for December the 3rd, will be found the following editorial paragraph, in reference to this application of the views of Dr. Stevens:—"We earnestly recommend a trial of injecting medicated solutions into the veins, particularly some of the neutral salts—*as muriate of soda.*" We are very far from attaching any importance, however, to the mere suggestion; and, notwithstanding the contemptuous expressions used by some of our contemporaries towards our brethren of the northern capital with re-

gard to cholera, we have no hesitation in ascribing to them the undivided merit of carrying the idea into effect, and thus establishing one of the most curious and interesting facts in modern practice.

We are happy to find that M. Moreau de Jonnès has called the attention of the Academy of Sciences in Paris to the treatment by saline injections into the veins, so that we may hope to see a fair trial given to it in France. By the way, M. Moreau de Jonnès, in acknowledging the receipt of some numbers of this journal, sent to him without our knowledge from an official quarter, does us the honour to observe,—

"J'ai reçu vos cinq numéros de *la Gazette Médicale de Londres.* J'y ai trouvé des articles du plus haut intérêt, et dont je vous fais tous mes remercîmens. Les rédacteurs de ce recueil remplissent avec une digne indépendance la mission de dire la vérité." * * *

COURTEOUS CRITICISM.

When an eminent professional man devotes a portion of his leisure moments to the elucidation of matters requiring research, it might be thought that there could be but one feeling entertained towards him by all parties—that of gratitude and admiration for the noble use to which he applies his dignified retirement. A few weeks ago we noticed the valuable new production of Sir Astley Cooper on the Anatomy of the Thymus Gland, and pointed out some few of its excellencies. It was, then, with some surprise, and, we confess, with no small indignation, that we read in our *courteous* contemporary's last number some passages of which the following is a short specimen:—

"Sergeant-Surgeon Cooper does not enjoy the *otium cum dignitate* of his sinecure. * * * Anxious to dispel the *ennui* which *oppressed* him, Sir Astley was *wont to lounge at his butcher's shop*, where his imagination was *regaled with vivid associations of former days*. On one of these occasions, Sir Astley's attention was excited by a remarkably large thymus gland in a calf."

Such is the sort of silly vulgarity with which it is attempted to sneer at the services of such a man as Sir Astley Cooper. We would merely ask, for what class of readers can such stuff be possibly intended? Surely not for any who would claim to belong to a liberal profession, or to hold the rank of gentlemen in society.

MEDICO-CHIRURGICAL SOCIETY.

May 22, 1832.

Mr. Lawrence in the Chair.

This was the last evening of the session, and the attendance was rather fuller than usual. Two papers were read, of which we are enabled to present the following abstracts:—

Upon Distortion of the Spine and Pelvis, from Rickets; with Illustrations of a peculiar Conformation of the Skeleton produced by that disease. By Alexander Shaw, Esq.

The first portion of this paper was devoted to controverting the opinion sanctioned by several writers of eminence, that rickets may occur at any period of life; an opinion which the author contended was not only erroneous, but very important in its consequences. The author holds, for example, that deformity of the pelvis, so common in rickets, has nothing to do with the common lateral curvature of the spine, commencing about the age of puberty. Having pointed out this distinction, Mr. Shaw proceeded to notice a peculiarity which he has observed in the skeleton of those who have suffered from rachitis. The first circumstance which strikes us on examining such a skeleton, is its diminutive size and the incurvation of the bones; but a more attentive examination of the general figure will shew something more than this, —namely, an absence of the relative proportions which distinguish the form of the natural adult skeleton. The human figure is characterized by the lower extremities possessing a remarkable length, as well as breadth and solidity of form, while the superior parts are comparatively small. Just the reverse holds good with regard to the ricketty subject; the head, chest, and upper extremities, have a bulk and development disproportioned to the size of the pelvis and lower limbs.

With a view to illustrate this distinction more minutely, the author took the measurement of eight deformed skeletons, and contrasted them with corresponding measurements of the natural skeleton. In the former, the bones of the first division, or those above the pelvis, fell short of their due development one-tenth, while those of the

second division, including the pelvis and lower extremities, fell short to the extent of one-third. The head retained nearly its natural size, while the pelvis had its dimensions diminished in the proportion of 1⅓ inch to every eight inches of the natural size, and this in whatever direction the measurements were taken. In making this calculation, the pelves of fourteen ricketty females were examined.

Mr. Shaw next proceeded to offer an explanation of the above phenomena, directing attention to the manner in which the human frame is progressively developed. In the first instance, previous to birth, the body must bear a relation to the act of parturition; and in the second place, it must afterwards become fitted to maintain the erect posture—two conditions which the author holds to be incompatible with the same form; so that a change in the relative development of the parts becomes requisite, after the former purpose has been completed, before the latter can be fulfilled. The distinguishing character of the child's form, when newly born, is the comparative largeness of the head, the capacity of the chest and belly, while the lower limbs are remarkably small; that which distinguishes the adult, on the other hand, is the bulkiness of the lower limbs, as compared to the upper. If the space from the crown of the head to the spine of the ilium be measured during infancy, it will be found to exceed the space measured from the same part of the ilium by one-ninth of the entire length of the body; whereas, in adolescence, instead of the lower division of the body being the shorter, it will be found to exceed in length the parts situated above it by about one-fourth of the entire length. It is therefore to be concluded that the growth proceeds unequally in the upper and lower divisions of the frame—that it advances with more rapidity in the pelvis and lower extremities than in the spinal column, thorax, and upper limbs. Reverting, in the next place, to the peculiarity in the skeleton affected with rickets, it was shewn that the proportions which occasion this are the same as are exhibited when the development of the body is incomplete, or during the period of childhood. He proposes, therefore, this explanation of the conformation in rickets—viz. that when this disease attacks the osseous system, it not only softens their textures and causes distortion of the bone, but likewise retards or deranges the growth. As this happens in childhood, when a remarkable change is taking place on the relative dimensions, it follows that the body must be prevented from acquiring those proportions which distinguish the mature form. In further corroboration of this idea, Mr. Shaw dwelt upon the peculiar nature of rickets, which he represented as a specific disease, in which all the functions suffer derangement. In the osseous system it is not, as has been supposed, the mere deficiency of phosphate of lime which characterizes the disorder, but there is besides a general disorganization of the minute structures, and an irregularity in the deposition of the materials of the individual bones.

He next alluded to the forms of dwarfs and of those above the ordinary stature, with a view of shewing the manner in which the proportions vary. In the former, where the development is slow, the upper half of the body exhibits, at its maturity, the same preponderance which belongs to it at birth, and in this respect the dwarf has a correspondence in figure with the subject of rickets. On the other hand, when the growth has been accelerated, the lower limbs are elongated in an increased ratio, as may be perceived in most persons who are unusually tall. In an individual examined by Mr. Shaw, whose height amounted to six feet four inches and a half, and whose relative dimensions were compared with those of a person of the ordinary stature, it was found that the difference of length in the lower extremities was so much as eight inches, while, on the entire of the superior division of the body, it was only half an inch.

Having completed the above illustration of his views, the author again directed attention to the common lateral curvature of young females, with a view of shewing that not only do the pelvis and lower limbs display their perfect symmetry, but that they expand and grow as in the natural figure at the very time that the spinal curvature is getting daily worse; which state of matters is inconsistent with the idea of this condition being connected with rickets. In conclusion, Mr. Shaw pointed out the importance of the above considerations in reference to midwifery; the mere lateral curvature not influencing the pelvis, but rickets doing so in two ways—first by the obvious change in the shape of the bones, and secondly by their imperfect growth or development. To this last, Mr. Shaw attributes much importance (though it seems to have been overlooked by obstetric writers), and affirms, on the faith of numerous measurements, that the difficulties of parturition experienced by ricketty females, are quite as much dependent upon this latter circumstance as upon the bones being misshapen.

Case of Double Uterus, with Remarks on the Structure of the Human Ovum. By Robert Lee, M.D. F.R.S. &c.

Dr. Lee, in August last, examined the body of a woman who had died, eight days after parturition, from inflammation of the peritoneum and parts of the uterus. She had previously borne several living children. The uterine organs were found to be curiously malformed: the uterus itself was divided into two lateral halves, opening into a

common cervix: the os uteri and vagina presented the ordinary appearances. In the right half (or cornu) had been the fœtus, and the whole inner surface was lined with rough irregular flakes of deciduous membrane. One ovarian, and one fallopian tube, were connected with this cornu, and the same was the case with the other cornu. Both ovaria were enlarged, but the right one was the larger, and contained a distinct corpus luteum. The left cornu had its surface every where coated with a delicate membrana decidua, which formed a short sac at the cervix, but had a smooth circular opening into the fallopian tube. (A wax model and drawings of the parts were exhibited.)

Dr. Lee then proceeded to notice all that was curious and authentic in the works of authors who have described similar malformations. In every case hitherto observed, the uniformity of one fallopian tube, and one ovarium, to each division of the bilocular, bifid, or bicorned uterus, is remarkable. The term double uterus does not therefore seem to be very correct. In the *Maternité* a similar division of the organ is frequently found in the children who are opened. Chaussier describes the case of a woman who died after having had her tenth child; she had but half a uterus, with one tube and ovarium. Vallisneri relates that he found in a woman two uteri, one opening into the vagina, the other into the rectum. All the varieties hitherto noticed, Dr. Lee thinks may be reduced to form: 1, the uterus and vagina, externally regular in aspect, but internally separated into two cavities; 2, the variety above described; 3, the latter, with the addition of a septum in the cervix and vagina; 4, a double os uteri, with a single vagina.

The presence of a deciduous membrane in the unimpregnated cornu has never before been mentioned by any author; but that it always exists, under the circumstances, Dr. Lee considers as highly probable. In the lower animals, when impregnated, the membrane covers the whole inner surface of the uterus. The existence of the membrane renders superfœtation impossible, as well as menstruation, from the unimpregnated cornu.

There is an account of a remarkable case of double uterus in the Philosophical Transactions, vol. 64; it is by Dr. Purcell, of Dublin. The preparation there described was obtained by Mr. Hunter, for his collection, and Dr. Lee was allowed to examine it, with a view to ascertaining whether there was any decidua in the unimpregnated cornu. None were found; but it may have been removed inadvertently, or perhaps had been completely decomposed in the lapse of time.

Opportunities of observing the state of the parts in the human female soon after impregnation, have been few. John Hunter examined the body of a girl who was supposed to have been a month pregnant. There was a pulpy substance, "like the retina of the eye," covering the inner coat of the uterus, (the organ itself being large and soft); but no embryo, even with the help of a magnifier, could be detected. Sir Everard Home, with Mr. Bauer's assistance, found an embryo in the uterus of a girl eight days pregnant, (Phil. Trans. 1817;) but it is more probable that impregnation had taken place here a month or six weeks before. Dr. Lee next gave an account of three preparations which are in the museum of the London University —his information was derived from Mr. Alexander Shaw, who put them up. The preparations are of the uterine organs after recent impregnation, and in all of them the fallopian tubes are open, or present the same appearances as in the unimpregnated uterus. Not until the sixth or eighth week can we ever ascertain the state of the ovum in the pregnant uterus with any degree of certainty. The amnion is then found as a transparent sac, containing the embryo, and the fluid in which it floats. The chorion envelops the amnion with a gelatinous fluid interposed. Both these members are peculiar to the ovum; but the decidua is a production of the lining coat of the uterus. Dr. Wm. Hunter has given an accurate description of the decidua, in his "Gravid Uterus." The opinions of Professor Burns, Velpeau, and others, relative to the formation of the deciduous membrane, are well known. Dr. Lee's conclusions from various facts which he adduced were, that the fallopian tubes are open during the early months of gestation; that the ovum may attach itself to the fundus, body, cervix, or even over the centre of the os uteri; and that the decidua forms neither a shut sac nor inorganic layer before or subsequent to the arrival of the ovum in the cavity of the uterus The soft flocculent albuminous matter which at this period lines the inner surface of the uterus and envelops the ovum, becomes gradually converted by a peculiar vital process, essentially different from inflammation, into those delicate membranous layers and vascular structures by which the embryo is nourished and sustained in the uterus.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

LONDON HOSPITAL.

*Another case of Hydrophobia**.

A REMARKABLY fine healthy little boy, aged four years, was brought into the hospital from Bow Common, June 13th, about four o'clock in the afternoon.

* In the case of hydrophobia, at the London Hospital, which we published last week, the quantity of laudanum in the enema was stated to be "two drachms;" it ought to have been *six drachms*.

About seven weeks since he was bitten in the face in several places by a dog; he was then seen by a medical man, who recommended the bitten parts to be excised, but the parents objected to the operation. The dog, after attempting to bite some other persons, was destroyed, and in its stomach were found pieces of sticks and straw. Two days since, the boy complained of heat and uneasiness about the face; and yesterday morning he was dull and sleepy, and had a slight aversion to taking fluids: in the evening he could hardly be induced to drink at all, and then only a small quantity. The child slept little during the night, and continued very restless.

When he was admitted, his look was wild; he appeared to dread any one approaching him, and the slightest noise would make him start up in bed: he was restless, and constantly changing his position; respiration convulsive; when water was offered to him, he pushed it violently away, and placed his hand to his throat as if he felt uneasiness there; tongue and lips dry; pupil dilated.

Ordered half a grain of the acetate of lead, to be taken every half hour. Three leeches to each side of the neck.

Vespere, 10 P.M.—The child was less restless; pulse 124. The medicine had been given him in a medicine-spoon, and, by means of a little force, the greater part of each dose was swallowed.

One grain of the acetate of lead was directed to be given, with ten drops of laudanum added to each dose, every half hour.

During the night the symptoms became aggravated: he was lying naked in the bed, and would not permit any clothes to cover him. He took nine doses of the medicine in the course of the night; after which he seemed to suffer from pains in the bowels and limbs; consequently it was discontinued. At seven o'clock in the morning an enema, containing castor oil and forty drops of laudanum, was administered.

14th, 1 P.M.—He is now much weaker, and lies delirious in a state of nudity on the bed, continually crying out; he is constantly picking his lips, which are covered with blood and viscid mucus; pulse 150, with very little power; eyes dim, and continually rolling upwards. The child gradually sunk, and died a little before four in the afternoon. The parents would not permit an examination of the body.

Case of Rumination.

David Hunter, aged thirty-nine, living in Bethnal Green, applied recently at the hospital for relief as an out-patient, under the following circumstances. About a quarter of an hour or twenty minutes after every meal, the food which has been taken into the stomach is brought up into the mouth, to be subjected to a second process of mastication. He has been accustomed to this ever since he was five years of age. His bowels are regular, and he has always enjoyed excellent health. It is completely an involuntary act, and is unattended with any feeling of sickness, being rather pleasant than otherwise. It takes place in a greater degree after taking animal food, being small in quantity after a meal consisting of vegetables. The man was formerly a patient of the late Mr. Headington, who mentioned the case in his lectures. He was induced to apply for relief in consequence of the habit being offensive to his companions. Some tonic medicines were prescribed for him, from which he experienced no benefit.

REPORT OF CHOLERA, UP TO FRIDAY, JUNE 24, 1832.

New cases in Great Britain (exclusive of London) since May 25, the date of our last report	1909
Deaths.....	721
Total number of cases throughout Great Britain (inclusive of London) since the commencement ...	14746
Deaths.....	5932

CHOLERA IN IRELAND.

There must be some desperate mismanagement on the part of the Irish Government, or of the Irish Central Board, as there is such a general complaint of a lack of medical assistance in all the provincial towns attacked with cholera. Dublin, we know, abounds with practitioners, and experienced ones too; yet we find that Drogheda, a place but twenty miles from the capital, was lately left trusting to a single medical man, when the disease was cutting off the people there by hundreds. In Tullamore, where, according to the latest accounts, the cholera has carried off above 200 already, *there has not been a single case of recovery.* Why is there not a plentiful supply of hands sent into the provinces?

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

June 1832.	Thermometer.		Barometer.	
Thursday . 14	from 52 to	71	29·56 to	29·66
Friday. . . 15	51	67	29·86	29·92
Saturday . 16	51	70	29·96	30·03
Sunday . . 17	51	73	30 03	Stat.
Monday . . 18	53	77	30 04	30·06
Tuesday . 19	50	74	30·06	30·02
Wednesday 20	51	74	29·96	29·92

Prevailing wind S.W.

The 14th, 15th, and 20th, cloudy; rain at times on the 14th, 15th, and 17th; otherwise generally clear.

Rain fallen, ·325 of an inch.

Charles Henry Adams.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 30, 1832.

ON CLINICAL INSTRUCTION;

WITH A

Comparative Estimate of the mode in which it is conducted in the British and Continental Schools.

By Robert J. Graves, M.D. M.R.I.A.

King's Professor of the Institutes of Medicine, one of the Physicians of the Meath Hospital, &c. &c.

To the Editor of the London Medical Gazette.

Sir,

The following lectures* upon clinical instruction were delivered at the Meath Hospital in 1821. Since that time I have witnessed with pleasure the introduction of many improvements in medical education, and have only to regret that the particular department to which I drew the attention of my class has not received a corresponding degree of cultivation; indeed, strange as it may appear, the means of improving himself in the preparatory branches of the profession are much more numerous, and much better arranged, than those which the student possesses of acquiring practical knowledge. It is time that this error should be corrected; it is time that those whose duty it is to teach should seriously and anxiously apply themselves to this subject. I had hoped that the interesting controversy between Clark and Tommasini, that the observations of Otto, Autenrieth, and Frank, upon the defects of medical education in Great Britain, would have stimulated my countrymen to the adoption of the desired alterations in the mode of conducting clinical instruction, and would have rendered any efforts of mine unnecessary. This hope, however, has not been realized, and consequently I feel myself bound to make, through the medium of your valuable and influential journal, an attempt to draw the attention of our schools and universities to this important topic.

Yours, very truly,

Robt. J. Graves.

Dublin, June 8, 1832.

Gentlemen,—Before we commence an examination of the cases at present in the medical wards of this hospital, it is necessary to explain the method of instruction which I mean to adopt. Employed elsewhere in learning the principles that constitute the basis of medical education, you ought to be impressed with a precise notion of the peculiar objects and utility of hospital attendance; you come here to convert theoretical into practical knowledge; to observe the symptoms of diseases previously known to you only through the medium of books or lectures; to learn the art of recognizing these symptoms, and of appreciating their relative importance and value; to study their connexion with morbid alterations of internal organs; and, finally, to become acquainted with the best method of relieving your patients by the application of appropriate remedies.

Such, gentlemen, are the objects you seek in coming here; and in proportion to the number and importance of these objects, are the degree of responsibility attached to your clinical instructors, and of blame to yourselves, should the opportunities which this institution offers for your benefit be neglected.

The other branches of medical education may be cultivated at different times, and according to a certain order of succession—one period of your studies demanding a particular application to anatomy, another to chemistry, while a third must be especially devoted to materia medica. With the observation of disease it is otherwise. From the very commencement, the student ought to witness the progress and effects of sickness, and ought to persevere in the daily observation of disease during the whole period of his studies.

The human mind is so constituted, that in practical knowledge its improvement must be gradual. Some become masters of mathematics, and of other abstract sciences, with such facility, that in one year they outstrip those who have laboured during many. It is so,

* The lectures are here incorporated into one.

likewise, in the theoretical parts of medicine; but the very notion of practical knowledge implies observation of nature; nature requires time for her operations; and he who wishes to observe their development will in vain endeavour to substitute genius or industry for time. Remember, therefore, that however else you may be occupied—whatever studies may claim the remainder of your time, a certain portion of each day should be devoted to attendance at an hospital, where the pupil has the advantage of receiving instruction from some experienced practitioner. A well-arranged, and sufficiently extensive hospital, contains every thing that can be desired by the student; but, unfortunately, his improvement is seldom proportioned to the opportunities he enjoys. Whence this deficiency? How does it happen that many attend hospitals day after day, and year after year, without acquiring much practical knowledge? This may be attributed to want of ability or diligence on the part of the student, or to an injudicious or careless method of teaching on the part of the hospital physician. It may be well to examine more in detail the errors to which the student and the teacher are respectively most exposed.

A great number of students seem little, if at all, impressed with the difficulty of becoming good practitioners; and not a few appear to be totally destitute of any prospective anticipation of the heavy, the awful responsibility they must incur when, embarking in practice, the lives of their fellow-creatures are committed to their charge. It is by persons of this description that the earnest attention, and permanent decorum, which ought to pervade a class employed in visiting the sick, are so frequently interrupted. Young men of the character to which I allude, attend, or as it is quaintly enough termed, *walk* the hospitals very regularly, but they make their appearance among us rather as critics than as learners; they come, not to listen but to speak; they consider the hospital a place of amusement rather than of instruction. I am happy to be able to state that such characters are not very numerous here, for this hospital possesses no other attractions, confers no special qualification beyond the knowledge which may be obtained within its walls*.

* Since this was written, the Meath Hospital became for several years a privileged hospital. Latterly this premium upon idleness has been again withdrawn from us, and I most heartily rejoice that this and other hospitals have ceased to form a sort of favoured oligarchy to the exclusion of the less extensive institutions of this city; every thing like monopoly tends to retard the advancement of science, and I see no reason why an hospital with 50 beds should be inferior to one with 100. It is not the quantity of disease a teacher treats which renders his lessons instructive: his diligence and accuracy of observation are the best means of instructing the pupils.

Of those who are anxious to learn their profession, a great number fail, and are found wanting when their studies are finished; in a few, the failure may be traced to a deficiency of intellectual powers; but in the majority it is owing to their studies being erroneously directed. Thus I have known many who have displayed a taste for the study of the progress and treatment of acute diseases, while they paid but little attention to complaints of a chronic nature. This predilection is not confined to students; professors and authors in general seem to participate in this taste; and, consequently, we find that acute diseases form the favourite subjects of clinical lectures, and occupy the greatest portion of medical literature,—and that for obvious reasons; for if the course of acute diseases, such as fever and the phlegmasiæ, be compared with that of chronic maladies, we shall find that the former begin, continue, and end in a manner comparatively so regular and definite, that their progress can often be accurately predicted, and their terminations foreseen,—a circumstance which enables us not only to predict the event with confidence, but obtain, by the well-timed application of active remedies, relief, evidently the result of the means employed, and, consequently, reflecting credit both upon the physician and the art of medicine. How satisfactory are our feelings on arresting the progress of pneumonia by venesection, or tranquillizing the mania of delirium tremens by means of opium! Far different is the case with chronic diseases; in their commencement generally obscure, insidious, and irregular; in their terminations necessarily uncertain; frequently transferring themselves, as it were, from one part of the system to another, occasioning unexpected and anomalous symptoms, and involving in their destructive course almost every tissue of the body. From the very length of their duration, they are also more liable to be modified by new physical and moral influences, affecting either the mind or body: and are, in a word, more closely leagued with time, the parent of mortality. In the treatment of such affections, the greatest judgment and patience are requisite; there is here no room for the application of *heroic* remedies; nor can the physician expect, from his most persevering exertions, that speedy benefit by which he acquires eclât in acute cases, for it must be remembered that chronic diseases require chronic remedies.

This most difficult department of medicine surely claims not the least portion of your attention, and you will attach more importance to this subject on considering that a knowledge of chronic diseases is essential to the surgeon, inasmuch as those who labour under them remain exposed to accidents which constitute his peculiar province*.

* At the time this lecture was written, the ab-

Many students fail from another cause: instead of studying the most common, and on that account, the most important diseases, they acquire a taste for observing and relating singular and rare cases, as if their chief object was to obtain a store of curious medical information. Let me warn you against this amusing, but comparatively unprofitable employment of your time. Suffer not yourselves to be misled by those who prefer the gratification of an idle curiosity to the laborious investigation of ordinary diseases.

Students should aim not at seeing many diseases every day, not at visiting daily numerous cases; no, their object should be constantly to study a few cases with diligence and attention; they should anxiously cultivate the habit of making accurate observations. This cannot be done at once; this habit can be only gradually acquired. It is never the result of ability alone; it never fails to reward the labours of patient industry. You should also endeavour to render your observations not only accurate but complete; you should follow, when it is possible, every case from its commencement to its termination; for the latter often affords the best explanation of previous symptoms, and the best commentary on the treatment. Did time permit, I could expose many other erroneous practices calculated to render your studies comparatively unprofitable; but I must turn from the student to the teacher—from the errors of the learner to the imperfection of the mode adopted for instructing him.

I have had an opportunity of observing with attention three different methods of conducting clinical instruction: the first is that practised in Edinburgh and Dublin. I shall select that of Edinburgh for examination, being by far the most celebrated of the British schools of physic, and much resorted to even by foreigners for instruction. Two clinical clerks, one for the male, another for the female wards, are selected by the physician from among the senior pupils; their business is to write an accurate history of the cases, to report the effects of medicines, and record the symptoms which may have occurred since the physician's last visit. All this is generally done with fidelity and zeal. At his daily visit the physician stops at the bed of each patient, and having received the necessary information from his clerk, he examines the patient, interrogating him in a loud voice, while the clerk repeats the patient's answer in a tone of voice equally loud. This is done to enable the whole audience to understand what is going on; but indeed, when the crowd of students is considerable, it is no easy task; it requires an exertion almost stentorean to render this conversation between the physician and his patient audible by the more distant members of the class; while the impossibility of seeing the patient, obliges all who are not in his immediate vicinity to trust solely to their ears for information*. This information is not indeed neglected, for every word so attentively listened to, and heard with so much difficulty, is forthwith registered most faithfully in each student's case-book; and afterwards all the observations the professors make in their clinical lectures are taken down with equal care and fidelity. It is really a pity to find so much labour and diligence thrown away; for it is evident that the practice of medicine cannot be thus taught or learned, as it were, by hearsay; and it is consequently to be feared, that many are annually dubbed Doctors at Edinburgh, who have been scarcely ever called on to write a prescription. The chief objection to this mode of teaching is, that however well inclined the student may be, he is never obliged to exercise his own judgment in distinguishing diseases, and has no opportunity of trying his skill in their cure; and, consequently, at the end of his studies he is perhaps well grounded in the accessory sciences—is a perfect medical logician—able to arrange the names of diseases in their classes, orders, and different subdivisions; he may be master of the most difficult theories of modern physiologists; he may have heard, seen, and if a member of the Medical Society, he may have also talked a great deal; but at the end of all this preparation, what is he when he becomes a full Doctor?—a practitioner who has never practised!

I do not assert that a diligent student may not obtain a good deal of knowledge by attending one or several clinical courses in Edinburgh; no doubt he will gain many useful general ideas concerning the nature and treatment of disease; and if he himself examines the patient after the physician's visit, he may even acquire a certain degree of tact in recognising symptoms and appreciating their value. This method of instruction is indeed very useful, and nothing better can be devised for a beginner; but for the more advanced student it is by no means sufficient, nor is it calculated to give him practical experience, without which all other acquirements are of no avail. I say it does not give him experience, because he has at no time been charged with the responsibility of investigating a case for himself, and by himself—because at no time has he been called on to make a diagnosis unassisted by

surd idea that the education of a surgeon should differ from that of a physician, had not been altogether abandoned.

* When this information was conveyed, as it formerly was at Sir P. Dun's Hospital, in Latin, the student had to encounter another barrier to the acquisition of knowledge. I have called the language *Latin*, in compliance with the generally received opinion concerning its nature.

others—and above all, because he has never been obliged to act upon that diagnosis, and prescribe the method of treatment. If those who have been thus educated, and who have been made doctors upon so slender a foundation, were to confess the truth, we should be presented with a picture calculated to excite dismay, if not a stronger feeling. How many doubts and distracting anxieties attend such a man at his first patient's bedside? If the disease be acute, and life in imminent danger, if he shrinks under this sudden and unusual load of responsibility, he gains little credit for professional ability; if, on the contrary, inexperienced as he is, he assumes that decision of judgment, that energy of practice, which experience alone can confer, it is not improbable that the result may be still more disastrous.

Gentlemen, I am not drawing a picture from my imagination alone; I have had occasion too often to shudder at the original—too often to deplore the sad effects resulting from the well-meant but totally mistaken treatment employed by young men; and often have I regretted that, under the present system, experience is only to be acquired at a considerable expense of human life. There is, indeed, no concealing the truth, the melancholy truth, that numbers of lives are annually lost in consequence of mal-treatment. The victims selected for this sacrifice, at the shrine of experience, generally belong to the poorer classes of society, and their immolation is never long delayed when a successful candidate for a dispensary commences the discharge of his duty. The rich, however, do not always escape; nor is the possession of wealth in every instance a safeguard against the blunders of inexperience. This charge of inexperience is not necessarily confined to the beginner; it applies equally to many an old practitioner, whose errors have grown, and have increased in strength, during a long succession of years; because, from a defect in his original education—from the absence of a properly directed clinical instruction, he commenced practice without having previously acquired the power or the habit of accurate observation; because he had not in his youth been taught to reason justly upon the facts presented to his view; because, not having learned in the beginning to think accurately, he contracted a loose and careless mode of examining the progress of disease, and the effects of remedies; and, consequently, the lapse of time has had no other effect upon his errors, than that of rendering them more inveterate. Such a man has generally an overweening confidence in his own judgment; he never detects or is conscious of his own mistakes; and instead of improvement, years bring only an increased attachment to his opinions—a deeper blindness in examining the results of his own practice; and do not such persons abound in every branch of the profession?—are there not general practitioners, are there not physicians, are there not surgeons, are there not apothecaries, who answer to this description, and who nevertheless are cheerful in their demeanour, and enjoy a good repute among their clients? Believe me, gentlemen, the quacks who cover our walls with their advertisements vend not annually to the community more poison than is distributed according to the prescriptions of your routine and licensed practitioners;—and yet the science of medicine is improving daily, and treatises on the practice of physic are every day multiplying. Why, then, is society so infested? Many circumstances concur to produce this effect; but the most influential is undoubtedly that which now occupies our attention: I mean a system of clinical instruction radically wrong, because it does not teach the actual practice of medicine. Is there any other profession or art, or even trade, in which any but a madman would embark unprovided with a store of practical knowledge? But enough of this unpleasing subject. Let us next consider what systems have been adopted in other countries, with a view of judging how far it is either practicable or expedient to introduce them into this*.

In France, the mode of conducting clinical instruction is very similar to that which we have already described, and consequently it is attended with nearly the same advantages and defects. In the French hospitals, however, no reports are dictated to the clerks, and more care is taken to explain the symptoms and progress of each case at the bed-side of the patient; in fact, these explanations answering to the original institution and design of clinical lectures, are attended with many important advantages, and are well worthy of imitation. By this means the trouble and uncertainty of a circumstantial and detailed description are frequently avoided by a direct reference to the matter to be described; and

* As truth has obliged me to expose a fault, which the Edinburgh school shares in common with the other schools of Great Britain, I am bound in candour to acknowledge the very great advantages which Edinburgh, in other respects, offers to students; they there find themselves surrounded by so much diligence, enthusiasm, and zeal, that they can scarcely resist the impulse of improvement, and consequently many learn there to think and to labour, who had been previously careless idlers. Were it possible to learn practical anatomy in Edinburgh, and were the mode of conducting clinical instruction improved, what might not be effected in a university otherwise so eminent? In looking over the list of professors, we find the illustrious names of Black, Cullen, Duncan, Gregory, Home, Monro, &c. &c. Several of these names occur in the list in the dual number, and one in the plural: it is not easy to reconcile this hereditary possession of certain professorships with purity of election. The mantle of genius rarely falls from the father upon the son in other countries: it may be different in Scotland.

the interest of the student is secured by a very slight exertion on the part of his instructor, while the latter owes many new ideas to the degree of attention which he is thus forced to give each case. It is true that the duration of the visit is thereby increased; and in Italy, where the same plan is pursued, it is not unusual for Tommasini to expend, in the morning, more than two hours upon eight or ten cases, besides the time devoted in the evening to the same purpose. Where the importance of the subject to be taught is so great, it is wisely judged that the teachers must be laborious; and it is thought necessary to use every possible means to convey clear ideas concerning each case to the student. His attention is not distracted by seeing a great number of cases in rapid succession, nor (as is too often the case in the hospitals of Dublin and London) are the inquiries dictated by a laudable curiosity on the part of the student, suppressed by a forbidding demeanor or an uncourteous answer from his teacher *. Although the French clinic thus presents several manifest superiorities over the British, yet it is liable to the chief objection already urged against the latter—that the student is not supplied with an opportunity of learning the actual practice of his profession. I am by no means disposed to join in the cant of humanity; yet I cannot overlook another disadvantage of this mode of teaching. I cannot help feeling that it is scarcely justifiable to lecture upon a patient's case in his presence and in his native language; that it is cruel to explain (as must, when this method is adopted, be often done) that the patient is labouring under a fatal complaint. During such a lecture, I have often watched the worn and pallid countenance of the sufferer, while he listened attentively to the record of his past and present sufferings, and I have marked the settled expression of despair it assumed when the prognosis thus tediously ushered in was too clearly announced. It is cruel to banish from the sick man's bed his sole remaining comfort—it is unmerciful to scare away hope, his only consolation during hours of pain and watching. We ought never to allow any expression to escape from us which could possibly add the terrors of apprehension to the weight of actual suffering. On this account, while we borrow the useful part of their system from the French, we must correct so glaring a defect by making use of the Latin language, whenever it is absolutely necessary to make any observation that might alarm the patient *. One of the most important duties of a surgeon, or physician, consists in the practice of humanity; and it is very doubtful whether the student does not experience as much difficulty in deriving benefit, not so much from the precept as the example of his seniors, in this department of his profession as in any other. Observe, gentlemen, I speak not of French but of Irish hospitals; for, with the exception of the objection already adverted to, the conduct of the French medical men is in every respect praiseworthy. We do not find them indulging in coarse, harsh, and even vulgar expressions to their hospital patients; we do not find them provided with two vocabularies—one for the rich, and another for the poor †. The medical, more than any other profession, requires that the better feelings of our nature should be cultivated and fostered. The nature of anatomical pursuits obliges us to violate many of our natural prejudices, and disregard some of our strongest propensities; let us therefore be doubly anxious to give, by means of the most diligent cultivation, an additional and more vigorous growth to our better feelings—to our social affections;—and if we are accused of disrespect for the dead, let us answer the accusation by our humanity to the living.

But to return to our subject. The third mode of conducting clinical instruction, is that adopted generally throughout Germany; and which, in addition to the means of improvement, comprehended in the plan of the French and English methods, possesses the advantage of allowing the more advanced students to undertake the care of patients in the hospital, under the direction of the attending physician.

The importance of clinical instruction is so much felt in Germany, that each school has three distinct medical clinics attached to it, by which means the labour of teaching is divided among the professors, and the number of students attending each is diminished. There is one clinical hospital for

* In this respect our hospital physicians and surgeons have improved much since 1821. I am strongly disposed to believe that the improvement was not owing to a voluntary change, but to a certain salutary fear of public castigation from the weekly medical press; much, however, remains to be done, for the influence of the last century has not yet entirely ceased, and there are those still lingering among us, who no doubt regret the aristocratic era, when an impassable gulf lay between the student and his teacher.

* This rule is always observed in Germany, a country remarkable for the zeal and humanity of the medical profession. In Italy both professors and students are less scrupulous. Thus Dr. Clark relates that he has heard the case of a phthisical person explained, in all its bearings, by the professor at Bologna, in the patient's presence: in another instance, which occurred at the same place, a female, labouring under cancer uteri, burst into tears on hearing a detailed account of the nature of her complaint!

† When the above lecture was delivered, the abuse I speak of was but too frequent; and will it be credited that many other and greater abuses had existed during the preceding generation? Death, the most efficient of all reformers, had then removed several of the chief actors from the scene, for which, as on most other occasions, he has, I rather think, been undeservedly censured.

the treatment of acute diseases, and another for chronic diseases, while a clinical dispensary is devoted to the care of extern patients. The pupils are divided into two classes—the more advanced, who get the care of patients, and the junior students, who merely look on and listen. When a patient is admitted, his case is assigned to one of the practising pupils, who, when the physician is visiting the ward, reads out the notes he has taken of the patient's disease, including its origin, progress, and present state. This is done at the bedside of the patient; and before he leaves the ward, the physician satisfies himself whether all the necessary particulars have been accurately reported by the pupil. After all the patients have thus been accurately examined, the professor and his class proceed to the lecture-room, and a list of the patients and the practising pupils is handed to the professor: the cases admitted that day are first inquired into, and the pupils are examined concerning the nature of their diseases, their probable termination, and the most appropriate method of treatment,—each student answering only concerning the patients entrusted to his special care. During this examination, the pupil's diagnosis and proposed remedies are submitted to the consideration of the professor, who corrects whatever appears to be erroneous in either, and then the student retires to write his prescriptions, while the rest of the cases and pupils undergo a similar examination. At the conclusion, the prescriptions written by the students are read out in order by the professor, who strictly comments on and corrects any inaccuracy or inelegance they may contain. When the prescriptions have been revised and corrected, they are signed by the physician, and handed to the apothecary to be made up and distributed. In some clinics, the price of each medicine is affixed to the bottle or box containing it, in order that the students may become acquainted with the comparative expense of various prescriptions, and may thus be enabled, in private practice, to accommodate, as far as is possible, the expense of the remedies to the circumstances of their patients. The clinic for extern patients is conducted on the same principles: patients who are able to attend, are examined at the dispensary; those who cannot leave their homes are visited by the senior practising students, who always seek the advice of the professor when the case is urgent, or the treatment doubtful. Nothing, gentlemen, can be better adapted than this plan of clinical instruction for the improvement either of the beginner, or of the more advanced student: this daily deliberation and anxious discussion concerning the nature and treatment of each case, is peculiarly interesting, and serves to accustom the beginner to habits of accurate examination, whereby he is taught to interrogate nature for himself, and learns the history and treatment of disease, not from books and descriptions, but from direct observation. The advantages gained by the practising pupils are too obvious to require comment: being obliged to give reasons for every plan of cure that they propose, they are accustomed to a rational and careful investigation of disease; and enjoying the most important of all advantages—the early correction of their errors—they commence private practice with a sufficient degree of experience to render them unlikely to commit any very serious mistakes.

It is evident that, according to the German method, no regular clinical lectures are necessary, as the pupil becomes accurately acquainted with the physician's views of each case, and no step is taken in the treatment without the reasons for it being given. This is the best sort of clinical lecture; the pupils have their doubts solved, and their erroneous views corrected, while the professor is enabled to mention, as the disease proceeds, every thing which he thinks is illustrative of its nature.

Note.—Eleven years' experience enables me strongly to recommend the method of instruction pursued in Germany. Since my appointment to the Meath Hospital, I have had extensive opportunities of observing its good effects. Not a session has elapsed without furnishing proofs in its favour. This system, however, at first met with much opposition, and its introduction was ridiculed in every possible manner; even now it may be doubted whether its well-wishers are as numerous as might be expected. It is still opposed by several narrow-minded persons, whose opinions have much weight with the pupils.

I remember perfectly well having only two practising pupils in one class, but I was not discouraged; and although we have had many numerous classes at the Meath Hospital, I doubt if any of them contained more talent and worth than was shared between my two pupils, Dr. Townsend and Dr. Stokes.

Since the latter, from being my pupil, has become my colleague, he has evinced the most indefatigable zeal in co-operating with me in instructing the pupils of the Meath Hospital; and I am sure he joins me in testifying the constant gratification we have received from observing that our efforts have been so far successful, that no season elapses without bringing under our immediate observation several pupils whose diligence, zeal, and moral worth, insure our warmest approbation. Many of these gentlemen have already distinguished themselves,—and will always carry with them the best wishes of myself and my colleague.

ON FEVER.

To the Editor of the London Medical Gazette.

SIR,

THE theory which assigns to fever an invariably local origin, and which regards it as merely identical with inflammation, is, if I mistake not, losing ground among the profession. Cases are every day occurring which do not, during life, present a single symptom of inflammation, and in which, after death, no trace whatever of that morbid condition can be discovered. This indisputable fact, if it be not entirely overlooked, necessarily leads to the conclusion, that however frequent in fever an inflamed state of certain organs may be, that state is not essential to the existence of the disease, of which, instead of being the cause, it is very often nothing more than an accidental consequence. At the same time, no practitioner of even moderate experience can be ignorant that the state of simple excitement attendant upon certain local inflammations, and constituting what is called *symptomatic fever*, does very frequently become converted into true *idiopathic fever*, merely from the long continuance of the inflammatory action. It would thus appear that fever may exist either independently of inflammation, or as a consequence of it; but in this latter case it is, I believe, not less than in the former, a general disease, implicating the whole of the system. This opinion I shall endeavour to illustrate by a few brief observations.

If we consider attentively the recognized causes of fever, we shall find that they one and all exert, by either directly or indirectly, a powerful sedative, or debilitating influence on the brain and nervous system. Some of these causes, such as human or marsh effluvia, certain passions of the mind, &c. would seem, both from their nature, and from the suddenness with which they occasionally produce their effects, to act directly; while unwholesome, or insufficient diet, which is undoubtedly a very common cause of fever, would appear to operate rather in an indirect manner, affecting the brain through the medium of the blood, which is either very considerably diminished below the standard quantity of health, or is so altered in its qualities as not to afford the proper stimulus to that organ. But even in this latter case the brain is nevertheless the primary seat of the fever, for until it becomes implicated, nothing more than mere debility exists.

Having thus seen how causes, so different in their nature, and in the manner of their operation, are equally productive of fever, we cannot, I think, have much difficulty in comprehending how some local inflammations may give rise to the same disease. The brain, it has been shewn, is the part which first suffers, and the manner in which it suffers is by having its active powers diminished to an extent incompatible with the healthy performance of its functions. What then, even reasoning *à priori*, would be more probable than that inflammation of the cerebral substance, or of the stomach, or bowels, would, when of a proper degree of intensity, and when long enough continued, exhaust the energy of the nervous apparatus, and thus occasion a state of the brain precisely similar to that induced by the causes previously mentioned? How correct such reasoning would be is rendered sufficiently manifest by experience, which frequently presents us with cases of fever originating in these causes.

But in the cases just alluded to, the local inflammation, and the fever which results from it, are distinct, and totally different affections, and have no more right to be considered identical than any other cause and effect have. Nor is there greater propriety in applying the epithet, *local*, to fever when thus produced, than when it owes its origin to the action of a morbific poison, since the influence of this must, in the very commencement, be confined to some one particular part. Fever, in fact, by whatever cause produced, is invariably a *general* disease, affecting, in a greater or less degree, every organ, and every tissue of the body. An attentive consideration of the numerous and varied symptoms of the disease will be sufficient at once to render evident the truth of the above proposition, and to satisfy the mind that they could be simultaneously produced by no other cause than by an affection of the brain and nervous system, such as that to which I have alluded.

The first obvious and leading phenomena of fever is prostration of all the powers, mental as well as bodily. The

patient feels dull, and is averse to any active employment; there is a feeling of partial, or of universal chilliness; the skin becomes pale; the countenance, even at this early stage, loses whatever of animated expression it before possessed, and wears instead a look of intense anxiety. The pulse is quick, soft, and weak; nausea supervenes, and frequently vomiting, with pain in the head, back, and several other parts of the body. Respiration, too, is affected; the muscles which assist in augmenting the capacity of the thorax performing their office imperfectly, and thus giving rise to a feeling of great oppression.

A train of symptoms, resembling those just described, though seldom equally extensive and general, attend upon the commencement of almost all inflammatory affections; and depend upon a similar cause, namely, a derangement in the functions of the nervous system. But in these cases the derangement is merely temporary, the result of that confusion in the system which necessarily accompanies the establishment of a new (the inflammatory) action. In fever, on the contrary, the same phenomena are produced by actual exhaustion of the nervous powers, a cause which must continue to operate for a considerably longer period.

From the symptoms above enumerated as marking the incipient stage of fever, it must be manifest that the three grand functions of innervation, circulation, and respiration, are all more or less affected. We shall afterwards see that the functions of secretion and excretion are likewise implicated in the disease. These facts sufficiently prove the general nature of fever, and, in conjunction with the order in which the several symptoms occur, point distinctly to the brain and nervous system as its primary seat.

The next step in the progress of the disease, after the appearance of the symptoms which I have described, is one which deserves to be particularly considered, both as it confirms the opinion which has been here advanced respecting the proximate cause of fever, and as it is to its operation that one of the most prominent, and important phenomena of the disease, ought, if I am not greatly mistaken, to be attributed. The step to which I here allude is, in most cases, the suppression, and in all, the alteration of one kind or other that takes place in the various processes of secretion. But before proceeding to state in what way this symptom affords a confirmation of my views respecting the proximate cause of fever, and to explain the influence which it exerts on the subsequent progress of the disease, it will be necessary for me to advert to statements made by both Doctors Smith and Tweedie, in their respective works upon fever.

After observing, in his excellent "Clinical Illustrations of Fever," that the brain and nervous system are early and primarily engaged in the febrile action, Dr. T. thus proceeds:—"The circulation next partakes in the disorder; there is generally, though not invariably, quick pulse and heat of skin, to which, as a consequence of the previous condition of the sensorium, succeeds a vitiated state of the secretions," &c.

An equally short passage from Dr. Smith's work will suffice. "The order of events then is, first, derangement in the nervous and sensorial functions; this is the invariable antecedent. Secondly, derangement in the circulating function; this is the invariable sequent: and thirdly, derangement in the secreting and excreting functions; this is the result in the succession of morbid changes."

Now I am perfectly aware, that, in attempting to question the accuracy of two writers upon fever of such acknowledged talents, and of such extensive experience in that disease, I lay myself open to the charge of excessive presumption. Close and repeated observation, however, has thoroughly convinced me that the order in which the several symptoms succeed each other, is not exactly that described in the two paragraphs just quoted. I have the firmest belief that the derangement which takes place in the functions of secretion and excretion, invariably precedes the stage of excitement. It would, therefore, be nothing more than affectation of modesty, to give up my own well-grounded conviction out of deference to the authority of any names, however highly reputable. Nor have I here merely my own observation to rely on. Numerous writers on fever might be quoted, whose descriptions of that disease assign to the altered state of the secretions that priority of existence for which I am contending. I shall

rest satisfied, however, with adducing a remark made by the reviewer of Dr. Smith's work, in the *Medico-Chirurgical Review* for April 1830.

"No eloquence can persuade us, contrary to evidence of our own senses, that secretion and excretion become deranged only when reaction commences in fever. According to our own observation, those functions are deranged, though in a different manner, during the antecedent stage described by Dr. S. and all other writers on fever."

Indeed it is not easy to comprehend why a deranged state of the secretions should not be an early occurrence in fever, seeing that that state is, as Dr. Tweedie very correctly observes, "a consequence of the previous condition of the sensorium;" and not, as several passages in Dr. Smith's work erroneously imply, the result of excited circulation.

The reason why I have insisted so much on this point, will soon be rendered evident.

To the symptoms already enumerated as attending upon the primary stage of fever, or the stage of collapse, as it is called, there succeeds, after an interval, which varies in different cases, a state of a very opposite nature—that of excitement. In this, the pulse is strong, full, and bounding; the skin is no longer of a deathly pale colour, but is, in various parts, hot and flushed. There is a particular determination to the capillaries of the face. The eyes are deeply suffused; the arteries of the temples may be felt, and in some cases seen, beating with great violence. The pain in the head generally increases in severity; the urine assumes a deeper colour, and there is great and incessant thirst. All these symptoms become more and more aggravated, till at length a crisis arrives.

In watching attentively the progress of fever, it is impossible not to be forcibly struck with the remarkable change in its features which I have above attempted to delineate. The means by which that change is effected—by which the circulation, from being so slow and so weak that the blood might very well be termed—

"A cold, slow puddle, creeping through the veins,"—

becomes unnaturally strong and rapid; and the influence which such a change exerts on the system, are subjects of the most interesting nature, and to the consideration of which, therefore, pathologists have at all times bent the utmost powers of their minds. Various theories have, in consequence, been proposed, to explain these points. According to that most generally received, the excitement which follows the primary state of collape in fever, is of a sanatory character, being nothing else than an effort of nature to rid herself of the disease. But as the former part of this proposition has been proved by experience to be false—as, in fact, much of the danger of fever consists in injury done to the brain and other vital organs, during the unusual excitement—nature would shew herself extremely foolish in making such a furious effort. Another circumstance, little or not at all considered by writers on fever, is, that there should prevail such excessive thirst, while, from the secretions being partially or entirely suppressed, there is but little waste of the fluids. In the observations which I am about to make, I shall, I trust, be able to explain the cause of this, as well as that cause by which the stage of excitement is brought on.

A living body is continually the theatre of fresh revolutions. One set of agents is constantly employed in taking down the old materials of the fabric; another set in replacing them with new ones, and thus preserving its strength and integrity. In health, the whole of these old materials, the *debris* of the renovated body, is carried off by the various excretions; but, before they can be thus removed, a complete change takes place in the arrangement of their constituent particles. The greater part of them is converted into a variety of salts, which, being held in solution by the secretions which contain them, are in this way conveyed out of the system.

Now we cannot doubt that it is to the influence of the nerves—or it may be only to that of certain nerves, different in their nature, as well as in their functions, from those either of secretion or of muscular motion, but this is stated merely as conjecture—we cannot, at any rate, doubt that it is to nervous influence that the secreting vessels owe the eliminating and chemico-vital powers with which they are endowed. The necessary consequence of this dependence is, that these powers are affected

by every change in the condition of the nervous system. In fever, therefore, when the brain has its energy so much impaired that it cannot perform its functions in the usual healthy manner, the capillary vessels very soon become incapable of effecting, in the matter which ought to be removed out of the system, those changes which are required to fit it for expulsion. There occurs, likewise, a diminution in the quantity of fluids excreted; so that, altogether, there must be an accumulation in the blood of a large portion of useless matter, the due separation of which is essential to its purity, and, of course, to the well-being of the whole system.

In the above fact we may, I think, find a simple and satisfactory explanation of the excitement which takes place in fever. The blood, loaded with impurities which it can no longer discharge by the accustomed outlets, excites, by the irritating qualities which those impurities impart to it, and probably, in some measure by its bulk, the heart and arteries to an unusual and unnatural degree of action. We may thus, likewise, be enabled to explain why, at a time when there is so trifling a waste of the fluids, the system should so incessantly and so urgently demand a fresh supply of liquids: it is for the purpose of diluting, and thus rendering less irritating, the impurities which it is found impossible to throw off.

The correctness of my opinion respecting the means by which the stage of excitement is brought on, is rendered, at least, highly probable by the following considerations.

1st. A deranged state of the secretions invariably precedes the occurrence of the slightest degree of excitement. If it could be proved that is not the case, it is manifest that my position would at once be rendered untenable. But, as I have already observed, I cannot entertain the slightest doubt that the fact is as I have stated. Indeed it will scarcely, I think, be disputed by any one, when it is understood that I contend for the invariable pre-existence, not of suppression or even of diminution of the secretions, though these states are very frequent, but of a deranged and morbid condition of them.

2d. The pre-existence of such a condition being admitted, its adequacy to produce the excitement cannot, I suspect, be questioned.

3d. Excitement of a similar kind, supervening upon other diseases than fever, admits, in this way, of a satisfactory explanation; although, from its being often brought on by causes aprently the most different in their operation, it has hitherto, in many cases, been deemed perfectly inexplicable. Thus, after extensive hæmorrhage, we have a state of pyrexia, resembling in every respect that which exists in ordinary fever. In like manner, towards the termination of phthisis, and of several chronic affections, a similar condition is very frequently met with. Now, in every one of these cases, diminished energy of the brain and nervous system, by the alteration which it induces in the functions of secretion and excretion, is the primary cause of the excited action.

4th. In proportion as the secretions become re-established, the excitement invariably diminishes. This fact, which every writer on fever has mentioned—which every practitioner must have observed—makes strongly for the soundness of the doctrine which I have been endeavouring to maintain.

5th. The excretions, upon the decline of fever, invariably contain a much larger quantity of salts and other matter than they do in their ordinary healthy state. I need only refer to the urine, which is so loaded with these substances that a copious deposit takes place almost immediately after it has been passed. This affords an additional proof that there does actually take place, during the progress of fever, an accumulation in the blood of matter which, in a state of health, is regularly excreted.

It very generally happens that the profuse perspirations attendant on hectic fever, smell strongly of acetic acid, which they no doubt contain in a very considerable quantity. Now I have repeatedly observed a similar state of the cutaneous secretion, occurring on the decline of common fever; and I have no doubt that in both cases the phenomenon is produced by the same cause. This cause is, in all probability, a peculiar action of the capillary vessels, by which certain of the impurities of the blood are so modified and united as to produce the substance in question. One of the constituents of acetic acid is car-

bon, and when we consider how imperfect, during fever, is the excretion of carbon, both by the lungs and by the skin, while of it the old materials of the system in a great measure consist, we cannot have much difficulty in conceiving whence that constituent, at least, is derived.

Let us now briefly consider the theory of fever which I have here proposed, with reference to the proper manner of treating that disease. And here I must at once confess, that I cannot pretend to suggest a plan of treatment either quicker, safer, or more certain in its effects, than that which is at present generally followed. My only object is to show, that the views which I entertain respecting the nature of fever, are in perfect accordance with the knowledge which experience has afforded us of the degree of success attendant upon the principal measures of cure of which trial has been made.

The propriety of a person who labours under fever abstaining almost entirely from aliment, is unquestionable. This being the case, the strong aversion to food, which exists from the very commencement of the disease, is usually termed a friendly intimation—a wise provision of nature. Now, to this mode of representing the matter, there is one forcible objection, and that is, that, by deluding us with the alleged existence and operation of an intelligent, directing, and controlling principle; by leading us back, in fact, to the doctrine of an Archæus, it prevents a philosophic investigation of the real manner in which the phenomenon in question is produced. After what has been already said respecting the deranged and exhausted state of the nervous energy in fever, it must be evident that the aversion to food is a necessary consequence of that state. The secretions which have been proved to be so essential to digestion, are either greatly altered or entirely suppressed. The final step in the process of assimilation, that by which certain parts of the blood are converted into the various tissues of the body, is nearly or altogether arrested. Languidly and imperfectly performed as the several functions of the body are, the process of absorption, which still in some measure continues, furnishes of itself a supply large enough to satisfy all the demands of the system. There cannot, therefore, be that sense of want—that "craving void," which we denominate appetite. On the contrary, the slightest quantity of food must, under such circumstances, give rise to a feeling of disgust, similar to that experienced by a person in health, when, after having already eaten fully as much as the wants of his system require, and his assimilating organs can turn to good account, he attempts to introduce into the stomach a still greater quantity of food.

After the stage of excitement has commenced, the desire for cold and diluent liquids is not less strong than the aversion to every kind of aliment. It is not a little surprising, then, that some of the ancient physicians, who shewed themselves so observant of the intimations of their favourite principle or entity—nature—and so ready to comply with her demands, should here neglect her suggestions, and horribly torture their patients, by withholding from them, for a very considerable time, every species of liquid. Experience has long since taught practitioners, that such a proceeding is not less injurious than it is distressing and cruel; and accordingly, at the present day, persons labouring under fever are allowed to drink as much of any simple and cooling liquid as they please. But no author that I am acquainted with has attempted to assign a reason, why there should exist so urgent a demand for fresh supplies of fluid, when the expenditure is so small; nor can the phenomenon be accounted for, unless I am greatly mistaken, in any other way than that which I have already explained. If fever were perfectly identical with inflammation, bloodletting would in almost every case be not only a safe, but a requisite measure. Every person, however, who has had any experience, must be aware that it is frequently neither the one nor the other. The best practitioners never abstract blood in fever, unless when the excitement is so great as to threaten injury to some particular organ. It is, indeed, under such circumstances alone, that we are justified in having recourse to the measure, which, however useful in preventing the injury that might otherwise accrue to certain organs from the high state of excitement, never contributes, in the slightest degree, to the direct cure of the disease. On the contrary, it must, I think, be admitted, that when-

ever, in any case, it has been found necessary, on the grounds to which I have alluded, to abstract a large quantity of blood, the cure of the disease is invariably tedious. Now these practical truths perfectly accord with the views of the nature of fever which I entertain.

We hear a great deal at the present day respecting the efficacy of the neutral salts in fever. During the last two years I have more than once given them a fair trial, and I could not find that they exerted any beneficial influence on the disease. I have no hesitation, therefore, in predicting that a very short time will deprive them of their popularity. Indeed, if we only reflect that the morbid state of the blood, which they are intended to correct, is not the cause of fever, their failing to cure the disease is what, *à priori*, we would expect.

The use of active purgative, diuretic, and diaphoretic, medicines, by restoring in some measure the excretions, would be more likely to prove advantageous; and there is no doubt that their operation is frequently productive of benefit, carrying off from the blood some portion of its impurities, and thus lessening excited action. But the energy of the brain and nervous system still remains impaired; and while this state continues, no medicines whatever are capable of effecting a perfect and permanent re-establishment of the several secretions. Besides, in employing these remedies, we ought always to bear in mind, that where much debility exists, their good effects may be more than counterbalanced by the expenditure of nervous energy which their operation occasions.

From all that has been here said, it must be evident that we have no means of directly curing fever. We may mitigate its symptoms, and prevent some of its contingent evils, but the disease still continues, and is terminated only when the powers of the brain and nervous system are restored to a healthy degree of vigour. At present we are not acquainted with any means by which this can be at once effected; we are therefore obliged to wait until it be accomplished by the natural powers of the system. This is sure ultimately to happen, if we can only in the meantime keep the several vital organs from sustaining severe injury; and likewise prevent a total suspension of any of the vital functions; for the languid and imperfect manner in which most of the mental and bodily functions are performed during fever, and the trifling expenditure of nervous energy consequent thereupon, allow to the brain and nervous system a degree of repose, by which they must, in time, be completely refreshed and invigorated.

JOHN M'DIVITT.

Kegworth, Leicestershire,
May 12, 1832.

NOTES AND OBSERVATIONS

ON

THE LOSS OF LIVES FROM CHOLERA ON BOARD THE SHIP BRUTUS;

And on the Conduct and Constituency of the Board of Health in Liverpool.

BY JAMES COLLINS, M.D.

THE arrival of the Brutus in our port, on Wednesday, the 13th June, with several cases of cholera on board, has excited so much pity and commiseration for the poor sufferers, that I flatter myself a communication on this subject may not be uninteresting to the profession, or unworthy the pages of the Medical Gazette. My object is to record facts, in illustration of the necessity of our Medical Boards discharging, without fear or compromise, the duties for which they have been appointed; and by so doing, of upholding the confidence which is usually reposed in the certificates of medical men. It does not require much sagacity or moral feeling to see the necessity of this, and not to do so implies a total disregard to both.

The vessel above mentioned sailed from Liverpool on the 18th May, for Quebec, with 330 passengers on board, consisting principally of agricultural paupers from various parts of England. They were sent out at the expense of their respective parishes, with the intention of being located in Upper Canada, according to the principles of the emigration scheme for relieving places of their surplus population. Little did they know how soon their prospects of happiness were to be turned into mourning and death!

On the 25th, seven days after the vessel sailed (though other accounts state but two), the first case of this formidable disease broke out. The first person it attacked was a young man, in the bloom and vigour of youth: he recovered. The next was an old woman, who soon fell a victim to it. Then it laid its grasp upon another and another, but at first so slowly that five or six days elapsed before more than two were seized with it on the same day. At last it broke out with such fearful and destructive rage, that in little more than one fortnight it numbered among the dead seventy-six of the passengers and four of the crew; and, to make the scene the more melancholy and appalling, they were short of provisions, clothes, and other necessaries, and, to use the words of the surgeon, "were destitute of the common articles of bed-covering." I will not say that they were stowed away more like bales of cotton, or dead lumber, than human beings; but even if they were, it would not have differed much from the usual manner in which the steerage passengers leave this port for America. The custom here is to carry as much freight as possible—to make the most of the voyage—no matter what may be the result to the poor creatures that are so often the victims of the cupidity and speculation of the brokers.

The captain, after having made several ineffectual efforts to continue the voyage, was obliged at last to put back, from want of hands and other necessaries. He arrived on the 13th, after having had one hundred and thirteen cases of cholera on board, and eighty-one deaths; thus exhibiting a picture of human suffering and destitution seldom witnessed.

The Board of Health in Liverpool comprises several influential merchants and magistrates, and the two rectors of the parish, together with eight or nine physicians connected with one or other of the medical and charitable institutions of the town. The latter constituted the Medical Board, and were selected by the magistrates (or rather by the mayor), without the consent or sanction of the profession, on the principle that, as they belonged to some of the public charities, they therefore must be respectable men; though it is notorious as the noon-day sun that these appointments are procured more by patronage than talents. All documents and communications from the Board must be signed by the mayor as chairman, except the medical certificates, which must have the signature of the president of the Medical Board. So much I deem it necessary to say in explanation of the constitution of these bodies, to enable the reader to understand what follows.

The disease first broke out at Liverpool on the 25th April. The Medical Board, however, only admitted it to have done so on the 12th May, in an official communication which they published on that day; and yet this unfortunate vessel was allowed to sail on the 18th, with a clean bill of health, signed as bills of health usually are, certifying to foregn nations that there was no cholera or any other malignant or contagious disease in Liverpool, and consequently that all vessels from it ought to be admitted by them without riding quarantine. Thus that was stated which was not the fact, and that signed which ought not to have been signed, even on their own shewing; for in doing this, they were deceiving foreign nations who reposed confidence in their documents. Now I may ask, what would have been the result to the poor settlers in Upper Canada, had the ship Brutus imported among them the seeds of this disease? Was it by a similar and culpable negligence that this disease has been imported into New York and Philadelphia, where it is now said to be scattering death and terror around it?

As I have touched upon this subject, I would leave it defective if I did not go a little more into detail of the way in which this disease first broke out among us, and of the way in which it was attempted to be hushed and smothered at its birth. The subject may be trivial in itself, but it unfolds some little traits of the character and feelings of what is termed the respectable portion of the profession here, and in this respect may not be uninteresting or unworthy of record to the future historians of the disease. It first broke out here on the 25th of April; a woman, on her passage from Ireland, was seized by it at the moment of landing here from one of the steam-vessels. The captain feeling alarmed, communicated the fact without delay to the mayor. The latter, as chairman of the Board of Health, wrote immediately to the president of

the Medical Board, requesting him to see this woman, and to say whether the disease was cholera or not. This gentleman could not go—his practice prevented him—but he wrote to one of the medical gentlemen connected with our Dispensaries to see the patient for him, and to report to the mayor what he might conceive it to be. He did so, and declared it cholera. The announcement was received like a thunder-clap; a member of the Medical Board, who had previously seen some cases of cholera in Sunderland, was dispatched to see this. He did so, and it was declared a case of abortion; and next day a communication to this effect was made to the public, and a simultaneous cry raised, how cautious medical men ought to be in circulating reports that the cholera was in Liverpool!

Notwithstanding all this, several other cases were reported in a few days after, by private and respectable practitioners. But the eyes and ears of the Board were no longer disposed to see or hear of the arrival of the disease among us; but, as it were to satisfy *the public*, and to give the sanction of authority to their assertions, the same medical gentleman who saw the first case was dispatched to see some of these, and became a second time the messenger of glad tidings, by reporting them not to be cholera. The merchants were pleased, the Board of Health were pleased, (many of them being merchants principally interested in the foreign trade,) ships crowded our docks, all anxious to get away before cholera should be declared by authority to be among us—nothing could equal their bustle and competition in this respect; at last the influence of public opinion began to be felt—private practitioners hesitated to report the cholera cases they met: they did not wish to expose themselves to official contradictions. The Medical Board became the subject of very free animadversions, and they felt that more than ordinary tact was now necessary to stem the torrent; and they gave proofs that they were not defective in this respect. They resolved, as it were for the greater *security* and *satisfaction* of the public, that the Board should report no case as cholera until seen and unanimously declared such by three of their own body. The device was admirable, and well contrived; for it seldom happened that any three of them could be got together to see the same case, and if perchance they were, they differed in opinion: one said it was cholera, another not; and of course the public, like the culprit in law, got the benefit of the doubt. It was of no consequence what other medical men may deem it; their opinion went for nothing; it could not be cholera, as three of the Medical Board differed about it.

Thus things were kept quiet from the 25th of April until the 12th of May, clean bills of health were issued, and vessels allowed to leave this port for all parts of the globe, carrying with them the seeds of future mortality and pestilence; and, at the same time, certificates from the chairman of our Board, that Liverpool was free from cholera at the time of their sailing. Thus the port remained open, whilst that of London was closed, and hence became the great outlet for the export trade of the kingdom, and consequently a source of great profit and speculation to the merchants. It was a bad wind that did not blow good to some. At last the public opinion began again to be felt, and through its influence the Board was obliged to give in and make a virtue of necessity, by publishing for the first time an official report, on the 12th of May, that the disease had broken out here, though the first case occurred on the 25th of April, as one of their own body admitted—the very gentleman who at first denied it.

On the very day that this official report was published, he summoned together the members of the profession here, and in a public lecture which he delivered to them on this disease, he admitted, certainly with more candour than judgment, that the case which he reported on the 25th of April to be abortion, was one of cholera, and that other cases which he about the same time denied also to be this disease, were cases of it—thus putting upon record evidence against himself and the Board, not very creditable to either. But things did not rest here; clean bills of health still continued to be issued, and vessels allowed to sail with certificates that the cholera was not here. At last the impropriety of such conduct became so glaring and notorious, that the collector of the Customs interfered, and wrote to the mayor, as chairman of the Board of Health, to know when he should receive from him an official notice that the

cholera was in Liverpool. Then, and not until then, did the Board make this communication, though they must know that, until they made it, vessels would be allowed to sail out with clean bills of health as usual. Were it not for this interference, it is difficult to say how much longer the communication may have been kept back, and clean bills issued.

This communication to the Customs was only made on the 19th May—that is, seven days after they admitted the disease to be in Liverpool; and in it only four cases were reported, though the gentleman so often alluded to, in his public lecture, admitted that there were thirteen or fifteen cases up to the 12th. How they will reconcile all this, I do not know; it requires no comment—it speaks for itself; and therefore I shall not weaken the effect by attempting any. The unfortunate vessel, the Brutus, was allowed to sail on the 18th, with a clean bill of health, at the very time that, by their own shewing, the disease was raging among us. The sufferings and mortality which occurred speak volumes as to the impropriety of such conduct, and forcibly illustrate the credit that ought to be attached to the official documents.

Such are a few of the facts that have tended to compromise us in this town. I wish merely to preserve them as a lesson for the future, and a record of the past, in the hope of directing the attention of the profession to the impropriety of allowing vessels to sail with clean bills of health from an infected port, for their own sakes and that of humanity; and in allowing me to do so through the pages of this journal, I think you will be doing a service to medical men, and promoting the respectability of the profession.

James Collins, M.D.

Liverpool, June 21, 1832.

CASE OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

I should feel greatly obliged if you would insert the accompanying case of cholera in the Medical Gazette at your earliest convenience.

Your obedient servant,
Henry George.

Phillimore-Place,
Kensington.

"The change which a great and sudden depression of nervous energy works in the quantity and quality of the secretions, has undoubtedly a share in provoking the act of vomiting.

"Nausea is not only an ordinary consequence, but a never-failing cause of nervous depression."
—*Travers's Inquiry, &c.*

Sarah Groom, aged 42, the mother of twelve children, was attacked with slight diarrhœa on the morning of the 15th of June. On the evening of that day, some aromatic and opiate confection, with sal volatile, were administered.

16th.—Has been vomiting incessantly through the night a colourless fluid; has had several motions in the course of the night, resembling water-gruel; is now dreadfully exhausted; eyes sunk in their sockets; voice scarcely audible; skin, particularly of the hands and face, of a leaden hue, cold and damp; violent cramps in the legs; tongue cold; great oppression about the præcordia; if asked where she feels most pain, constantly refers to that region; has made no water for the last twenty-four hours. A small dose of calomel and opium was given (calom. gr. iss. opium, gr. ¼.) I saw her for the first time about 10 o'clock, A.M. two hours after the administration of the calomel; her general appearance was then nearly as above described, with great restlessness; frequent and heavy sighing. I ordered the following medicine to be taken immediately:—

Liq. Potass. ʒj. Aq. Puræ. ʒx. M. ft. haust. statim.

She vomited soon after its exhibition. I mixed two drachms of the liquor potassæ with about twelve of water, and directed a tea-spoonful to be given every five minutes.

1 o'clock, P.M.—Has had no vomiting since I last saw her; has taken all the medicine, though previous to its exhibition the stomach would not retain the smallest quantity of cold water, which was, and is still constantly prayed for; has slept a little. I mixed ʒj. of liq. potass in ℥iss. of water, and directed a tea-spoonful to be given every ten or fif-

teen minutes, but oftener should the vomiting return.

9 o'clock, P.M.—Scarcely any vomiting through the day; has taken all the medicine; the stage of collapse much less distinctly marked. Within the last hour the stomach has been rather more irritable; she is disgusted with the taste of the alkali; no urine has been passed.

℞ Ammon. Carbon. gr. iv. Pulv. Rhæi. gr. iss. Pulv. Opii, gr. ⅙. M. ft. pilul. 2nd. quâque hor. capiend.

17th.—Vomited occasionally in the night, but slept a good deal; skin warm; pulse steady, weak, and rather quick; tongue dry, and covered with a brown crust; no urine has been passed; the catheter was introduced, and the smallest quantity of high-coloured urine drawn off. The gentleman who introduced the instrument thought the bladder felt contracted; she still complains bitterly of oppression at the præcordia; has had a motion, which is perfectly healthy in appearance, and very fœtid.

℞ Sulph. Quin. gr. j. Ammon. Carbon. gr. iv. Pulv. Rhæi. gr. j. M. ft. pil. 4ta quâque hor. capiend.

9 o'clock, P.M.—Has passed a very comfortable day; very little vomiting; has taken tea, barley-water, jelly, and milk; no urine has been passed.

Contin. Pilul.

18th.—Passed a very comfortable night; is a little sick this morning, and has vomited what is apparently dark green bile; but in every respect is much better; no urine has been passed.

℞ Hyd. Sub. gr. ij. Ext. Coloc. c. gr. viij. Pulv. Opii, gr. ¼. Gum Camphor gr. iij. M. ft. pil. ij. stat.
Contin. pilul. ton.

9 o'clock, P.M.—No relief from the bowels; no sickness; skin soft, warm, and moist every where; pulse quick, about 100, soft; tongue moist, but covered with a brown fur; expresses herself as feeling very comfortable; no urine has been passed.

Contin. pilul. ton.

19th.—Is very comfortable this morning; passed about six ounces of darkish coloured urine in the night; no relief from the bowels; an injection was given, which brought away fæculent matter, in quantity sufficient, containing plenty of bile.

Contin. pilul. ton.

9 o'clock, P.M.—Has passed a very comfortable day; has made more than a pint of very healthy urine; has taken beef-tea, &c.

Contin. pilul. ton.

20th.—Going on well; the catamenia appeared in the night.

9 o'clock, P.M.—Has been in much pain in the loins, and lower part of the abdomen and the thighs; skin rather hot; pulse very quick; restless; no relief from the bowels.

℞ Pulv. Rhæi, gr. xii. Conf. Opiat. gr. x. Tr. Card. c. ʒj. Aq. Menth. Pulv. Ex. M. ft. haust. stat.

21st.—No relief from the bowels; suffered much from pain in the night; thinks it possible she may be miscarrying.

9 o'clock, P.M.—All uterine irritation has ceased; pulse 80; tongue moist; bowels slightly moved.

℞ Liq. Potass, ♏lxxx. Tr. Aurant. ℥ss. Liq. Opii sed. gtts. xij. Aq. Puræ. ℥iiiss. M. ft. Mist. Capt. 4tam post 6tis horis.

22d.—Going on very well, &c.

INJECTIONS INTO THE VEINS.

To the Editor of the London Medical Gazette.

SIR,

Now that the plan of treating cholera by injecting saline substances into the veins has been practised and found pre-eminently successful, I hope you will do me the justice to insert the last part of my paper, which was omitted on the 14th of April;—"for the stone which the builders refused is now become the head of the corner." Had it then appeared with the rest of my observations, my claim to the originality of the idea would have been undoubted.

I have had no cases of cholera in London sufficiently under my own direction to put it in practice, nor could I succeed in making those who had, sufficiently understand my views of the pathology of

cholera, which led me to propose it, to give it a trial. The explanation, however, of the principle upon which substances thus introduced into the circulation act in effecting the cure, is still clearly mine. It is this:—The saline substances being stimulant, and mixed with the blood, are carried with it to every part where the blood can go; thus more completely stimulating all parts than can be done by external means; and the restoration of the natural condition of the blood is the result, and not the cause of the action thus excited, in every part where the stimulus has reached.

One proof of my having early proposed injecting stimulants into the veins for the cure of cholera, is the circumstance of my having bought an apparatus for doing it so long ago as last February, when I was in Newcastle, but have had no opportunity of using it.

I remain, sir,
Your obedient servant,
JAS. WM. EARLE.

14, Old Broad-Street,
June 25, 1832.

"It appears, therefore, that, as there is a point previous to the complete collapse, when no medicine will have its proper effect when taken into the stomach, so also there is a point previous to death when no external stimulant can effectually restore the circulation. The degree of diminution of the temperature would, perhaps, bring us nearer than any thing else to the knowledge of this point, beyond which we cannot hope to be of service; but it must necessarily vary so much in every constitution, the previous state of which we are seldom acquainted with, that we are on no account justified in withholding this last and only chance of restoration. Even in these extreme cases we have yet, I say, another means by which we can stimulate the circulation, and obviate the want of absorption; and it would certainly be allowable, as well as practicable, to try its power. We are able, with a syringe, to introduce into the blood a stimulant in any quantity, and of any strength that may be judged proper. I would suggest spirit of wine and distilled water as possessing only stimulant properties. The effect of this experiment on animals is the *sudden* production of the same state of excitement which is observed to come on slowly when taken into the stomach. This is not, for obvious reasons, an experiment which should be tried in a poor's man hovel, but I should not hesitate to put it in practice should an opportunity occur. In this way Dr. Philip's experiment of applying spirit to the brain and spinal marrow will be in effect repeated as soon as the blood, carrying the spirit with it, shall have reached those organs, while the water, by diluting the blood, will render it more fit for entering the capillary vessels. If, by such means, reaction should be happily established, I would treat the case precisely as if the circulation had been restored by external stimulation."

[We have thus complied with Mr. Earle's request of inserting a portion of his paper which was formerly omitted. It may be proper to state, that we condensed the paper alluded to, with great care, and, we have reason to believe, that we thus added to its interest, without abstracting a single sentence of importance to be retained. Among other paragraphs omitted was that which we now publish at the author's request. If Mr. Earle rests his claim to the stone which he says "is become the head of the corner" on what is contained in the above extract, regarding the injection of medicated solutions into the veins as a means which might possibly be used, we must remind him, that he therein but incidentally repeats what had been several weeks before specifically recommended by ourselves, by Dr. O'Shaughnessy, and various others;—if, again, his claim rests upon the proposal to inject spirits and water in preference to any thing else, the value of the remedy would probably have been better appreciated had Mr. Earle been able to state that it had ever been used.—ED. GAZ.]

OPIUM IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

FROM a perusal of the cases of cholera treated by saline injections, detailed in your Gazette, and which have not been

attended by favourable results, it would appear that death has ensued from a continuance of the secretion into the intestines, and consequent exhaustion of the patient. When a quantity of fluid is taken by a person in good health, an increased impulse is given to the vessels of the skin and kidneys to carry off the superabundant fluid, and the same result must take place in cholera from the saline injections; the impulse in this instance taking place to the bowels from the diminished energy of the vessels of the skin and kidneys, and from the previous action.

It appears that, in the treatment of cholera, this fact has been too much overlooked, and too much reliance has been placed on the saline injections alone. This treatment, to be effective, must be combined with those remedies which restrain this secretion. In the cases of cholera which fell under my care, I found that larger doses of conf. opii than are usually administered, and frequently repeated, were sufficient to check the inordinate action of the bowels. It is, however, of less importance in what form opium or other astringents may be administered, provided the result is obtained, and the saline injections be thus rendered of more permanent utility.

Your obedient servant,

J. R.

June 25, 1832.

PULSATION OF THE VEINS.

To the Editor of the London Medical Gazette.

Sir,

In your last number an instance of *pulsation of the veins of the back of the hands and fore-arms, synchronous with the arterial pulse*, is related by T. O. Ward, M.B. Oxon, who says he never met with an account of a similar phenomenon. The following quotation of a note by Dr. Elliotson to his last edition of Blumenbach's physiology, may be interesting, especially as in the latter gentleman's case, as well as in that of the former, a pulmonary inflammation existed.

Your obedient servant,

L. U.

"In a young lady whom I attended for chronic bronchitis, accompanied by a violent cough, and who ultimately recovered, all the *veins of the back of the hands and forearms pulsated synchronously with the arteries.* An unusual pulsation of the veins, synchronous with that of the arteries, occurred for some days twice in a young man who died of cerebral disease, with constriction of the mouth of the aorta*; once in a middle-aged man, with affection of the head and abdomen, who recovered†; once in a middle-aged man, who died with dropsy and palpitation‡; and lately in a girl who died with symptoms of hydrocephalus§. In a case of epidemic fever, the same was observed by Weitbrecht for twenty-four hours‖; and he had previously seen a similar case, but doubted his senses. Haller's remark upon it, is *ego quidem non intelligo*¶.

INJECTION OF SALINES AND OF SERUM INTO THE VEINS IN CHOLERA.

Extract of a Letter to a Member of the Central Board of Health.

Hull, June 24, 1832.

Sir,

I enclose you Mr. Casson's report of the cases in which injection of fluid into the veins has been tried, though, I regret to say, with a fatal result in all.

The disease has changed its *locale* in this town, and is now extending itself into the Old Town; and, although the daily numerical report is much smaller than formerly, individual cases still present themselves, which, in spite of all treatment, proceed steadily and rapidly to a fatal termination.

I wish I could send you any information that would give me more pleasure to communicate, respecting this dreadful scourge. —Believe me,

Yours most faithfully,

James Alderson.

My dear Sir,—As you desire to have, for the information of the Central Board of Health, a brief statement of those cases in which I have tried the injection of fluids into the veins, I give you the subjoined accounts.

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* Journal Complementaire, t. xxi. Juin 1825.

† Journal der Practischen Heilkund, Sept. 1815.

‡ Archiv. für Medicinische Erfahrung, July and August 1822.

§ Haller's Disputations, t. v. p. 407, 1736.

‖ Dublin Hospital Reports, vol. iv.

¶ Elem. Physiol. t. ii. p. 356.

I confess I was much chagrined and disappointed at their fatal termination, when the commencement of the plan had raised my hopes so high. The re-action was so decided and complete, that I thought to myself, now we have arrived at the *ne plus ultra* of treating the disease. The anticipations in which I have indulged for the last seven months are at present disappointed; but I will not yet despair. We appear to have now in our possession a power which probably only requires to be properly wielded, to give it the desired success. I shall make further trial of the natural serum, if I can obtain sufficient for the purpose; and if not, I would endeavour to come as near as possible in imitation of the natural fluid. This plan I proposed to the Editor of a medical periodical on the 5th of last March, but my letter was not published, for some reason or other. I also proposed it to Dr. Longstaff and yourself, so long ago as the 27th December last. Dr. L. at that time promised to communicate it to his friends at Newcastle, but his other engagements prevented his doing so.—I am, dear sir,

Your obedient servant,

RICHARD CASSON,

M.R.C.S. London.

To Dr. Alderson, Chairman to the Medical Committee of the Board of Health, Hull, &c. &c.

CASE I.—*In which the injection of ninety ounces was followed by immediate improvement — Relapse — Operation repeated, and eighty-five ounces more thrown in, with the same, though more temporary, benefit—Operation performed a third time, to the extent of sixty-nine ounces, making altogether 244 ounces — Result: complete Coma, and ultimately Death.*

Jane Carter, aged 42, a woman of irregular habits, and addicted to drinking spirits when she could obtain them, is in a state of great poverty, and filthy in her house and person.

May 98th.—Was attacked last evening at nine o'clock. I saw her at five o'clock this morning. Has been purged a dozen times since the attack; the quantity discharged from the bowels excessive, and quite characteristic; cramps violent in the legs, body, and arms, which are increased by external warmth. Hands and arms, and lower extremities, cold and livid; fingers corrugated, voice low—a hoarse whisper. Calomel and opium treatment, with anodyne injections, to stay the purging and cramps, which had the effect. Saw her twice afterwards, and found her growing worse. Saw her again at half-past 11 A.M. No pulse at wrist, nor above the elbow; lies on her back motionless, and apparently dying. Respiration short, and with long intervals. At that time I was about to inject serum, when one of the medical journals was put into my hands, containing the Leith plan of injecting water holding in solution the carbonate and muriate of soda. The success of this plan determined me not to adopt my own, which had but once been tried with some effect, but not favourable in the end. At 12, when we commenced, her skin was cold, wet, and completely livid; the eyes as much sunk in the orbits as I had ever seen; the areola deeply marked. After injecting about sixteen ounces of the fluid, at the temperature of 110° F., Dr. Alderson felt returning pulsation at the wrist, which gradually improved as the process went on. We stopped at 90 oz., at which time we had obtained a strong pulse. The skin in both extremities became of a natural warmth and colour, the lips became of a natural redness, and there was a red flush upon the cheek; the eyes had become projected forward, the voice natural. She expressed herself much better, and her friends about her all acknowledged what they termed the wonderful change, and left her about ten o'clock, quite comfortable and animated. Saw her again about half-past 2, when she had relapsed, and was in a state of collapse, and the pulse extinct. At a quarter past 3, eighty-five ounces more had been thrown in, with the same re-action, but she was more inclined to be comatose than before. In a quarter of an hour, or twenty minutes, the pulse again left her, when sixty-nine ounces more were thrown in with a temporary effect; her breathing became stertorous, with a mucous râle; deep coma, from which we could not arouse her, and she expired at four o'clock.

This woman had choleric symptoms about three weeks before.

CASE II.—*Slight re-action produced by the injection.—Death in about two hours afterwards.*

Another woman at the Cholera Hospital, where I had the valuable assistance of Mr. Clay, Surgeon to that Institution, between 50 and 60 years old, of intemperate habits, and asthmatic, had the injection tried, which had the effect of bringing back the pulse, and some warmth; but the re-action was not so decided, and she died in two, or two hours and a half afterwards.

CASE III. *In which after other remedies, and among these the exhibition of saline powders, had failed to produce any beneficial result, the operation of injecting into the veins was resorted to, with the effect, after 112 ounces had been thrown in, of relieving all the symptoms—Relapse—Sixty-eight ounces more injected, with temporary benefit, soon followed by death—Post-mortem appearances.*

Robert Beharill, a sawyer by trade, subject to diarrhœa, had been affected in his bowels a full week before; was attacked

with cramps in the body and lower extremities, about four in the afternoon, after taking a walk.

June 10, 1832.—I saw him at half-past five: he had been sick, and thrown up, he thought, all his dinner, consisting of broth and dumpling. Had been purged seven or eight times a day. Motions watery and transparent, with a small portion of ricy sediment at the bottom of the fluid. Pulse feeble, and 80; skin cool and damp, legs warm; eyes a little sunk; no areola, nor bluenessof limbs. Gave an ipecacuanha emetic, which brought up a considerable quantity of dumpling in lumps. Mustard poultices to chest, abdomen, and back. Suppository of cerupe Acet. and opium, when purging was troublesome. The saline powders, so strongly recommended, to be taken every half hour. Nine o'clock: has had one evacuation, no sickness; has sunk rapidly; pulse nearly gone, skin cold, damp, and cerulean; eyes greatly sunk. Saw him at eleven, with Drs. Alderson, Mr. Hardy, and Mr. West, surgeons, whose kind assistance was very acceptable. Pulse all but gone, sinking, and apparently a lost case. The whites of three eggs were blended with 120 ounces of water, at the temperature of 110°, containing the usual proportion of muriate of soda and carbonate. At commencing, the pulse was 148 and quivering. After throwing up twelve ounces, perspiration broke out on the surface: respirations, forty-eight in a minute. After 17 ounces some improvement was manifested in the pulse; but still small and quick. Before this period he was almost unconscious of what was going on: now he begins to complain, and move about, requiring to be held; opens his eyes and looks about at the room, and at the operation going on: eyes suffused; says he feels a little easier; asked for drink, and took barley-water. When 48 had been given, the temperature had fallen to 108°, and it was increased to 112°. After 54 ounces, a very profuse perspiration broke out. After four ounces more, a great improvement took place in the force of the pulse. When 78 ounces had been injected, said, and it was evident to us, that he was much better; breathes less short; the voice much improved; pulse 134, and, in a minute after, 124. When he had got 97 ounces the hands became sensibly warmer, and he again said he felt still better. At 112 ounces, breathes, as he says, "a deal easier." The cramps in the legs, which had continued, were nearly gone, except a little in the right foot. Pulse 126. At this period he began to vomit excessively, shooting the contents of the stomach over the whole party, the bowels acting at the same time in bed. The injection was now stopped. Soon after he got out of bed without assistance, and discharged a pint of the peculiar fluid from the bowels; from this time he had no further motion. At this time his skin was quite hot enough, and dry; his eyes had acquired their usual prominence; the voice, which before was so slow as only to be heard by applying the ear to his mouth, had acquired its natural tone and compress. At about half-past two, he was reverting into collapse, when 112 ounces more was injected with the same cheering result; but at three, the collapse again returned, when about 68 ounces more were given with nearly the same result, except that he became comatose, and laid motionless on his back; his breathing became laborious, and he could not be roused by shouting to and shaking him. I then dashed his head with cold water, and applied wet cloths, which had the effect of rousing him from the lethargy, and he spoke again, and encouraged us to continue its application: this benefit, however, soon lost its effect, and at last he became quite insensible. However, as the pulse was still good, and the surface hot, I tied up the arm above the orifice already made, and drew off a pint of fluid from the vein, of a florid colour and thin. The pulse flagged, and there was no improvement. He died in about half an hour, at six in the evening. The *blood* taken away remained florid and uncoagulated, except about half an ounce floating at the top.

Sectio Cadaveris, twenty-five hours after death.—Cellular substance filled with air, from decomposition; skin about the neck and shoulders of a green colour. Head; dura mater natural; much air in the veins of the pia mater; a little effusion, of a red tinge, beneath the dura mater. On slicing the brain many dark bloody points oozed, considerably more than I had ever before observed. About one drachm of fluid in right ventricle; some darkish fluid about the foramen magnum. On cutting through the carotids, as they pass out of the bone, they threw out *part thin blood*, which was florid, and part air. The pericardium contained about an ounce and a half of the same fluid. No effusion into the cavity of the chest.

Case IV. *In which thirteen or fourteen ounces of* Human Serum *were injected, with the effect of partially restoring animation and accelerating the pulse — Death three hours after the operation.*

J. T. between 70 and 80 years of age, apparently a lost case; with pulsation scarcely perceptible at the wrist, and in a comatose state, with his eyes shut and motionless. Had about 13 or 14 ounces of human serum thrown into the vein of the arm on the 11th May. Mr. Wallis, of this town, lent me his kind assistance. During the operation the assistants of the Cholera Hospital, and this gentleman, felt the pulse, which they described as much increased, and, to use their

expression, bounding. The patient was roused, and asked for drink, looked about him, and turned over upon his side: his lips acquired a redder colour. Soon after he complained of pain in his head, his pulse again flagged, respiration stertorous, and he sunk in about three hours. I was not present at the examination. I am told that the arteries of the brain were ossified, and that there was some bloody fluid found in the brain. No notes were taken.

MEDICAL GAZETTE.

Saturday, June 30, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

THE LIBEL TRIALS.

"Thus fools are mulcted, while knaves do pass scot free."—*Old Play.*

THE trials in the Common Pleas on Monday and Tuesday were of too striking and important a character to be allowed to "come like shadows, so depart," without a few brief observations. In another page we have given a report of the proceedings, as we are wont to do on such occasions, there being generally facts and incidents in trials of this description worthy of being specially preserved.

Here were two libels, essentially the same,—one borrowed from the other, but modified in certain typographical respects and circumstances presently to be noticed — still virtually the same— yet the first promulgator of *the* libel is punished by a verdict against him—damages, one farthing: the copier, perhaps the too easily duped corroborator of it, by a verdict against him—damages, £400! We must confess that this does strike us as a not a little singular and odd conclusion — and such as to set the nature of legal determinations in rather a curious light. But it was trial by jury, and that in some measure accounts for it: not that we entertain by any means a contemptuous opinion of this invaluable relic of the wisdom of our ancestors, but it must be admitted that there are questions sent to juries day after day for which this mode of decision is any thing but adequate. We recollect, and if we mistake not we have quoted some years ago, a case or two in point relative to jury trials, which it may not be irrelevant to repeat here. An eminent barrister being consulted about a claim, wrote as follows: "I am clearly of opinion that there is no claim either in equity or law; but as it is impossible to tell how the evidence may impress a jury, I advise it to be tried." And an eminent equity judge is known to have said: "I think I had better decide rather than send the question to the *toss up* of a jury." So much for the boasted trial by jury, far more valuable as a means of protecting the liberty of the subject against the oppressions of government than as a means of insuring justice between man and man.

But in reviewing the principal features of these two remarkable trials, is it impossible to assign any modifying circumstances which may have led to their strongly contrasted results? We shall unreservedly state what our impressions are in this respect, and trust they will be received with the same impartiality with which they are offered.

As there were two distinct trials, so were there two different judges, two juries, and two very different defendants. "Look here upon this picture and on this," — but presently for this "counterfeit presentment of two brothers." The mode of defence comes first. And what could be more different than the manner of conducting each action? The defendant in the first pleads the general issue, gets what evidence he wants out of the plaintiff's witness, but calls no witnesses of his own, thus securing for himself the last word, and, finally, addresses the jury with a wide scope, crying out lustily for the "liberty of the press." The jury deliberate,

and let him off for a farthing. Mark now the contrast. The defendant in the second action puts in a different plea; he undertakes to prove that the libel is true, and its publication justifiable; his allegations are so ill drawn up that they are insufficient even though they were proved, and they are far from being made out; he calls witnesses to testify as to intentions where they should testify as to facts; some of them can give testimony to nothing; some to transactions that occurred since the action was begun; and some, by their indiscretion, do him positive injury. All this is caught up by the leading counsel for the plaintiff, and given with double force to the already prepared jury. The judge finds nothing but aggravation in the nature of the defence, and the jury, finally, after seeing during the latter part of the day no redeeming feature in the case, retire, and bring in a verdict for 400*l.*

Wakley, in truth, is now a first-rate hand at defending a libel. Nobody has more experience—nobody can let himself fall more softly; but he has had to pay amply for his practice. What is commonly said, that "he who defends himself in a court of law has a fool for his client," does not apply to this accomplished libeller. He is *just the reverse* of a fool on these occasions. Such skill and such effrontery has he now attained, that he argues points of practice from the numerous cases in which he has been at various times himself concerned — (Tyrell *v.* Wakley, Cooper *v.* W. &c. &c.) — with the opposite counsel; brow-beats them, and takes tribute of terror and courtesy from them by turns—is impudently facetious with the judge, and bamboozles the jury into indulgence. Nothing can be more strongly illustrative of his talent in the latter way than the tone in which he proceeded with his defence: he told Ramadge to his face that he verily believed him to be the author of the libellous letter for which the action was brought; that the nurse whose testimony should be given was not forthcoming, being dead, and that he supposed Dr. Ramadge could account for it; that if Dr. Ramadge had attended her, *that* sufficiently accounted for her absence. As for that "infamous man, and vile quack and slaughterer," as he called him, John Long, nothing could exceed the virulence with which Wakley attacked the famous rubber and inhaler as he sat in court, directly face to face (it was an exquisite scene!). Then the description of the inhaling process, and the doings of the "Duchesses," and the imaginary conversation and eventual coalition of Long and Ramadge—all this was given with such brazen vulgarity, mingled with considerable humour, that the jury were evidently tickled and put into the right tune for the concluding appeal on the "liberty of the press," and the request of a verdict for the defendant. They did the best they could for their amusing friend, and amerced him in *a farthing!*

Things were far otherwise with Dr. Ryan. Without a tenth part of the talent of Wakley, he must fain be aping that notorious person: because the one has pushed a periodical into bad eminence on the libelling and slander system, the other thinks it would be a good speculation to follow the example; and while affecting to condemn the principles, treads diligently in the steps of his famous master. This folly it was, we must suppose, that induced Dr. Ryan to attempt editorship, with so imperfect an acquaintance with the common use and power of language. We have seen passages confessedly from his pen that would have disgraced a boy in the lowest form of a grammar school. Thus without the requisite talent—certainly without discretion—with zeal, no doubt—and perhaps no radically bad intention—just simple enough to permit himself to be led by the nose—he has fallen into the pit which his more

knowing master knew how to escape. We are strongly persuaded that his indiscretion, rather than any bad design, betrayed him. It can scarcely be doubted but that his intention in modifying the libel from the Lancet, was to reduce its virulence by omitting its most glaring untruths; and that this was the case may be further inferred from the fact of his subsequent apologies: but it is almost ludicrous, at the same time, to observe the awkwardness with which he attempted to *strengthen* what he retained of the libel, by the silly use of italics, and other typographical embellishments. And strengthen it he did to his cost; for on this point the judge, in addressing the jury, particularly dwelt, and, not without much shew of reason, drew from it the *animus* of the defendant. At all events it will be a serious lesson to Dr. Ryan as long as he lives, especially should he ever again copy Wakley's libels, how he ventures upon improvements of his own.

But Dr. Ryan's edition of the libel, we maintain, would never have so ruinously defeated him, were it not for the unaccountably weak nature of the defence, and the way in which that defence was mismanaged. What in the name of common sense could have induced the parties to plead a justification, upon insufficient allegations, which they were not able to make good? Why not be better acquainted with the sort of evidence which the witnesses summoned for the defence could give? How monstrously absurd to call a number of medical witnesses, not one of whom could say that he had actually refused to meet Ramadge, though that was the very point to be made out! Yes, there was one—Mr. Macrea, we think, from Islington—who declined to allow Dr. Ramadge to be called in—but added, "that the patient did not require any farther advice." These absurdities had but a sorry appearance in the eyes of the Court; but this was not all. Some of the defendant's witnesses did him positive mischief—Dr. James Johnson, for instance, who was about as indifferent a witness as ever we saw in a court of justice. He was abundantly ready, to be sure, to speak the "truth," and, we sincerely believe, "nothing but the truth;" but the "whole truth" had to be extracted from him in a short but singular cross-examination, in which the temper of the "philosopher" was cruelly put to the test by the ferocious onset of the barrister—Adolphus. He had often said, it appeared, that he would not meet Ramadge since his disgraceful conduct in upholding Long, but then he had never had an opportunity of putting that excellent design into effect. "Did Dr. Ramadge ever refuse to meet *you* in consultation?" inquired the cross-examiner. "Yes, he did," replied the witness, "but that was when I had not got my licence from the College, having still an examination to put in." "Did you never hear that his refusal was in consequence of your having falsified a medical case?" "Never." "Did he never tell you that you had falsified a medical case?" "He dare not do it—no man dare charge me with doing so:—in my absence I certainly did hear that he made such a charge upon me in the society, but he never gave me an opportunity of explaining how that mistake arose," &c. The effect of such evidence we need hardly describe. That able advocate, Sergeant Wilde, saw clearly how such testimony served the cause of his client;—in his address to the jury after the defence was concluded, he took special care to comment upon it; nor did the judge neglect to notice the same circumstance when pointing out to the jury how lamely the justification of the libel was made out.

With such consummate *gaucherie*, in-

deed, was every thing conducted on this luckless trial, that not a single witness served the defendant's cause. Even Dr. Tweedie, who was depended upon as the mainstay of the defence, could not depose that the practice which he adopted in Miss Bullock's case, after he was called in, was so opposite in character or so different from that which was already employed, but that the treatment of the plaintiff might yet have been applicable at an earlier period. Neither did Dr. Tweedie, in the account which he had to give of himself in the witness-box, come off without something to criticise. It appeared upon his own showing that he visited Dr. Ramadge's patient before that physician's arrival, and after this, in the adjoining room, thought proper to ask a question, most undoubtedly a very singular one, and which could not fail to elicit an angry reply. "How do you stand with the profession?" says Dr. Tweedie. "You are an impertinent fellow to ask the question," says Dr. Ramadge; and so proceeded the fracas. If we were to attend only to Dr. Tweedie's account, nothing could exceed the mildness of his demeanour, but there happen to be two other witnesses, who bear testimony to "words,"—and "high words," between the doctors. The above question, it will be observed,—and of this also the plaintiff's counsel made abundant use—might be put in a great many different tones, the gentlest of which would be calculated to irritate any man: and under no circumstances could the anteroom of a patient far advanced in fever be a proper place for such discussion. Dr. Tweedie knew, before Dr. Ramadge arrived, that he was coming; and he either ought to have decided on acting with him, or stated that one of them must retire. How could Dr. Tweedie find it necessary to ask Dr. Ramadge "how he stood with the profession?" Was not the question superfluous?

It can scarcely, we presume, be requisite for us to guard against misapprehension as to the opinion we entertain of the conduct pursued by Dr. Ramadge with regard to St. John Long: on this subject, indeed, our sentiments are on record, and nothing has since occurred to change, or even to soften them. It is easy for an expert counsel, like Mr. Sergeant Wilde, to represent the honest indignation of a whole profession—the expulsion from the London Medical Society—or the censure of his College, as the effects of a combination to crush Dr. Ramadge. To us, however, it is new to hear that the learned Doctor had ever attained honours in science, or emoluments in practice, calculated to render him an object of envy to his brethren, or, indeed, that his reputation had extended beyond the immediate circle of his employers, till borne into notoriety as an appendage of the brighter fame of a successful quack. That in advocating the lucrative novelties which to others appeared impudent impostures he was actuated by disinterested considerations, we have no less an authority than his own for believing, nor may we venture, after this declaration, to suppose that he was at all influenced by the hope that some of the straggling rays of sunshine which brighten the atmosphere of Harley-Street, might, by possibility, reach his humbler sphere, and the fortunes of the follower thus be gilded by the reflected glory of the original.

Still Dr. Ramadge must be prepared to find that there will be differences of opinion as to the extent to which he has gained by these actions: some may be disposed to think that the jury who gave him 400*l.* for the injury done to his character, were a trifle above the mark; and on the other hand, there may possibly be those—but we will not speak positively—who think the estimate at "one farthing" too low.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The second meeting of the British Association for the advancement of Science was held at Oxford, on Monday, the 18th June, and continued on the subsequent days of that week. It may be proper to recal to the minds of our readers, that the first meeting of this great association took place at York, last year, under the most distinguished patronage. The present meeting, held within the venerable walls of the University of Oxford, and under the patronage of some of its most distinguished ornaments, has been attended with the most brilliant success.

Monday was occupied with preliminary arrangements, and especially the formation of sections and committees, in which the numerous papers on different branches of science submitted to the association on this occasion were to be read, and where the votaries of science were collected together might enjoy the advantages of a mutual interchange of ideas. On the evening of that day the members of the Association were invited to attend at the Clarendon building, for the purpose of scientific conversation.

At ten o'clock on Tuesday morning, the following committees met in different apartments of the Clarendon building:—

1. The Committee of Mathematical and Physico-Mathematical Sciences.
2. Of Chemistry, Electricity, Galvanism, Magnetism, and Mineralogy.
3. Of Geology and Geography.
4. Of Natural History (including Medicine).

These committees appointed each its own chairman and secretary, and were employed, between the hours of ten and one, in their respective departments of science. At one o'clock the various committees met in the great theatre. Lord Milton, the president of the preceding year, delivered an eloquent address, on resigning his duties to Dr. Buckland, who, on taking the chair, opened the business of the meeting by an appropriate speech. Professor Airey, of Cambridge, then read his promised report "on the state and progress of astronomical science, in reference particularly to physical astronomy." He was followed by the Reverend Professor Whewell, of Cambridge, who read a report furnished by J. W. Lubbock, Esq. Vice-President of the Royal Society, "on the means of calculating the time and height of high water." These valuable reports were listened to with the utmost attention, by a crowded audience, which included the beauty and fashion of Oxford.

The members of the Association resident in Oxford, afterwards gave a sumptuous entertainment to their fellow members, in the great Hall of New College. Two hundred and fifty-three noblemen and gentlemen sat down to dinner on this occasion. Dr. Buckland was in the chair, supported on his right hand by Lord Milton, and on his left by the Vice-Chancellor of the University. Among the company present, we noticed the Marquis of Northampton, Lord Selkirk, Lord Morpeth, Lord Sandon, Viscount Cole, Sir Thomas Acland, Sir Thomas Brisbane, Sir David Brewster, Mr. Davies Gilbert, Professor Hamilton of Dublin, the Rev. A. Sedgwick, of Cambridge, &c. &c.

A variety of appropriate toasts and speeches enlivened this social meeting. On the following morning the whole Association breakfasted, by invitation, with the Vice-Chancellor, the head of Exeter College. The hall of this college being insufficient to accommodate the numerous party assembled, tables were laid in the gardens. At ten o'clock the Association adjourned to the Clarendon, where, separating into their respective sections, scientific business was resumed, as on the preceding day.

Many interesting papers upon different branches of science were read at the sectional meetings on this and the subsequent days, which want of space prevents us from enumerating. We must make an exception, however, in favour of one paper, bearing more directly than others upon medical science, —namely, Dr. Prout's important "Observations on Atmospheric Air;" in the course of which, this distinguished philosopher pointed out, that, in London, the air underwent a remarkable and sudden increase in its specific gravity, at the precise period when cholera first appeared there.

The reports read at the *General* Meeting, on Wednesday, were—"On Ther-

mo-Electricity, and on the allied subjects in reference to the discoveries recently made in them," by the Rev. Professor Cumming, of Cambridge. "On the present state of Meteorological Science," by James David Forbes, Esq. F.R.S.L. & E.; and "On the Phenomena of Sound," by the Rev. Robert Willis, of Cambridge.

On the evening of Wednesday, Mr. Ritchie, of the Royal Institution, delivered a popular lecture on the recent discoveries in electro-magnetism. Dr. Turner gratified a numerous audience by a display of experiments illustrating the phenomena of chemical action.

The morning of Thursday was set apart for the ceremony of conferring, in full convocation, honorary degrees on four of the most distinguished cultivators of science, members of the association, unconnected with the university of Oxford—namely, Sir David Brewster, Mr. Brown the well-known botanist, Mr. Faraday, and the venerable John Dalton. At the conclusion of this ceremony, honourable alike to the university and the association, Dr. Buckland proceeded, with a numerous equestrian party, to survey the geology of the neighbourhood; while Professor Henslon, with a party of pedestrians, enjoyed a botanical excursion. Sectional meetings were held in the evening, in which important discussions took place.

The reports read at the general meetings of Friday and Saturday were "on the progress of optical science," by Sir David Brewster; "on the state and progress of mineralogical science," by the Rev. Professor Whewell, of Cambridge; "on the phenomena of heat," by the Rev. Professor Powel, of Oxford; and "on the recent progress of chemical science," by James F. W. Johnston, Esq. F.R.S.E. The business of the meeting concluded on Saturday with an interesting lecture "on fossil remains," by Professor Buckland.

It was agreed that the next annual meeting should be held at Cambridge, and the association separated with the most lively feelings of gratitude towards the university of Oxford for the uniform attentions and hospitality bestowed upon them during this week, so memorable in the annals of British science. We doubt whether upon any former occasion so many distinguished ornaments of science from all parts of the British dominions were ever assembled together.

(*From a Correspondent.*)

In consequence of the unanticipated extent of this Association, and the great accumulation of matter, the meeting not being previously prepared with a suitable plan of organization to meet the emergencies, the sister sciences of medicine do not appear, at the late meeting, to hold that place in the programme to which their importance and interest entitle them. Several scientific members of the profession appeared at different periods of the week, and enrolled their names; but the display of animal and vegetable physiology was somewhat meagre. The arrangements for sectional papers and oral communications were, we understand, thrown into some confusion latterly; and a forthcoming report by Mr. Broughton, on some recent physiological investigations, could not obtain a hearing before the morning of the last day, when the greater proportion of the members devoted to such subjects had left Oxford.

The popular exhibitions of the two great leaders in geology — Professors Buckland and Sedgewick—absorbed the almost undivided attention of the meeting, whenever these two talented geologists lectured; so that other sectional communications were necessarily postponed upon such occasions.

The experience of this meeting will no doubt enable the next to adopt more satisfactory arrangements upon several points; and we have learnt that the science of physiology is to be honoured in future with a separate section devoted to it—the medical sciences having been classed, on the recent occasion, under the general section of natural history.

From what we hear, it is quite clear that no blame attaches to the parties appointed to manage the proceedings of the Association; and we feel quite assured that a timely understanding of the various objects of the society will have the effect of producing a fuller contribution of physiological and medical subjects in general.

No one can for a moment wish to see any one department in science predominating over others, constructed as this Association is intended to be, of the

representatives of universal science. On this score, therefore, objections have been urged to the successive appointments of geological professors to the presidency — objections which, we believe, will be removed after the next meeting at Cambridge; and we trust that care will be taken to balance the contributions in each department, so as to render them all equally diffused, as to the opportunities afforded to their respective contributors.

We have been informed, that, after Mr. Broughton's physiological report had been read and discussed, a letter was read by the President of the section, strongly recommending to its notice some schemes of Mr. St. John Long's concoction, about the rot in sheep, and other matters relating to his general practice, certified by a committee associated for the purpose, and formed by the Viscount Ingestrie and a few others (Lord Sligo perhaps), whose names could not be caught, from the uproarious merriment which the announcement throughout excited, and which arrived at its climax when the names of the above *enlightened* nobles popped out!

Our readers cannot have forgotten the *Viscount's* testimony to his protégé's skill and science, *in drawing a substance resembling quicksilver from the necks and backs of children* placed under the friction system of Mr. St. John Long; and the Committee, we trust, will not be imposed upon by this insidious *Sligo*. On this account and some others, we think it adviseable that papers should be examined by a committee before they are received, notwithstanding the discretionary power existing as to their publication; otherwise we may see the Association disgraced in the eyes of Europe by communications from the most determined and impudent quacks in the country, and the bright names of Brewster, Dalton, Brown, and Faraday, on whom the University conferred honorary degrees, with many others illustrious in science, tarnished by the vile impositions and incorrigible ignorance of our Columbines, Eadys, and Longs.

REPORT OF THE TRIALS FOR LIBEL.

Court of Common Pleas, June 25, 1832.

(Sittings before Mr. Justice Bosanquet and a Special Jury.)

Ramadge v. *Wakley.*

This was an action brought by the plaintiff to recover compensation in damages from the defendant for a libel.

Mr. Serjeant Wilde and *Mr. White* conducted the plaintiff's case; the defendant appeared in person.

Mr. Serjeant Wilde proceeded to address the jury. The plaintiff was a physician of respectability and talent, residing in Ely-Place, and also a lecturer, and he believed doing credit to his profession, both by his practice and talent: he complained upon the present occasion of the defendant having published a libel in the *Lancet*,—a publication that undoubtedly possessed merit in some respects, and which had rendered itself very well known by other matters, perhaps not so much entitled to encomium. Mr. Wakley, every body must know who is at all acquainted with what is passing in the world, was a man of considerable talent, and therefore perfectly cognizant of the best means of effecting any object he might have in view. Upon the present occasion none of the ordinary topics about the liberty of the press could at all come in question, except, indeed, one portion of the liberty of the press, which was rather peculiar to the present day. It seemed that certain publications, the *Lancet* among the rest, as they would infer from the article in question, consider the liberty of the press to consist of the destruction of every body against whom the editor or his friends entertain ill will. To prescribe the conduct that the whole medical world should pursue, and, if any should deviate from it, by every means, to seek their destruction, was a liberty to which very few would accede. Upon the present occasion the libel referred to some transactions with a person whose name had also been very much before the public—Mr. St. John Long, who had adopted a new mode of practice in certain medical cases. That a new mode of practice, whether attended with success or not, should excite considerable objections, no one could be at all surprised. Of the merits of that practice he (Mr. Serjeant Wilde) knew nothing; but it had excited considerable opposition among the regular practitioners; and it was inferred from this libel, that Mr. St. John Long had requested Dr. Ramadge's opinion, and, he had given it in writing, which had created a feeling of disapprobation in the minds of his medical brethren. His not having joined in the general cry against Mr. Long appeared to have been the

foundation of a determination to destroy Dr. Ramadge; and but for which he would have been content to have left the merits of his practice to have been judged of by the public. The libel was a notice to all medical men, that they were not to exercise their own judgment, or to give their real and impartial opinion upon medical subjects. The facts to which the libel referred were briefly these:—A respectable gentleman, of the name of Bradford, was in attendance upon a young lady in July last. Her illness in an early stage was not alarming, but Mr. Bradford, having occasion to leave town, requested Dr. Ramadge, as his friend, to visit, which he consented to do. The patient grew afterwards worse, and ultimately the symptoms became alarming, which rendered it necessary to apply strong remedies at a certain period, the lady being then in a dangerous state. Dr. Tweedie was then called in, and this libel related to a difference that arose between him and Dr. Ramadge, Dr. Tweedie thinking fit to decline consulting with him on account of the opinion he had expressed regarding Mr. St. John Long. The fact of Dr. Ramadge having expressed an opinion regarding that gentleman's practice was allowed to deprive the patient of the benefit of the conjoint judgment of the two. They having been called in solely to confer respecting the condition of a particular patient, both of them being men regularly exercising the profession, which they were called upon at the moment to act in, one of them thought fit to offer to the other the insult of declining to confer with him on account of some other matter, relating to some other individual. In the end, however, the situation of these two gentlemen became so unpleasant that it was necessary for the friends of the patient to decide who should remain, and, ultimately, Dr. Tweedie, who had attended some part of the family before, was requested to continue his attendance, and shortly after this paper appeared—and he begged them to note that the article was dated the 28th of July, but the day of its publication was the 20th of August. It purported to give an account of the treatment of Dr. Ramadge prior to Dr. Tweedie's being called in, and of Dr. Tweedie's treatment subsequently, and its results; and it gave such an account of Dr. Ramadge's treatment as represented him to have pursued a course most violent and dangerous, which was calculated to ruin him in the opinion of the medical world. The libel was in the shape of a letter, and was headed—

"*Result of Upholding Quacks.*

"Sir,—The following account of a medical fracas shows that your strictures against quacks and their coadjutors are duly appreciated by respectable medical men. Ten days ago Miss Emma Bullock, of No. 1, Gloucester Place, Old Kent Road, was attacked with a fever that was declared to be typhus. The young lady was attended by Dr. Ramadge and ——, a medical practitioner, in —— Street, and from some reason she was not bled until Friday, when a vein in the arm was opened, and eight dozen of leeches to the head and neck were applied. On Saturday the temporal arteries were opened on both sides; she fainted, and the apothecary having left her, the nurse succeeded in bringing her round with a teaspoonful of wine and water, and from a state of delirium she became rational, but very weak. On Sunday another dozen of leeches were applied, and immediately afterwards she became delirious, when Dr. Tweedie's advice was required by the lady's relations. That gentleman, on his arrival, spoke to Dr. Ramadge privately, the purport of which may be gained from what followed. Dr. Tweedie, in the presence of Dr. Ramadge and ——, addressed Mrs. Reynolds, the sister of the patient, and said that, having attended the family before, he should be happy now to give his assistance to the young lady, but that Dr. Ramadge's conduct in the late correspondence with John Long had been such that no medical man of respectability could call him in, or consult with him, without injuring himself in the eyes of his brethren. That he (Dr. Tweedie) bore no private pique against Dr. Ramadge. He believed him, indeed, to be clever, but his character as regarded the above transaction rendered it imperative for all medical men to decline acting with him; and Mrs. Reynolds must therefore choose which she would intrust. Dr. Ramadge replied, in great anger, that he was a gentleman by birth, education, and profession, but that Dr. Tweedie was neither. Dr. Tweedie answered him by turning coolly on his heel, and walking out of the room. It is needless to say, that after this Dr. Tweedie was retained. He ordered the instant cessation of mercurial friction, of the tea-spoonsful of calomel, the draughts, pills, potions, powders, &c. A composing draught was given, and nourishing beef-tea ordered. Lotions to the head, &c. were applied, and under this treatment the patient is recovering. 'Who can decide when doctors disagree?' What can we poor uninitiated think of such opposite opinions? The names of the parties are stated in full, that the truth may be ascertained. I am told that Dr. Ramadge is frequently at supper with John Long.

(Signed) "One."

Dated July 28, the very day on which this young lady, under this successful treatment, died. The Learned Sergeant concluded by observing, that there were two questions for consideration—whether the defendant was the publisher of the libel? which

was admitted; and whether the practice of and treatment of the two physicians were represented with candour and *bona fide*?

Mr. Robert Bradford examined by Mr. Adolphus.—I am a surgeon and apothecary residing in Fleet-Street. In the month of July 1831, I was in attendance on a young lady of the name of Emma Bullock, residing with her sister in the Kent Road, near the Bricklayers' Arms. She was attacked with a common inflammatory fever in the first instance. I had occasion at that time to go out of town, and as Dr. Ramadge was attending another lady opposite, I asked him if he would go over the way and see a patient of mine, which he consented to do. I first attended on Friday the 15th July. The friends of the patient afterwards wished Dr. Ramadge to be called in, and he was accordingly called in on Thursday the 21st of July, and he prescribed ten grains of calomel to be given in a bolus. I saw my patient next morning, in company with Dr. Ramadge. The fever had considerably increased; the pulse was full and strong; she had passed a bad night, and showed symp toms of high inflammatory excitement. Dr. Ramadge ordered thirty leeches to be applied to her temples, a cold lotion to her head, which I had shaved the previous evening, and she had some fever draughts as well. Dr. Ramadge saw her the next morning, Saturday 23, when he ordered the lotion to be continued, and mercurial ointment to be rubbed on the calves of her legs. He (Dr. R.) saw her again on Sunday evening, in company with Dr. Tweedie. They had some words together, and, I believe, Dr. Ramadge did not see her after that. Dr. Ramadge signed a prescription with Dr. Tweedie, who afterwards tore off Dr. Ramadge's initials, saying that there was no consultation, and therefore he would not have them there. After Dr. Ramadge left her, I continued to visit her, and received Dr. Tweedie's commands until Wednesday evening the 27th July, when she died.

Cross-examined by Mr. Wakley.—Miss Bullock was affected with synocha. The temporal arteries were opened on the same day as the leeches were applied. I believe, when I first saw Dr. Tweedie, he and Dr. Ramadge were standing round the table, and were in altercation. Dr. Tweedie took me by the arm, and said, "I want to speak a word with you," and we went out. When we were in the room there were very strong expressions passed on both sides. Dr. Ramadge told Dr. Tweedie that he was no gentleman; and Dr. Tweedie replied, "You are a fellow that I can have nothing to do with." Dr. Ramadge said they were called there in consultation upon the patient's case, and he did not expect such conduct from Dr. Tweedie. It was unjustifiable or improper, or words to that effect. Dr. Ramadge said that he was a gentleman in every respect, by birth and education—that Dr. Tweedie was not, but the son of a milliner. Dr. Tweedie alleged as a reason for not meeting Dr. Ramadge, that no medical man would meet him, in consequence of his behaviour to, and connexion with, St. John Long. Dr. Ramadge made general observations justifying his conduct, and stating that he had a right to act and think as he pleased—that what he had done he did from conscientious motives, and Dr. Tweedie had nothing at all to do with it. Dr. Ramadge said nothing about his publication in vindication of St. John Long; but he said he had no connexion with Long, never saw him, and did not even know his person at the time of the publication, and his first connexion was through Mr. Harmer, the attorney, who met him somewhere in the city, and said that Long had been ill treated, and he replied that he could say something for him, and Mr. Harmer immediately replied, "You are the man we want." They afterwards both signed their initials to a prescription, but Dr. Tweedie tore Dr. Ramadge's away, saying, "I will pull off that fellow's initials; it was no consultation." Dr. Tweedie, in an angry discussion, asked Dr. Ramadge how he was received in society; to which he replied, "You are an impertinent fellow."

Mr. Wakley then addressed the Jury. He was quite at a loss what case he had to answer. None appeared to him to have been made out; but as his Lordship thought otherwise, he would as briefly as possible make a few observations in reply. What damage, then, had the plaintiff sustained? None whatever. He stands before this Court now, as he did before he entered it—a man who was lost to all feeling. How he stood in his profession who had become the advocate and acquaintance of John Long, who had twice stood his trial at the Old Bailey, and on the last occasion was convicted of manslaughter, and was sentenced to some months' imprisonment in Newgate, it was needless to describe. Dr. Ramadge has two objects in view: 1st, to rob you of your senses; and, 2dly, to make it appear that he is a persecuted man because he had taken up the cause of another persecuted man, who had *rubbed* some people out of their existence, and out of their cash. It was, perhaps, not known the manner in which the system was carried on at Long's house. Duchesses used to go to his house in Harley-Street, because they understood he was a good rubber, (*laughter*) and all sit there in rows, and inhale out of tubes, that led to a large chest, filled with common air, but which he makes them believe includes magic air, and there they suck from Monday morning to Saturday night—they sucking from his tubes, and he sucking from their pockets (*great laughter.*) Then goes the conversation round:

"How are you this morning?" "How are you? do you find yourself better?" "Have you been to any physician?" "Oh, yes! Dr. Ramadge, and he tells us that you have taken quicksilver in a fluid state from the brains of several insane persons." These people (continued Mr. Wakley) he supposed would believe that John Long could give good tough leather lungs to a consumptive patient, or reanimate the dead bodies in a churchyard. That noble lords might believe such trash he could easily enough imagine; but how Dr. Ramadge could, who was acquainted with the constituent parts of the human frame, he could not conceive. Mr. Wakley then proceeded to comment upon the plaintiff's having kept back a witness, and also a nurse, who he was informed had since died. Dr. Ramadge, he supposed, had attended her in anticipation of this cause; but he should have been able to have asked some material questions if she had been produced. He should have shewn that Dr. Ramadge was entirely deficient in skill, and he attributed the death of Miss Bullock —and it was a subject to which he recurred with a melancholy feeling—entirely to him; for when Dr. Tweedie was called in, who also ought to have been produced, she was too far gone, and he was obliged to pursue a treatment entirely different to what Dr. Ramadge had adopted. After assuring the jury that he had made inquiries before he inserted the letter, which he now believed was written by John Long or Dr. Ramadge himself, for the purposes of this action, Mr. Wakley concluded by appealing to them as a British jury, as the protectors of the liberty of the press, as the shield between the public and unprincipled men, who would sport with the feelings of the people, and trample upon the honour of a noble profession, to scout that man (pointing to Dr. Ramadge) from the Court, and return their verdict for the defendant.

Mr. Sergeant Wilde then claimed the right to reply to the facts which Mr. Wakley had stated in his speech relative to the witnesses, &c. but

His Lordship thought that the learned Sergeant, if he had intended to have replied, ought to have stopped the defendant in his speech.

Mr. Justice Bosanquet then charged the jury, and left them to say whether the letter was inserted from a spirit of fair and candid criticism, or to gratify some feeling of hostility which the defendant entertained towards Dr. Ramadge. He told them that they must entirely throw aside all that Mr. Wakley had said regarding John Long, and other matters which were not in evidence; but that in judging of the motives which led to the publication of the libel, they were not to lose sight of the animus which Mr. Wakley had displayed in the manner of conducting his defence, in illustration of which the learned judge quoted from his notes some of the many violent and abusive expressions of which the defendant had made use.

The jury, after consulting together for a short time, wished to know what was the lowest sum to carry costs?

Upon his Lordship intimating that any sum would, the jury immediately found for the plaintiff—damages—*One Farthing*.

Tuesday, June 26.

(Before the Lord Chief Justice and a Special Jury.)

Ramadge v. *Ryan and Others.*

This was an action for a libel in *The London Medical and Surgical Journal* of September 1, 1831, against Dr. Ramadge, of Ely-place, Holborn; the publication being edited by Dr. Michael Ryan, and published by Messrs. Renshaw and Rush, the defendants.

The defendants pleaded a justification.

Messrs. Serjeants Wilde, Spankie, and Mr. Adolphus, conducted the plaintiff's case; and Serjeant Taddy and Mr. Erle appeared for the defendants.

The following is the libel complained of:—

"Tweedie *v.* Ramadge.—Dr. Ramadge was in attendance on a case of typhus; the patient (a young lady) was bled from the arm on a Friday, and *eight dozen leeches* (96) applied to the head and neck. On Saturday both temporal arteries were opened—the patient fainted, and the apothecary (who was likewise in attendance) left her; the nurse brought her round with wine and water. On the Sunday, *another* dozen leeches were applied, and immediately she became delirious; when Dr. Tweedie's advice was requested by the relatives.

"Dr. Tweedle having spoken apart with Dr. Ramadge, addressed Mrs. Reynolds (the sister of the patient), and said, '*That having attended before, he should be happy now to give his assistance to the young lady,* but that Dr. Ramadge's conduct in a late correspondence with John Long had been such, that no medical man of respectability could call him in or consult with him without injuring himself in the eyes of his brethren. *That he (Dr. Tweedie) bore no private pique against Dr. Ramadge—he believed him, indeed, to be clever; but his character, as respected the above transaction, rendered it imperative for all medical men to decline acting with him; and Mrs. Reynolds must choose which she would entrust.* Dr. Ramadge replied in great anger, that *he was a gentleman by birth, education, and profession, but that Dr. Tweedie was neither.* * * * Dr. Tweedie answered him by turning coolly on his heel and walking out of the room. Dr. Tweedie was retained, and cured the patient by exactly opposite treatment. Dr.

Ramadge (it is said) frequently is at supper with John Long.—*Lancet.*

"Dr. Tweedie has honourably and faithfully discharged his duty to his medical brethren; and we hope every one else will do the same. We are well aware who it is —*and a medicul man to boot*—that makes the trio in these *family suppers.* Let him be warned in time. He takes upon him to defend this nefarious quack and manslaughterer in the face of the whole profession: let him take warning, or we will not spare him.—Ed."

Sergeant Wilde observed, that while these strictures purposed to have been made on an account that had previously appeared in another medical periodical, they far exceeded that account in malice and falsehood. Besides, a considerable period had elapsed between the publications; when the defendants might have ascertained the truth of their statments, if candour and fair criticism were their object; or that they had intended to promote the interests of science. Upon the present occasion, none of the ordinary topics about the liberty of the press can at all come in question—except, indeed, one portion rather peculiar to the present day—the denunciation of the character and conduct of individuals by editors, who may be actuated by motives of personal enmity or professional intrigue. That a person of skill and experience should be denounced because he had ventured to express fairly his sentiments of this new mode of practice, was not to be endured in this age of scientific research. Such was, however, his client's case; for writing a letter to St. John Long, (published in *The Sunday Times* of the 10th April, 1831) the faculty had endeavoured to oust him from his practice, and wrest from him his character. The learned Serjeant remarked on the manner in which the extract was introduced, the variations introduced into that extract, and the monstrous falsehood that the patient had recovered, and that under treatment exactly opposite, when in reality she had died in three days afterwards.

Mr. Bradford was the principal evidence to support the case, in the same manner as on the previous day.

Serjeant Taddy, in his defence, asserted that the statement of the case of Miss Bullock had been originally published in the *Lancet,* and was acknowledged to be extracted from that work, in which it appeared a month previous to the publication of the defendants, who, seeing no contradiction, were at liberty to think the statements accurate. He was prepared to justify those statements, although his clients had offered an apology to the plaintiff; and could prove that Ramadge had improperly treated Miss Bullock, ordering depletions when he should have ordered stimulants and strengtheners; and that in consequence of his connexion with John Long, he had been repudiated by the most eminent of the faculty.

Dr. Tweedie deposed to his being called in on Sunday, the 24th of July, to see Miss Bullock, and that he had refused to consult with Dr. Ramadge. He was, however, very chary of asserting that he had altered the course pursued by the plaintiff, merely saying he had not ordered depletions, &c. nor would he consent to having leeches applied again, as Ramadge had requested. He had given an opiate against the desire of the plaintiff. Miss Bullock's case was hopeless when he was called in; and he then so said from her distended abdomen, and other symptoms. He ordered her nourishing medicines, both food and physic, and introduced the catheter.

The connexion of the plaintiff with St. John Long, and his consequent expulsion from the Medical Society at a meeting of the members of that body in Bolt-Court, Fleet-Street, were then proved.

Mr. Brodie was called to prove that he had refused to meet Dr. Ramadge, but he stated that he never had done so; that he never had been requested to meet him; he was not aware that he had ever declared that he would not meet him. On the direct question, "would you meet Dr. Ramadge?" being put, the court interfered, and prevented him from answering.

Dr. A. T. Thomson was examined, and to the same purport as Mr. Brodie.

Dr. Uwins was called to the box, but not examined, it being understood that his evidence could go no farther.

Dr. James Johnson had frequently said that he would not meet Dr. Ramadge, but acknowledged, on being cross-examined by Mr. Adolphus, that Dr. Ramadge had publicly accused him of falsifying some case, adding, however, that he dared not to do so in Dr. Johnson's presence. It farther appeared that the witness was aware of Dr. Ramadge having declared that he never would meet Dr. Johnson.

A general Practitioner (whose name we did not hear) proved that he had declared he would not meet Dr. Ramadge.

Mr. Field, registrar to the London Medical Society, proved that soon after Dr. Ramadge's letter to St. John Long, a meeting was summoned, at which Dr. Ramadge was expelled,—only *one* member voting in his favour.

Dr. Babington, sen. Dr. Sims, Mr. Lawrence, Mr. Earle, and several other gentlemen, were in court, but were not examined.

Mr. Serjeant Wilde addressed the jury at some length, and with great force, pointing out the total failure of the attempt to justify, censuring in strong terms the evidence of some of those who had been called, and

attributing the whole to a conspiracy to put down Dr. Ramadge for supporting the discoveries of Mr. Long.

The Chief Justice said, the real point for consideration was not so much the general skill or system of science of the plaintiff, or his connexion with this or that person or body, but whether the alleged libel was a malicious and false statement of presumed facts, or whether it was a fair and full, an honest and candid account of a particular case, commented on with a view to elicit truth or institute a fair investigation. He remarked particularly on the alterations in the type of the extract from the Lancet, as well as the alterations in words. It was no plea of justification that the account had been published by another; for the plaintiff could prosecute for a second-hand as well as for an original account; nor, since justification had now been pleaded, could the apology formerly tendered avail the defendants.

The Jury retired to consult for a short time, and returned with a verdict for the plaintiff—Damages, *four hundred pounds.*

ROYAL INSTITUTION.

Friday, 1st June, 1832.

Mordan's Apparatus for manufacturing Bramah's Locks.

In order to explain to the visitors the uses of the various parts of Mr. Mordan's beautiful machinery, Mr. Faraday gave a rapid view of Bramah's original lock, with its several subsequent improvements. By means of a model on a large scale, he showed the principle of the spring and moveable wards, with their notches; and demonstrated also the principle on which the lock, as it was first constructed, had been "picked." Picking Bramah's locks, however, he proved was now out of the question, since the ingenious expedient of *false notches* was hit upon. In constructing those inimitable locks, the occurrence, it appears, of any two of them being identical cannot take place in some thousand years. The key is first made with its seven wards and notches, then the parts to which it is to be adapted; but the selection of those wards is regulated with so much more than even clockwork precision, and the theory of permutations and combinations so fully brought into play by a wheel of Mr. Mordan's having in it 199 varieties of notchings, that it is utterly impossible any coincidence of structure could occur within any limited period.

Some of Mr. Mordan's workmen were in attendance, and after the lecture gave the most satisfactory practical information relative to the mode in which the apparatus was used.

PROFESSORSHIP OF MATERIA MEDICA, EDINBURGH.

We are happy in being able to state that Dr. Christison has been elected to the chair of Materia Medica, in the University of Edinburgh: an appointment eminently calculated to uphold the character of the school.

COLLEGE OF PHYSICIANS.

Press of matter compels us to postpone our report of the meeting of the College of Physicians on Monday.

DUBLIN JOURNAL OF MEDICAL AND CHEMICAL SCIENCE.

As a notice of the above is included in an advertisement of the Gazette which has been inserted in some of the newspapers, we think it right to state that the length of the trials for libel rendered its postponement unavoidable.

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

June 1832.	Thermometer.		Barometer.	
Thursday . 21	from 45 to	70	29·90 to	29·84
Friday. . . 22	51	67	29·62	29·59
Saturday . 23	49	67	29·60	29·70
Sunday . . 24	47	69	29·76	29 89
Monday . . 25	47	69	29 95	29 99
Tuesday . 27	43	71	30 03	30·09
Wednesday 28	45	74	30·14	30·22

Prevailing wind N.W.

Rain fell on the 21st, 22d, and 25th; the other four days generally clear.

Rain fallen, ·625 of an inch.

Charles Henry Adams.

BOOKS RECEIVED FOR REVIEW.

Dr. Favell on the Nature, Causes, and Treatment of Spasmodic Cholera.

Mr. James on the general Principles and on the particular Nature and Treatment of various species of Inflammation.

Mr. Sharpe's Manual of Percussion and Auscultation. Composed from the French of Meriédec Laennec.

Mr. Hayden's Lecture, introductory to a Course on Midwifery, and Diseases of Females and Children, delivered at the Anglesey Lying-in Hospital.

NOTICE.

J. W. C.—The publication of the letter could serve no useful purpose, without the writer's signature in full.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 7, 1832.

SUBSTANCE

OF THE

LUMLEIAN LECTURES,

Read before the Royal College of Physicians,

By Dr. Watson.

May 1832.

Lecture I.

On Hæmorrhage from the Stomach.

Dr. Watson began by observing, that in commencing the biennial course of Lumleian lectures last year, he took up the consideration of those forms of *hæmorrhage* which fall to the care of the physician. He then briefly recapitulated the topics of the three preceding lectures. In the first of them a general view was taken of this subject of *internal hæmorrhage.* The probable origin of that still prevailing error was pointed out which ascribes every case of the escape of the blood from its proper vessels to rupture, or some other injury of those vessels. Various facts and considerations were stated, which seem to prove that in a great majority of instances the blood is effused through those pores or outlets which afford a passage to the natural fluids of the part, and to which we apply the name of exhalants. For the more convenient consideration of these hæmorrhages by exhalation: they were divided into two distinct classes, the idiopathic, or those which are independant of any discoverable change of texture in any part of the body; and the symptomatic, or those which are connected with organic disease; and this latter class was subdivided into two species, the first of them including those cases in which the hæmorrhage is dependant upon disease in the very part from which the blood proceeds; and the second, those cases in which the disease is situated in some other part, capable of influencing the circulation in the former, by reason of some intelligible connexion between them, either of structure, or function, or mutual relation.

This division of the subject enabled the lecturer to shew that hæmorrhage, strictly of the idiopathic kind, is of rare occurrence in comparison with that which is merely a symptom of some manifest disease, unless, indeed, that singular species of hæmorrhage be regarded as idiopathic which takes the place of some natural or habitual discharge, and especially of the menstrual evacuation in females.

It was shewn, further, that hæmorrhage by exhalation is very frequently preceded or accompanied by an evident accumulation of blood in the capillary vessels of the part affected; that this local congestion may happen in several ways, and from various causes, easily distinguishable from each other; and that attention to this circumstance supplies a rational ground for the analogy which has often been traced between hæmorrhage and inflammation; and furnishes, at the same time, the means of assigning the limits within which that analogy is confined.

The various hypotheses were next considered which have been brought forward with the view of explaining the proximate cause of hæmorrhage by exhalation; and it was shewn to be inconsistent neither with sound philosophy, nor with the strict observation of facts, to admit the validity of several of these hypothetical explanations: hæmorrhage being, in truth, a phenomenon which differs greatly in its nature in different cases, and which is dependant upon many distinct causes for its production.

With regard to the observed phenomena of hæmorrhage by exhalation, it was stated that it occurs much more readily and frequently from the mucous membranes than from any other tissues of the body; that it takes place from certain parts, in preference, according to the age of the person; that it is sometimes accompanied by circumstances which characterize it as being of an active or a passive kind; and that this distinction would be of great practical value if it were universally, or even generally applicable;

that most hæmorrhages, however, cannot be said to possess either an active or a passive nature; and in explanation of this prevailing indefinite character, it was stated that the distinction of active and passive applies chiefly to the small class of idiopathic hæmorrhages, while those which are symptomatic derive *their* character from that of the disease of which they form a part.

Attention was directed also to that remarkable form of *constitutional* hæmorrhage, as it has been called, to which some persons are habitually subject through a great portion of their lives, which recurs without any apparent detriment of their general health, is independant of any obvious exciting cause, and bears a marked analogy, in many particulars, to the periodical secretion from the uterus in women.

To this general sketch of the history and varieties of internal hæmorrhage was added a cursory account of its symptomatology.

In the second lecture was commenced a review of the principal hæmorrhagic diseases as they occur in different parts of the body. Epistaxis was shortly spoken of; and a somewhat fuller account was given of those formidable effusions of blood that take place within the cranium. Allusion was made to the light which a careful study of the symptoms and morbid anatomy of *cerebral hæmorrhage* seems calculated to throw upon the functions of particular parts of the brain. Those changes were described which are gradually effected in the extravasated blood, and in the cavity formed by it, when the apoplectic seizure has not proved immediately fatal; and illustrations of these alterations were exhibited from the museum of the college. One remarkable circumstance was particularly noticed, in which hæmorrhage of the brain differs from most other internal hæmorrhages—viz. that it seldom takes place by the way of exhalation, but results most commonly from the rupture of a blood-vessel. Some reasons were assigned for this difference, founded partly upon certain original peculiarities of texture and relation belonging to the vessels themselves, but chiefly upon their great liability to disease. This dependance of cerebral hæmorrhage upon changes produced by disease in the blood-vessels of the brain, explains in a satisfactory manner several of the most striking phenomena connected with sanguineous apoplexy: why (for instance) it is so especially a disease of advanced life; why the blood is most frequently extravasated in certain parts of the brain, the corpora striata namely, the optic thalami, and the parts immediately contiguous to these; why this particular form of apoplexy is so sudden in its attack, and is so seldom indicated by premonitory symptoms; and why it is so often associated with hypertrophy of the left ventricle of the heart. The circumstances which chiefly govern the diagnosis and the prognosis of cerebral hæmorrhage, were also touched upon.

The third, and last of the former course of lectures, was occupied with the important subject of pulmonary hæmorrhage. The substance of this lecture has already appeared in this journal in the report of a clinical lecture, delivered by Dr. Watson on the same subject in the Middlesex Hospital*.

Dr. Watson stated, that his original hope and design in the commencement of the lectures, had been to consider in succession the various hæmorrhages to which the human body is liable; but that the extent of the subject, and the difficulty he found in condensing it without becoming obscure, obliged him to confine his observations to some of the most common and remarkable forms of internal hæmorrhage, and to omit much that is worthy of notice in regard even to them.

He then proceeded to the consideration of *hæmorrhage from the stomach;* and explained that he used that phrase rather than the single term hæmatemesis, because the latter, signifying strictly a vomiting of blood, does not necessarily imply, nor indeed always accompany, hæmorrhage from the stomach; although it is one of its most common and most striking symptoms.

The peculiarity so frequently mentioned before, in regard to hæmorrhages from the mucous membranes generally, obtains also in this particular hæmorrhage. The effusion of blood is very seldom occasioned by the rupture of a large blood-vessel. It can rarely happen that any vein or artery belonging to the stomach is laid open by accidental injury, so as to pour out its blood. When hæmorrhage does take place from one or more of the larger blood-vessels, the aperture by which the blood escapes is commonly the result of chronic ulcerative disease. Dr. Watson had never met with an instance even of this kind. Andral states, that, at most, there are but five or six such cases to be found in the records of medicine.

[Three examples of fatal hæmorrhage from this cause were here detailed: of these, one was quoted from the Journal Hebdomadaire de Medecine, for May 1830; another occurred last year, in one of Dr. Latham's patients, in St. Bartholomew's Hospital; the third had lately fallen under Dr. Clark's observation in private practice. In all of these cases there had been marked symptoms of gastric disorder for some time previously to the hæmorrhage, and two of the subjects of it, whose habits of life were ascertained, had been habitual drunkards. A drawing by Dr. Carswell, from the stomach of Dr. Clark's patient, was handed round; shewing, upon its inner surface, several distinct cicatrices of former ulcers. In each

* Vid. Med. Gaz. vol. ix. page 623, 655.

of the three cases a small ulcer, lying immediately over the track of the coronary artery, had eaten its way through the coats of that vessel.]

But hæmorrhage from the mucous membrane of the stomach, and from that of the alimentary canal generally, takes place far more commonly by *exhalation;* and this kind of hæmorrhage occurs under various circumstances, and is attended with different degrees of danger. The evidence that the blood is *exhaled* from the membrane is, in these cases, very satisfactory and conclusive; because we are able to scrutinize closely the whole extent of the mucous surface, which cannot so well be done with regard to the mucous membrane of the lungs. When death has followed immediately upon the hæmorrhage, and has, indeed, been its rapid effect, the membrane has, again and again, been found completely entire and of its natural consistence and texture throughout; sometimes partially red, and pulpy, and vascular—sometimes universally so, the submucous capillary net-work of vessels being still gorged with blood—sometimes quite pale, the same system of vessels having been completely emptied by the last hæmorrhage—and sometimes studded with minute dark points, which could be made, by slight pressure, to start from the surface, and looked like grains of black sand. This latter appearance is very corroborative of the opinion that the blood escapes through the natural pores or channels, which it cannot enter so long as the solids and fluids of the body retain their healthy condition. These sand-like bodies are doubtless small portions of blood which have coagulated in the exhalant orifices of the membrane, and received from them their shape.

Hæmorrhage strictly idiopathic—*i. e.* independant of any apparent change of texture in the surface itself, or in any part obviously capable of influencing its blood-vessels—is as rare, probably, from the mucous membrane of the stomach as from the lungs; if we do not reckon among the idiopathic hæmorrhages those which are vicarious of the catamenia. The lecturer had never seen nor heard of any instances of hæmatemesis analogous to the epistaxis which is so common in children and young persons, and which affords the most familiar example of idiopathic hæmorrhage.

But hæmorrhage from the stomach, occurring in connexion with other constitutional hæmorrhages, or in their place—and, above all, occurring vicariously of menstruation—is abundantly common; it is the *most* common, indeed, of all the species of hæmorrhage by deviation. In a former lecture, when speaking of pulmonary hæmorrhage, Dr. Watson related some striking instances in which menstruation had taken place, so to speak, for years together through the lungs, without any apparent injury to the general health. A short example of a similar kind was quoted in reference to the stomach. Among the patients of the celebrated Hoffman was a woman of Amsterdam, who, for eight years, remained subject to a bleeding from the nose, which came on regularly every month, a few days before the menstrual period, and ceased upon the flowing of the catamenia: then the locus of this periodical hæmorrhage was changed, and for six years more, instead of epistaxis she suffered hæmoptysis, occurring under exactly similar circumstances—every month she had slight cough, and expectorated blood. At the time when she was under Hoffman's care, the hæmoptysis had ceased for six months, but it had been replaced by hæmatemesis, which returned every month, a little while before the appearance of the menses, and ceased when the natural discharge became fully established. This woman was plethoric, lived fully, and led an indolent life. Dr. Watson had selected this case the rather because it illustrates, in a short compass, several of the points in the general history of hæmorrhage touched upon in a former lecture. Thus it marks, not only the connexion between the uterine functions, and discharges of blood from several other organs of the body, but also the influence assigned to the different periods of life in succession in determining which organ shall assume the hæmorrhagic disposition. It exemplifies also the manner in which a tendency to hæmorrhage may be allied to constitutional plethora, and be fostered by individual habits of life.

Hæmorrhage from the stomach, vicarious of regular menstruation, is not generally thought to have any tendency to shorten the existence of those who are afflicted with it. Cullen states broadly, that this species of hæmatemesis is hardly ever a dangerous disease; and this is true; yet it is not so entirely free from peril as to preclude the necessity of some caution and qualification in stating the prognosis in such cases. The exhaustion and debility resulting from the mere loss of blood are sometimes so great as to create serious alarm for the patient's safety; and Mr. North has stated, that he has met with two instances in which suppressed menstruation was followed by repeated and at length fatal hæmatemesis. In neither of these women was the general health seriously deranged; nor, previously to the hæmorrhage, did there exist debility, or any other symptom calculated to induce the apprehension of danger; in fact, in both of these cases a strongly favourable prognosis was given by experienced physicians, a very short time only before the fatal event.

Hæmatemesis is sometimes also supplemental of constitutional hæmorrhois, and subsides when the latter is re-established.

Hæmorrhage from the stomach, independant of disease in that or in any other part, sometimes happens in the advanced periods of utero-gestation. Yet though it does not, in these cases, result from disease, it is difficult to class it among the idiopathic hæmorrhages. The want of periodical recurrence, and the absence of the hæmorrhage during the earlier months of pregnancy, are circumstances which sufficiently refute the old notion that this form of hæmatemesis depends also upon the discontinuance of the catamenia. It seems to be caused by the same kind of pressure and mechanical obstruction of the circulation, as is produced by certain diseases of the abdomen—to be adverted to presently.

Hæmorrhage from the stomach, by the way of exhalation, is often a consequence of disease or injury of the stomach itself; it is sometimes one of the earliest declaratory symptoms of scirrhus or cancer of that organ, occurring long prior to ulceration. Hæmatemesis attends also, very commonly, the ultimate stages of that fatal disease; and then it may be owing to the erosion of some vessel of notable magnitude, in the course of the process of disorganization (as in the examples already adduced); or, which seems to be much more common, it may result from a kind of general oozing, or exhalation, from the ulcerating surface, similar to that which is believed to proceed from the surfaces of pulmonary excavations in some cases of tubercular phthisis.

Bleeding from the surface of the stomach, and consequent hæmatemesis, is also a common effect of the action of corrosive or irritant poisons upon that membrane; but hæmorrhage of this kind is seldom from any distinguishable blood-vessel. Sometimes, as when the concentrated mineral acids have been swallowed, the membranes of the stomach, and with them their corresponding vascular apparatus, are partially destroyed; sometimes the submucous system of blood-vessels is seen to be gorged with dark blood, and blood is found extravasated both upon and beneath the mucous membrane, which itself remains unbroken.

These are all instances of the first species of symptomatic hæmorrhage, where the disease, or injury, of which the effusion of blood is a symptom, is situated in the very part from which the blood proceeds.

Symptomatic hæmorrhage from the stomach, of the secondary species, is also extremely common—that, namely, in which the bleeding depends upon disease situated elsewhere—yet in a part capable, when in a morbid condition, of influencing the flow of blood in the capillary system belonging to the stomach; and the viscera, with the diseases or the morbid alterations of which hæmorrhage from the stomach is apt to be connected, are especially the liver and the spleen.

All this is well known: and it is easy to see, from the peculiar construction of the venous circulation in the abdomen, how disease of one or both of these viscera may conduce to mechanical hyperæmia, or congestion of the submucous capillary tissue, and how that congestion may be relieved, under certain circumstances, by the effusion of serous fluid on the one or the other surface—or, under other circumstances, not perhaps easily discriminated, by the extravasation of the collected blood itself. It would be superfluous to describe the peculiar distribution and functions of the vessels which return the main portion of the venous blood from the stomach and intestines towards the heart. It is more than probable that one, at least, of the offices of the spleen is to provide a receptacle or reservoir for this blood, when its free passage through the portal vessels is temporarily obstructed. It becomes a sort of safety-valve, if such an illustration be allowable, which obviates the danger that might otherwise arise to more vital parts, from any great or sudden disturbance of the venous circulation. But when the obstacle to the return of the blood increases, or becomes permanent, the stress of the congestion will be continually felt in the submucous capillary system, and the hæmorrhage which is apt in such cases to occur from the loaded membrane, receives a simple solution on principles almost purely mechanical;—nay, the very circumstances which lead to the effusion of blood from the *mucous* surface on the one side, rather than from the *serous* on the other, may perhaps, as was more fully stated in a former lecture, be themselves susceptible of a mechanical explanation.

We may even before-hand perceive in what sort of affection of the *liver* this combination of hæmorrhage from the stomach is chiefly to be expected. We look for it, and in fact it principally occurs, in those cases in which the vascular structure of the liver is effaced or obliterated by interstitial deposition, or by some general disease pervading the whole organ, rather than in those where the morbid change is partial—as where separate tumors are disseminated through its texture; and there seems to be an analogy, in this respect, between the production of hæmatemesis and of jaundice. It is well known that jaundice is not very apt to concur with distinct deposits of the liver: the tubera diffusa, for example—unless, indeed, the tumors by chance come to press upon, and obstruct, its excretory ducts; otherwise the viscus enlarges and accommodates itself, up to a certain point, to the encroachments of the foreign matter; and those portions of the liver that intervene between the tumors

remaining healthy, continue to perform their proper functions. It is when the liver is uniformly diseased in structure, and often when it is in a state of atrophy—when it is shrunk and crimped up, rather than swoln—that jaundice is most apt to ensue; and Dr. Watson believed that the same observations would be found to apply, more or less, to hæmorrhage from the stomach, when that is a symptom of hepatic disease.

The connexion between hæmatemesis and what must be considered a morbid condition of the *spleen*, is of a somewhat different kind, and is full, indeed, of pathological interest. The state of the spleen in these cases, unlike that of the liver, is uniformly a state of enlargement; and the augmentation of bulk is not so much to be ascribed to disease inherent in its proper texture, as to distention by the mere quantity of blood which it receives and retains. The internal structure of the spleen furnishes a credible presumption in favour of that view of one of its uses to which allusion has just been made; and this structure and this presumed function, when considered together, throw a strong light upon some of the pathological relations of the spleen, which well deserve a greater share of attention than has yet been given to them, but to which, the lecturer observed, he could only advert, and that very briefly, in so far as they were connected with the subject before him.

The substance of the spleen is principally made up of a vast number of cells, which are divided from each other by thin, membranous, imperfect septa, crossing each other in all directions. These partitions proceed from the fibrous membrane forming the proper external tunic of the spleen, and are composed of the same extensible and elastic tissue. The freest communication exists, by means of the incompleteness of their walls, between all these cells. They communicate also, in the most direct manner, with the splenic veins, by a mechanism which is quite peculiar to this organ of the body. The very first branches given off by the main venous trunk after it enters the spleen, have their sides perforated or riddled as it were, by a number of holes; and a fine probe introduced into any of these orifices, passes at once into the splenic cells. The further the veins are traced, the larger do these perforations become, until at length the coats of the vein cease to form a tube, but separate into filaments that pass into, and become continuous with, the fibrous partitions of the cells, where they terminate. The splenic artery, on the other hand, as soon as it penetrates the spleen, diminishes rapidly in size, and divides into very small branches, which have but little communication with each other, which cannot be traced far, and which appear to be distributed upon the membranous septa. The cells are habitually filled with blood. A few nerves and some lymphatic vessels complete the physical structure of the spleen, as it is described by modern anatomists *. No one can fail to perceive how admirably it is adapted for the ready reception of blood, and for containing very different measures of that fluid at different times. It is to the varying quantity of blood which it receives that its remarkable and often rapid variations of size are owing; and the alterations which the blood undergoes when long detained in the cells, or when subjected there to certain injurious influences, offer an intelligible and sufficient explanation of its softness or induration;—its varieties of colour;—in short, of most, if not of all—the morbid appearances which it presents.

Now there are instances without end of hæmatemesis going along with evident enlargement of the spleen; yet we are not therefore to conclude that the hæmorrhage was necessarily occasioned by actual *disease* of that organ. In some of them the spleen has been observed to diminish in bulk in proportion as blood was poured out by the stomach. Dr. Watson was greatly mistaken if he had not more than once seen this himself. In such cases, the tumid condition of the spleen may be regarded as an evidence of venous obstruction elsewhere; and it may sometimes be reasonably supposed to depend upon disease, of a less striking and prominent character, in the liver, obstructing the passage of the blood through the branches of the vena portæ. Of this kind would seem to have been the case related by Morgagni, where, after repeated attacks of hæmatemesis, under which the patient sunk, the spleen was found to weigh four pounds, and to be gorged with dark blood, whilst the liver and other viscera of the abdomen were pale and exsanguine. Frank gives the history of a patient who had vomitings of blood, and whose spleen, taken from the body after death, weighed sixteen pounds. The ordinary weight of the spleen in a healthy adult, is from eight to ten ounces.

In Latour's work on Hæmorrhage, which is remarkable for the number of examples it contains, collected from various sources, and amounting to nearly one thousand, several instances are detailed of this combination of splenic enlargement and hæmatemesis. One of these occurred in the person of a friend of his, who had been living in a malarious district, and who had laboured during the greater part of two years under obstinate ague. This was followed by an immense enlargement of the spleen, which came to occupy almost the whole of the abdomen. Latour's experience enabled him to predict that hematemesis would probably supervene on this condition of the spleen;

* Andral, Anat. Pathol. tom. ii. p. 416.

and, accordingly, one night he was hastily called to his patient, and found that he had vomited an enormous quantity of clotted blood; a good deal passed away by the bowels also. This recurred from time to time, till, in the course of a month, the spleen was so far reduced in bulk that it could no longer be felt in the abdomen, and the patient lived and enjoyed good health for twenty-five years afterwards.

It is necessary, then, that in marking the connexion which frequently subsists between hæmatemesis and enlargement of the spleen, we guard ourselves against concluding that these circumstances hold always the relation of cause and effect. In many such cases, and perhaps in most of them, they are merely concurrent effects of one common cause; and that cause is chiefly to be sought in such morbid conditions of the liver, or of other parts within the abdomen, as are competent to produce a considerable impediment to the free transmission of blood through the system of the vena portæ.

When a large quantity of blood is effused into the stomach, it appears to have a nauseating and emetic effect. At least, the blood ejected in hæmatemesis is almost always considerable in amount. The stomach seems tolerant of its presence up to a certain point, and no further. A small quantity may, doubtless, pass onwards through the pylorus, after undergoing, more or less completely, the process of digestion in the stomach; and a *portion* of the blood pursues that course in most cases. But when it is vomited it comes up in a large quantity, usually of a dark colour, and more or less coagulated. Sometimes the coagula have evidently been moulded in the stomach; and sometimes clots are thrown up, partially deprived of the colouring matter of the blood, and resembling the fibrinous polypi so often met with in the cavities of the heart. The degree of the coagulation of the blood, and of its separation into serum and crassamentum, will depend upon the time that it remains in the stomach; and this again would seem to bear a proportion to the rate of its effusion.

When the blood is ejected through the œsophagus and mouth, we have demonstrative evidence of the existence of *hæmorrhage;* and the diagnosis of *hæmatemesis* may appear to be so simple as to admit of neither mistake nor doubt. The diagnosis of *hæmorrhage from the stomach,* however, is really oftentimes difficult and obscure, and to be established by presumptive evidence alone.

In the first place, bleeding may take place from the mucous membrane of the stomach, and no hæmatemesis ensue, especially when the blood escapes from its proper vessels in small quantities, and very slowly. In these cases the blood becomes visible only in the stools, where it may not be looked for, and where, if seen, it may not always be recognized, in consequence of the changes which it has undergone during its passage through the intestinal canal. And even supposing that its presence is detected in the alvine evacuations, it will remain uncertain in what part of that long canal it was effused. The hæmorrhage may even be profuse, and the patient may die, without any escape of the blood externally. There is a case related by Franck, in which death took place from hæmorrhage of the stomach without hæmatemesis; and both the stomach and the intestines were found distended by an enormous coagulum of blood, which had assumed their form.

Even when the blood is ejected by the mouth, the exercise of some care and sagacity are occasionally, though not always, required, in order to determine the part from which it was originally poured out.

Thus blood may be swallowed, and afterwards vomited; and so we may have hæmatemesis without hæmorrhage from the stomach, just as we may have hæmorrhage from the stomach without hæmatemesis. There are cases of slow bleeding from the lungs, the fauces, the mouth, or the nasal cavities, where the blood, collecting at the aperture of the pharynx, provokes from time to time an instinctive action of deglutition; and thus is gradually accumulated in the stomach up to that point at which the organ becomes impatient of its contents, and ejects them by vomiting. This is very apt to happen during sleep, and especially to young children; and as the blood when vomited is coagulated, and in considerable quantity, it is scarcely possible to conclude, from its mere appearance, that it has proceeded from any other source than the stomach itself. We are assisted towards forming a right judgment in such cases (when our attention happens to be directed to this source of fallacy), partly by the general history and symptoms, and partly by an examination of the mouth, fauces, and nostrils, to ascertain whether any coagula, or other marks of hæmorrhage, are visible on the mucous membrane belonging to those parts.

Again, blood may be swallowed knowingly and purposely, by impostors, and afterwards vomited. Hæmatemesis is one of the complaints which have frequently been feigned, either for the sake of avoiding some imminent punishment or distasteful service; or with the view of exciting compassion, and of profiting by the contributions of the charitable and the credulous; or sometimes even from a kind of wilful perversity, akin to insanity. Sauvages has recorded in his Nosology, the case of a young girl, who, being desirous at all hazards to escape the constraints of a convent, pretended that she was suffering from violent hæmatemesis. In fact,

she did for several days in succession vomit large quantities of blood, in the presence of the physician who had been summoned to her assistance. It was afterwards discovered, that on each of those days she had swallowed blood which had been secretly conveyed to her from the neighbouring shambles.

But in cases where no fraud is attempted, nor any blood swallowed, it occasionally becomes a nice matter to determine the origin of the hæmorrhage, when blood is ejected in large quantities by the mouth—to decide, namely, whether the blood has come originally from the lungs, or from the stomach. In copious hæmoptysis, the blood issues from the mouth in gushes, as it does in hæmatemesis; and the reflux of the blood into the pharynx, the tickling sensation it produces in the fauces, or the violence of the cough (which we know, even when the expectoration is not of blood, frequently occasions retching); these causes, acting singly or together, produce sometimes a convulsive contraction of the muscles of the thorax, which looks like the act of vomiting—and they often, indeed, give rise to actual vomiting. On the other hand, in sudden and profuse hæmatemesis, the irritation caused by the blood as it passes over the commencement of the air tubes, is apt to provoke a paroxysm of choking cough.

In these cases, equivocal at first sight, we may generally guide ourselves to a correct decision, by a careful investigation of the symptoms that precede, accompany, and follow the hæmorrhage. *Vomiting* of blood is commonly preceded by a sensation of weight and uneasiness in the epigastrium, and by nausea. Hæmatemesis is also, more frequently than hæmoptysis, ushered in by pallor of the countenance, giddiness, dimness of vision, and an approach to syncope. These latter symptoms are not to be regarded as premonitory of the *hæmorrhage*, although they have been so considered by some; they are more probably a sign that it has already taken place; and yet they are preliminary of the *hæmatemesis*. On the other hand, *hæmoptysis* is wont to be announced by dyspnœa, cough, tickling in the throat, and a sensation as if of bubbling within the thorax. Most commonly too, before the expulsion of much blood from the lungs, some sputa are coughed up, composed more or less of that fluid. And to those persons who are in the habit of employing all their senses in exploring the symptoms of disease, the ear, applied mediately or immediately to the surface of the chest, will often collect unquestionable evidence, the particular details of which it formed no part of the lecturer's present business to consider.

The symptoms that usually *succeed* the hæmorrhage in either case, afford equally valuable assistance to our judgment in cases which might otherwise be doubtful. Generally, copious hæmoptysis goes on, in a succession of mouthfuls, for some time; whereas there is mostly only one access of full vomiting. At any rate, at the close of abundant pulmonary hæmorrhage, the patient manifestly *coughs up*, and expectorates smaller quantities of blood; while we usually may observe, that a few hours after hæmatemesis has occurred, slight griping pains come on in the abdomen, and a portion of blood is got rid of from the bowels.

The blood, moreover, which comes from the lungs, is commonly florid, and mixed with bubbles of air—*frothy*: that which proceeds from the stomach is most often dark-coloured, coagulated, or grumous, and mingled with the fragments of the food last taken, with mucus from the stomach, or with bile.

These distinctions, drawn from circumstances that accompany the actual expulsion of the blood, are manifestly open to fallacy, and require to be corrected by the observation of other circumstances, whenever, from causes already adverted to, the acts of vomiting and of expectoration go on, as it were, together.

Other questions, often of much importance in regard to the ultimate diagnosis, when the hæmorrhage is traceable with certainty to the stomach, are, whether it be idiopathic, if, indeed, it *ever* be so; whether it be supplemental of some other discharge; whether it depend on disease of the stomach itself, of one or more of the contiguous viscera, or of the system at large? Certainly a very great majority of cases of hæmorrhage from the stomach are symptomatic; and the nature and seat of the disease, of which the bleeding is a symptom, may in many instances be determined without much difficulty. That which depends upon *incipient* cancer of the stomach, while it is by no means of rare occurrence, is also more frequently than other forms of hæmorrhage from that organ, obscure. It must be obvious, that a little attention to the symptoms and past history of the patient, will generally suffice to elucidate the nature of the case, where hæmatemesis supervenes immediately upon the introduction of corrosive poisons, or within a certain interval after they have been swallowed; where it depends upon the bursting of a large aneurism; where it breaks forth among other symptoms of scurvy or purpura; where it is the result of an *advanced* stage of cancer of the stomach; where it accompanies organic disease of the liver, spleen, or heart; where it occurs as a symptom of yellow fever; where it takes the place of suppressed or imperfect menstruation; or where it is occasioned by the pressure of the gravid uterus; in all these cases there is, ordinarily, no room for mistaking the one disease for the other, or for regarding the hæmorrhage as idiopathic.

With regard to the *treatment* that should be adopted in cases of hæmorrhage from the stomach, Dr. Watson observed, it must be apparent, from what had just been said of the many different morbid conditions upon which it might depend, or with which it might be essentially associated, that this was a part of the subject into which the limits of these lectures would not allow him to enter to any satisfactory or useful purpose; and which, therefore, he was obliged, for the present at least, to refrain from discussing at all.

COW-POX SUSPENDED BY MEASLES.

To the Editor of the London Medical Gazette.

Sir,

The following case of measles appears to me worthy of being placed on record; first, on account of the singular character and duration of the initiatory symptoms, and, secondly, from the remarkable suspension of vaccination for the period of sixteen days, which took place during its progress. You will oblige me by giving it insertion in the pages of your useful journal.

I am, sir,
Your obedient servant,
George Gregory.

31, Weymouth-Street,
June 29, 1832.

Eliza Finch, four months old, residing at No. 4, East-Place, Chapel-Street, Pentonville, was vaccinated on Tuesday, May 15th, at the Small-pox Hospital, being then in perfect health. On Thursday, May 17th, the child was attacked with bilious vomiting and *drowsiness*. The mother was sensible that these symptoms were indicative of serious disease unconnected with the vaccination, and therefore placed the child under the care of Mr. Searle, of Cumming-Street, Pentonville; to whose kindness I am indebted for the following clear detail of the progress of the case.

On the 18th the child passed two stools, composed principally of blood. The head was hot. There was constant drowsiness and frequent attacks of sickness, but no tenderness of the abdomen. Calomel was given, at first alone, and afterwards combined with jalap, every three hours. On the 20th, one slight green slimy motion, but no relief of symptoms. On the morning of the 21st, a warm clyster of salt and gruel was administered, but without effect. In the afternoon the vomiting became very urgent, and, milder remedies having failed to alleviate the irritable state of the stomach, recourse was had to the hydrocyanic acid. One minim was mixed with an ounce of distilled water, and a teaspoonful directed to be taken every half hour. After three or four doses, the sickness subsided entirely. The head was now considered as the organ primarily affected, and as the bowels had never yielded, four leeches were applied to the temples, a clyster containing two drachms of Ol. Tereb. and the same quantity of Ol. Ricini, was thrown up, and two grains of calomel, with as many of scammony, given every third hour. On the following morning (Tuesday, the 22d), several offensive green slimy motions were passed. The head appeared relieved. There was no return of sickness. On the evening of the 23d the drowsiness recurred. The face being somewhat blanched by the application of leeches, a large blister was ordered to the nape of the neck. On the 24th there was manifest relief: the child took some notice of what was passing around it, but the stools maintained the same unhealthy character. Three grains of blue pill, with one scruple of sulphate of magnesia, were therefore directed to be taken every three hours. On Friday, the 25th May, being the *ninth* from the invasion of symytoms, measles made their appearance. The eruption was of a very bright colour, and the subsequent stages of the disease were as mild as the former had been severe. *Not the slightest affection of the chest was apparent at any time.* The eyes were but slightly inflamed, and that only at the very commencement of the attack.

When the child first came under the care of Mr. Searle—viz. on Friday, May 18th,—the vaccination had evidently taken effect. During the tumultuous week which followed, the vaccine papulæ maintained their station, but made no advance. On Monday, the 28th of May (the fourteenth of vaccination), when Mr. Searle's attendance on the child ceased, the vesicles were considered by him to be about as forward as is usual on the eighth day, and

he observed some traces of an areola, but the suspension of vaccine action must have continued for many days afterwards, for on Monday the 11th June, when the child (convalescent from the measles) was brought to the Small-pox Hospital to have the appearances on the arm registered, I found *two good desiccating vesicles, with a declining areola.* The appearances tallied precisely with those of the *twelfth* day in ordinary cases. It was the twenty-eighth day in this case; so that vaccination was here suspended for *sixteen* days, during which time the rubeolous germ was making its circuit of the constitution. I presume that this child imbibed the measles on the 17th, when it first sickened, and that the period of *incubation* was eight days.

LACERATION OF THE PERINEUM.

To the Editor of the London Medical Gazette.

SIR,

PERHAPS you will permit me to make a few remarks in reply to a criticism, by Mr. Fincham, on my paper, in your Gazette of the 9th instant.

Mr. Fincham, it seems, considers himself very familiar with the accident I described, and hazards a subdued sneer because I have not happened to hear of it in lectures, or met with it in books. I think it will be admitted, that if the occurrence had been distinctly and repeatedly noticed, it *would* have found a place in authors*; but as it has not, as far as I am aware, is not the conclusion warrantable that its nature has not been completely understood? Such has been, and still is, my view of the case, and I prefer it to the loose declarations of Mr. Fincham to the contrary.

But I have difficulty in conceiving that Mr. Fincham has rightly comprehended my explanation, or that he is acquainted at all with the kind of occurrence in question; for he makes the impression on the integuments of the scalp a *consequence* of their tumefaction, whereas I have stated that it preceded and caused that condition;—and further, he suggests that the lever might have been used, in utter forgetfulness of the fact that the closeness of the marginal contraction and the augmenting bulk of the imprisoned head, would have opposed an insuperable obstacle to the passage of any instrument that could be expected to prove serviceable. Had, therefore, an indication for immediate delivery arisen, I fear it could not have been fulfilled, in accordance with the principles of British midwifery, but by having recourse to perforation; assuredly, however, none but a very urgent indication ought to have been held sufficient, and not any until the effects of venesection, and perhaps of opium, had been tried.

With respect to the "*rather* more uncommon state" of parts intended to be so ironically adduced by Mr. Fincham, without advancing my own opinion as to the comparative frequency of the cases which I would distrust, or abiding by that of Mr. Fincham, which I would entirely repudiate, I will be content to refer it to the decision of all candid and well-informed practitioners.

I remain, sir,
Your very obedient servant,
SAMUEL MALINS, M.D.

Liverpool, June 30, 1832.

* The German accoucheurs, who relate in detail the causes of laceration of the perineum, are quite silent on this.

TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

Hull, June 29, 1832.

SIR,

IT may be deemed superfluous in me to offer any remarks on the treatment of cholera, especially after so many practitioners have written so elaborately on the subject; but I hope you will have the kindness to excuse my venturing to add to the already numerous lists of modes of treatment, on the consideration that the mode which I have followed has been uniformly successful, in all cases where no other medicine has been previously administered.

To enter into the symptoms, origin, and history of cholera at present, is foreign to my intention. I shall here merely remark, that from all that I can glean, either from essays, periodicals, (among which your valuable journal ranks the highest), as well as oral relation, aided by my own observations on

the several cases that have fallen under my care—that this disease arises from malaria, *sui generis;* combined with a certain state of the atmosphere which acts with fatal rapidity on the human frame—affecting particularly the large glands—when debilitated by passion, fatigue, recent disease, exposure to the atmosphere in a languid state with an empty stomach, but more especially when the person suffers from destitution.

From this view of the subject, I was led to consider the disease as somewhat similar to the worst cases of English spasmodic cholera, modified by climate, constitution, &c.; and that if I could combine or form any medicine, so as to determine powerfully to the surface of the body, and thereby promote profuse perspiration, the distressing symptoms would subside, or at least be greatly relieved. It is needless to tire you with an account of the various pharmaceutical experiments which I made, in order to form a mixture which would produce the effect desired. The following *recipe* is the result of my investigations:—

℞ Ol. Olivar. ℥j.: Tr. Acet. Opii gtt. xc.; Ol. Cinnam. gtt. xx.; Tr. Iodina gtt. x.; Spt. Vini Rect. ℥j.; Spt. Æth. Sulph. ʒj.; Tr. Acaroid. ʒij. M. ft. Mistura.

To this mixture I sometimes add from thirty to sixty drops of tr. assafœt. with the very best effect. On visiting a patient with symptoms of cholera, I give the one-half of the above mixture, and the other half in about an hour afterwards, unless previously ejected; then I administer a fresh dose. If the case be very violent, I give the whole of the above mixture, and order additional blankets to be spread over the sick person, with stone bottles filled with hot water to be applied to the feet. This generally produces warmth and cessation of pain in a few minutes, especially in the milder cases. The patients have always expressed their satisfaction in the restorative and invigorating qualities of the medicine. After one or two doses have been administered, the pulse rises, the extremities become warm, the face resumes its natural colour, and profuse perspiration takes place over the whole body. When the spasmodic symptoms have been very severe, there is generally great debility of the system, for which I have given the decoct. dulcam. with good effect, keeping the bowels open by means of the following mixture:—

℞ Pulv. Rhei. Pulv. Carb. Magnes. aa. ʒss. Spt. Annis. gtt. xxx. Aquæ Cinnam. ℥vij. M. ft. Mistura. Cap. Coch. ij. Mag. ter in die.

If the consecutive fever ran high, with strong pulse, I have taken from six to ten ounces of blood, and given a mixture of the Carbonate of Soda in Aqua Cinnam. But in general, the great and profuse perspiration relieves the whole system; and except debility, and sometimes dyspepsia, the rest of the treatment consists in relieving any symptom which may arise from confinement, &c.

The table in the opposite page is an abstract of the several cases which I have had under treatment since the beginning of May to the present date, 28th June, 1832.

Of the twenty-four cases there are only three deaths; the first of which is No. 3, who had been under the care of Mr. Hardy, surgeon. My assistant, Mr. M'Coll, attended her. A little before her death she got a dose of the medicine, by which she was much relieved, but in this case, as well as that of Nos. 8 and 9, which were treated conjointly with calomel and the mixture, they died. I have therefore given nothing since except the mixture, nor allowed the patient either fluids or solids, until the symptoms were relieved. It may, perhaps, be necessary to remark, in regard to the olive oil, that I took the idea from an account which I read of the plague at Alexandria and Grand Cairo, in which it was stated that dealers in olive oil escaped from the contagious influence of the plague; and that none of the persons who had their clothes smeared with olive oil were affected; and also that two negro slaves recovered from the plague by taking a quantity of the oil, which, if not carried off by stool or vomiting, always induces profuse perspiration.

In a very severe case I gave a (quart) clyster with the olive oil, Tr. Opii, &c. with the very best effects. The acetous tincture was made from the residue of the spirituous tincture of opium after pressure.

The formulæ of the Tr. Iodine is ʒj. to an ounce of Rect. Spirit. I have sometimes used the Tr. Zingiber in place of the Spt. Vini Rect.

The above are a few practical hints

relative to my practice in this disease; but I beg leave distinctly to state, that in submitting them to the public, I do not wish to interfere with the mode of treatment of any practitioner, but merely to state the mode which has been successful with myself; and also to satisfy several medical friends who, having heard of the success attending this method of treatment, wished me to publish the cases.

The insertion of the above in your valuable journal, will much oblige,

Sir, most respectfully,

Your constant reader,

Tho. Buchanan, C.M.

Month.	Day.	No.	Names.	Residence.	Age.	Doses taken.	Termination.
May	6	1	Mrs. Bowers	Drypool	22	Two	Cured
—	—	2	Mrs. Carter	Water Works .	22	do.	do.
—	—	3	Mrs. Hillary	Wincolmlee ...	42	do.	Death §
—	11	4	John Fenton	—— ...	32	Three	Cured
—	12	5	Martha Meggitson...	—— ...	22	Two	do.
—	—	6	John Adams	New Dock.. ...		Four	do.
—	17	7	Ann Philipson	Drypool.....		One	do.
—	19	8	Michael Fenton	Wilcolmlee ...	56	Seven †	Death
—	29	9	J. Fenton (2d time).	—— ...	39	Three ‡	do.
—	30	10	Mrs. Wright	Chapel Street .	27	Two	Cured
June	1	11	——'s servant girl	Myton Gate ...	20	do.	do.
—	4	12	Mr. Wright.....	Chapel Street .	32	Three	do.
—	—	13	Mr. Savage	Spencer Street.	45	One large	do.
—	—	14	Female*	Myton Gate ...	32	do.	do.
—	5	15	Mr. Wharton	—— ...		Two	do.
—	6	16	William England ...	Witham	53	Four	do.
—	7	17	Maria Brown	Church Side ...	23	Three	do.
—	8	18	Mr. Grey	Myton Gate ...		do.	do.
—	9	19	Peter Pine	George Yard...	17	Five	do.
—	—	20	Mrs. Richardson ...	—— ...		Two	do.
—	16	21	Mr. Cogan	Humber Street	67	Three	do.
—	—	22	Elizabeth Acrid	Blanket Row .	27	Five	Convales.
—	25	23	William Lee	Black F. Gate.	65	Two	Cured
—	26	24	Mrs. Towers	—— .	61	Four	Convales.

* Name unknown, Mrs. D.'s.
† Along with Calomel.
‡ Treated with Calomel.
§ Had been under care of another medical gentleman.

ON SALINE INJECTIONS IN CHOLERA.

By J. Adair Laurie, M.D.
Physician to the Glasgow Cholera Hospital; Professor of Surgery, Andersonian University.

To the Editor of the London Medical Gazette.

Sir,

As saline injection into the veins seems still a favourite remedy in cholera with some of your correspondents, perhaps the results of my experience on this subject may not be devoid of interest.

I know not who was the first to *propose* medicated injections into the veins, but, if I mistake not, Professor Delpech was the first to execute the proposal. He shewed me his gravitator when he was in Glasgow, in February last, and at that time he had thrown in camphor and water without success. It was my intention to have followed up the practice, but the want of a proper instrument and *procrastination* prevented my doing so. Soon after the first trials were made in Edinburgh, I commenced the practice in the Cholera Hospital under my charge. Following the directions of Dr. Latta, I injected from 70 to 150 ounces within a few hours, and *all* my patients, to the number of six, died. It is unnecessary for me to give you the details of these cases; in all of them temporary benefit was produced, but in several the fatal result seemed accelerated. I then tried, in one case, the addition of albumen to the saline solution; in another the serum of bullock's blood; in a third,

human serum; in a fourth, the transfusion of blood; in two cases, small quantities of whiskey; in other two, a few drops of laudanum; and still *all* died. I began to suspect that the quantities injected were by far too great, notwithstanding the flattering statements which were published, and I resolved to limit the amount to thirty ounces at a time, to throw it in very slowly, and to watch carefully the state of the head and the respiration. Under these precautions four have recovered; but so many have died in despite of all precautions, so evidently injured by the practice, that I have now almost entirely laid it aside, as not only useless but frequently hazardous. I have injected twenty-six cases—twenty-two of whom have died! Does any plan of treatment merit the name of a cure, under which nearly six out of seven perish?

I shall give you a few cases as examples. The four first are those which *recovered;* I dare not say were *cured.*

Case I.

Mary Thompson, æt. 40 (Case 336) *, June 9th, 5 A.M —Surface cold, voice weak, pulse imperceptible at wrist, in iliacs very feeble; respiration 26; cramps in legs; vomiting bilious. Seized yesterday morning with purging, followed by cramps. Generally enjoys good health, and bowels regular before present attack.

To be placed on steam-bed, and have saline (Stevens's) powders.

11 A.M.—Has had warm-bath, with effect of reviving pulse and increasing the temperature of body. Purging and vomiting now characteristic.

Hab. St. Op. gr. i. Cont. Pulver.

One, mid-day.—Pulse perceptible, but not countable, at wrist; temperature of surface pretty good—of mouth 87° F. Hands not clammy nor shrivelled. Pill not given.

Half-past 1.—Fourteen ounces of saline solution (as recommended by Dr. Latta) introduced slowly into vein at bend of arm—two ounces by syringe, twelve by Blundell's gravitator. Pulse much stronger, 120; temperature of mouth 90°. Inclines to sleep; on which account injection not carried farther. Says her sensations are not improved, but she lies quieter.

Twenty minutes to three.—Pulse nearly imperceptible; slight vomiting; no purging; tongue loaded, dry; no urine; temperature of mouth 88°; respiration 42; cheeks cold; forehead warm.

Rep. Injec.

Quarter past three.—Seventeen ounces, temp. 106°, introduced by gravitator. Respiration fell to 30; pulse became quite distinct, 116. Lies quiet, with eyelids half closed, apparently asleep; says feels relieved; temperature of mouth 90; tongue same.

This woman passed through a very severe form of the bilious secondary fever. During the 10th, 11th, and 12th, she had a severe struggle; on the 13th she made urine; and on the 20th was dismissed cured.

Remarks.—I think the injection was beneficial; at the same time I have seen equally severe cases recover. It seemed to act as a momentary stimulus, and did not in the least modify or diminish the secondary fever.

Case II.

James Andrews, æt. 22, June 10th, 3 P.M. —Surface cold; pulse gone at wrist; respiration abdominal, loud; pain in back, and severe cramps in legs; purging. Has had purging for three days, and vomiting and cramps for sixteen hours. Is in the habit of drinking spirits, but enjoys generally good health.

Hab. Stm. Opii, gr. j. et dein. descendat in Baln. tepid.

7 P.M.—Pulse became perceptible at wrist while in bath, and at 5 P.M. ℥xvij. saline solution introduced by gravitator, with effect of restoring pulse and putting him asleep, after ℥xij. had been thrown in.

7½ P.M.—Pulse becoming feeble, injection repeated to ℥vj.

10½ P.M.—Fell asleep immediately after last injection, and has continued so ever since. Pulse 116, distinct at wrist, but feeble. Hands still livid; tongue dry; sensations comfortable; no discharges.

This lad passed through a severe form of secondary fever, with threatened congestion in the head, for which he was twice bled, blistered, salivated, and had diuretics. On the 19th he was dismissed cured.

Remarks.—I have seen more hopeless cases recover without injection, and the violence of the secondary fever leads me to think that in this case also the injection acted merely as a temporary stimulus.

Case III.

John Durham, æt. 34.

10th June, 5 A.M.—Pulse gone at wrists; respirations natural; tongue furred, cold;

* These numbers refer to the case in the Hospital Journals, and are given here for future reference in those cases in which I am obliged to conceal the name.

countenance characteristic; cramps in legs and thighs.

Has been ill for four days, and very acutely so since last night; general health good.

Descend. in Baln. tepid. et dein sumat Op. gr. iss.

8 A.M.—Pulse rose in bath, and continued good for two hours. At present perspires profusely; skin colder; pulse feebler, 132; voice weaker; temperature of mouth 88°; hands blue.

Rept. Injectio.

10 A.M.—Thirty ounces saline solutions, temperature 108°, injected by vena saphena; pulse became fuller, 116; temperature of mouth, 92°; sensations easier; and he fell asleep.

11 A.M.—Profuse cold perspiration continues to pour from whole surface; pulse very rapid and feeble; temperature of mouth, 90°; one small stool, bloody.

Rept. Injectio.

10½ P.M.—Twenty ounces injected soon after last report, which raised the pulse, and seemed to check the cold perspirations; diarrhœa increased soon after, and the stools were passed in bed. Half a drachm laudanum given, since which no purging, and he has slept soundly; pulse 96, feeble; surface cold, not moist; voice good.

Quiescat interea.

REMARKS.—This man promised to recover rapidly; and on the 11th made urine freely, (with me the grand test of *certain* recovery) but on the 12th, phlebitis shewed itself in saphena of left leg, and ran a severe course. He is still in hospital, but free from danger. The injection certainly did good here; but as he passed his stools in bed after the second injection, (for which he had laudanum) and drank a greater quantity of spirits, particularly gin, than any patient that I ever attended, I think that in him also it was merely a temporary stimulus. I could not say that but for the injection this man must have died.

CASE IV.

Very similar to case 3, except that he was injected while in the warm-bath. His recovery for 48 hours was very rapid; inflammation of the vein supervened, for which he is now under treatment, but I think free from danger.

REMARKS.—Your readers would with justice pronounce me a bore were I to give you even a tithe of my unsuccessful cases. I shall therefore content myself with the following selections. Finding that the ordinary plan of injecting was, in my hands, so unsuccessful, I put my patients into the warm bath, and threw in the fluids while they lay in the water.

Case 4th, as already stated, is a successful example of this mode of injecting. The following was fatal:—

CASE V.

(Case 377) æt. 24.

June 18th, 2 P.M.—On admission, surface rather cold; countenance pale, and eyes deeply sunk; pulse at wrist very feeble; respiration deep; voice pretty good; thirst; pain at epigastrium; cramps; vomiting and purging of characteristic fluids; cheeks red.

Says has been in bad health for ten days back, but bowels regular till this morning, when purging came on with great severity, attended by cramps and vomiting. No treatment.

Capt. statim Opii Pulv. gr. iss.

While in bath of temperature 102°, at 3 P.M. 24 ounces saline mixture, containing muriate of soda, ʒij. to ℥xxxvj. water, and ʒss. carb. sodæ injected. Vein small, and fluid passed slowly, at first by syringe, afterwards by gravitation. Pulse improved slightly, but she became faint, and vomited while injection was going on. Continued pretty easy for an hour after injection, when respiration became excessively difficult; in paroxysms, starting up and throwing herself violently forwards. The sinapisms and other remedies had no effect, and at half-past seven P.M. she died in great agony.

Inspection 12 hours after Death.

Head.—Meningeal vessels somewhat turgid; brain, when cut into, presented numerous bloody points; no effusion into ventricles; substance rather soft.

Chest.—Recent pleuritic adhesions; veins full, but not gorged nor distended; lungs weighed ℥xx. in structure healthy; heart flaccid; being opened under water, was found to contain no air; a small fibrinous clot in right ventricle, intermixed with dark coagula; left ventricle contained considerable quantity of dark clotted blood.

Abdomen.—Liver pale, firm, having numerous fissures on both surfaces, and the left lobe much enlarged; gall-bladder full, not distended; ducts pervious; bile nearly healthy; spleen large, five inches long by two and a half broad, weight nine ounces; stomach of ordinary size, containing a few ounces of whitish fluid; mucous membrane throughout firm, admitting of traction at both orifices, in colour healthy, except very slight ecchymosis towards cardiac extremity; small intestines contained a few ounces of

whitish fluid; coats throughout healthy; the mucous of pale pink tinge, firm, entire, and admitting of slight traction; Peyer's and Brunner's glands numerous, prominent, and pale, the latter in great numbers towards colic extremity of ilium; large intestines, contents small in quantity resembling those of small, coats perfectly healthy; kidneys healthy, not gorged with blood, and containing in pelvis small quantity of cream-like fluid; urinary bladder empty, not contracted, its mucous surface vascular; uterine system throughout vascular and turgid; uterus containing very small quantity of bloody mucus; vagina livid; mucous surface hard and corrugated.

I quote the following case, as affording an example of the effects of large quantities injected at one time:—

Case VI.

Mrs. ——, æt. forty (Case 330.)

June 6th, 9 A.M.—Symptoms well marked; cramps excruciating*.

3 P.M.—At one, the injection of saline fluid commenced, and ℥lxx. were slowly thrown in by syringe, through gravitator, with effect of raising the pulse, improving the temperature and countenance, and allaying the restlessness.

11 P.M.—At 8 P.M., symptoms being aggravated, injection repeated to ℥lx. The restlessness was at first relieved, the pulse fluttered at wrist, and the temperature rose. Face becoming flushed, the injection was intermitted. In half an hour the restlessness became extreme; temperature of surface high; tongue dry; delirium and great mental anxiety.

12 midnight.—Died.

Inspection, 18 hours after death.—No lividity of face, nor of any part of body; features placid, not much shrunk; general appearance plump, that of recent good health. *Head:* Meningeal vessels distended with dark blood; substance of organ injected, soft; ventricles filled with limpid fluid. *Chest:* Venous system not distended; heart and large vessels did not contain more blood than ordinary; right ventricle contained a fibrinous clot, intermixed with dark coagula. Lungs crepitating; in structure normal; congested posteriorly; weight ℥xxxvi. *Abdomen:* Peritoneal coat of stomach and intestines presented a peculiar yellowish white appearance, as if coloured with white lead. Stomach nearly empty; ecchymosed toward pyloric orifice; contained a minute quantity of bile. Mucous membrane thin; softened towards cardiac extremity. Small and large intestines contained about a quart of a peculiarly white mucous matter, of the consistence of gruel. Mucous membrane throughout free from injection, and of the peculiar* yellowish white colour already alluded to; texture of membrane normal. Liver very pale, bloodless; gall bladder nearly empty; spleen small, exceedingly soft; kidneys flaccid; urinary bladder empty.

In another case, in which ℥ccxxx. were injected eleven hours after the commencement of the disease, and which terminated fatally in twenty-two hours, ℔j. or ℔iss. of blood flowed from the vein by which the fluid was thrown in, after death. The lungs weighed ℥xxii.; the heart and large vessels contained ℥iv. of blood, and the large venous trunks were *empty*. The mucous membrane of intestines was throughout blanched, but entire in structure. Liver bloodless; kidneys flaccid. This patient vomited most profusely during the injections.

It occurred to me that the thick, tarry state of the blood, might be the cause of the failure of the injections, the solution not being mixed with it, and I resolved to take blood from the one arm, while I injected by the other. The following case shews the result:—

Case VII.

——, (Case 370), æt. 55.

June 17th, 4 P.M.—On admission skin of moderate temperature; countenance characteristic; voice weak; respiration hurried; pulse 68, feeble; cramps in legs and arms; thirst. Says that health was good, and bowels regular, until last night at ten, when she was seized with vomiting and purging of watery fluid, and very soon cramps; no urine since attack.

Hab. st. Pulver. Op. gr. iss.

10 P.M.—Pulse barely perceptible; tongue and skin cold; face moist; discharges continue; decidedly collapsed; begins to get restless.

Descendat. in balneum tepid. et restante in balneo fiat injectio salina.

12 midnight.—When placed in bath, temp. 100°, pulse became distinct, with cessation of cramps; ℥xv. injected; pulse became full, and 88; face warm, and she slumbered. A vein in opposite arm was then opened, and ℥v. dark, rather thick blood abstracted. Pulse becoming feebler, injection repeated to ℥xv.; and as pulse improved, ℥v. blood drawn. Pulse again failing, injection carried to ℥ix. farther. Dur-

* I omit the details, this letter being already too long.

* I call this peculiar, because, although I have inspected very many cases *carefully*, I never before saw this appearance so strongly marked.

ing operation forehead was cool, and face warm; she was free from uneasiness, and inclined to sleep. On being placed in bed felt very weak; pulse was barely perceptible, and skin rather cold, but she was free from restlessness.

June 18th, mid-day.—Soon after last report pulse became imperceptible; but she was so restless, and head so evidently affected, that I did not venture to repeat the injection, and she died an hour ago.

The previous cases and dissections afford some idea of what has been done by injections in Glasgow, and the results have led me almost entirely to abandon the practice. I have done so with the utmost regret and reluctance. I was strongly attached to the remedy in theory; and when I say that I persevered, although my eight first cases died, I fear I expose myself to an accusation of obstinate rashness. Some of my medical friends were present at the injections, and I have been most ably and assiduously assisted in almost all of them by my zealous and talented friend, Dr. A. Buchanan; but he (and I believe all who have witnessed the operation here) is quite satisfied that saline injections will not cure cholera. My experience would lead me to draw the following conclusions:—

1st. As to the quantity injected. Is it safe to inject pounds at a time? In my practice, every case in which upwards of thirty ounces were thrown in at one time died—some of them with manifest congestion of the head and lungs. Case 377 died from violent spasms of the chest, and in Case 450 the pupils became suddenly enormously dilated. What, it may be asked, becomes of the fluids in those cases in which congestion does not follow? It runs off by the stomach and bowels. In Case 319, I injected 230 ounces without removing the tube from the veins. During the operation the patient vomited profusely. Her own words were, "As fast as you put in water by my veins, it runs *out* by my stomach." I am sure she was correct. In many other cases acute pain was complained of at the epigastrium. Now, if cholera consists of an affection of the mucous membrane of the stomach and bowels, by which the blood is drained of its fluid portions, what good can accrue from feeding the disease, and literally washing the capillaries with salt and water? If benefit be to result from injections, the pulse rises so quickly that I cannot help concluding that it acts as a plain stimulus; and if we throw in too much, we either kill our patient by producing congestion, or feed the disease by overloading the blood-vessels.

2dly. When do we know that enough has been thrown in? If I were to continue the practice, I would cease injecting whenever the pulse was steadily improved — whenever the patient fell asleep, whether the pulse were improved or not—whenever the respiration was much hurried, and whenever acute pain was felt in the abdomen. In the first instance, because injection had done all that it can do; and in the others, because it had begun to do harm.

3dly. What cases are most favourable for injection? It appears to me, that injection will only do good after the violence of the disease has expended itself, the greater part of the discharges having taken place, and before permanent sinking or re-action are established. If fluid is thrown into the circulation of a person whose system is at the moment labouring under the poison of cholera, the salt and water is drained off along with what remains of the serum of the blood, and the mucous membranes are injured by having an increased quantity of fluid forced through them. On the other hand, in my practice, if we were too late in employing the remedy, throw in what quantity we chose, the pulse never returned to the wrist; it became powerful—too strong—in the iliacs and carotids; and if we persisted, delirium and fearful irritation soon closed the scene. If re-action has begun, injection is useless—probably injurious.

If the above conclusions be correct, injection is dangerous, inasmuch as it is no easy matter to catch the proper period; and where it seems to succeed, it is at best doubtful, because a case which will *bear* this stimulus would probably recover without it.

4thly. What are the immediate effects of injections? By far the most common, in my experience, is drowsiness. The patient falls asleep, unless he vomit; the cramps are very frequently renewed; acute pain (sometimes excruciating) seizes the epigastric region, either during, or soon after the injections. In cases about to terminate favourably, the pulse rises, or returns, after a very few

ounces have been thrown in; the colour is partially restored to the cheeks; the temperature is improved, and the distressing sensations relieved. I have almost uniformly seen nearly the same transitory good effect produced by the warm-bath. Put a patient, in the stage which I have ventured to describe as favourable for injection, into the warm-bath, his pulse will become distinct; his cramps leave him, and his temperature rise. Inject such a case to ℥x. or ℥xx. ounces while in the bath, and *probably* he will be benefitted. If, on the other hand, the pulse do not rise in the bath, or the patient is too weak to bear it, he is beyond the reach of injection, or any other remedy: such, at least, is my experience. I am sorry to say that I have in no instance seen the *miraculous* effects described by some of your correspondents: almost all my patients were too drowsy to talk much. I reckon talkativeness, however coherent, a bad symptom; I have always seen it followed by stupor or delirium; it seemed the last flicker of the expiring flame; a proof, in truth, that the organ of thought was morbidly excited. One woman suddenly inquired for her children; a second was grateful, and confident of recovery; a third was inspired with the most gloomy notions of futurity: all these died delirious.

The instrument which I use is Blundell's gravitator. When the fluid does not pass by its own gravity, I insert the point of a syringe, filled with the solution, into the top of the tube, leaving the cup attached, and partly filled with water. I advert to this circumstance to remark, that if the circulation is so languid that the fluids cannot be introduced through the gravitator unassisted by the syringe, the case is nearly hopeless, and will probably be injured by forcible injection.

Such has been my experience of saline, and other injections into the veins. I write under great disappointment, because it is a remedy which I hailed with delight. Let me add, that no man in the profession will be more rejoiced to see the above conclusions refuted, and proved to be erroneous, than

Yours, very obediently,
JAMES LAWRIE.

CHOLERA AT WHITTLESEA — FURTHER EVIDENCE OF ITS CONTAGIOUSNESS,

WITH THE

Details of Two Cases cured by the Administration of a Saturated Solution, followed by the Liq. Opii Sedativus.

BY ROBERT VENABLES, M.D*.

To W. Maclean, Esq. &c. &c.

Whittlesea, near Peterborough,
28th June, 1832.

SIR,

I HAVE the honour to acquaint you that I arrived here on Saturday last, and immediately waited on Mr. Boultbee, the principal surgeon, and Mr. Bowker, the magistrate. I should have written to you, for the information of the Board, before this, but that I was anxious to obtain as much information as possible upon the nature and character of the epidemic now prevailing in the place, that I might submit a more perfect history for the consideration of the Board. I shall therefore consider the subject in the following order. 1. The origin or introduction of the epidemic into this town. 2. Its characters. 3. The degree of severity. 4. Mortality. 5. Therapeutics, &c.

Introduction or Origin.—Upon this question there seems to be little difficulty. A man, named Atkinson, a resident at Ramsay, about nine miles distant, and where the disease was prevailing, left that place on the 23d May last, to visit his daughter, residing and keeping a school at Whittlesea. When he had completed about half the distance, he was seized with spasms, cramps, and other violent symptoms. He preferred being carried to his daughter's at Whittlesea, to returning to Ramsey, and was accordingly brought to this town. Messrs. Boultbee and Smith, on visiting him, found him affected with all the symptoms, and declared it a case, of cholera. Next, two children, who had attended Atkinson's daughter's school, were seized, and died. It has been asserted that the children never went to school after Atkinson's arrival: I merely state this assertion, and leave it to those who are more credulous than I am to

* Communicated to the Central Board of Health, by whom we have been permitted to have it copied.

put the most charitable construction upon it. After these three cases the disease spread rapidly about Ingram's End and Briggate, where it first appeared. Briggate is seated on the banks of a stream, and some have concluded from hence that the disease attaches itself, or follows the course of the waters. There are no coaches pass through this town, and the greatest intercourse is by means of rivers and waters, or canals, &c. Atkinson going to Ingram's End* was entirely owing to its being the residence of his daughter. The disease has since spread through all parts of the town, respecting neither age, sex, person, or locality.

Whittlesea is situated in a flat or plain country, and surrounded by marshes or fens. The people are in many instances comfortable and cleanly in their dwellings; still these people have suffered equally with the filthy, the intemperate, and the dissolute.

Characters.—The disease resembles in its mode of attack and general symptoms, pretty much what I have observed in London. Thus, in a large proportion of instances, the disease is ushered in by a preliminary stage of diarrhœa, of from three to ten days' duration. In some few cases the attack has been sudden, the disease being fully developed without any premonitory interval. This, however, I believe, has been rare. Many have complained of a kind of twitching, or inward spasmodic convulsion of the stomach, which invariably, when experienced, forewarns the party of what is coming on; and many have asserted or declared that they anticipated an attack when they experienced this sensation, and their prediction is invariably verified.

Severity.—The purging soon assumes a limpid shreddy appearance—in fact, like rice-water, with shreds diffused through it; this often continues for several days before vomiting comes on. Sometimes the purging is muddy, or gruelly-looking, but never, after the two or three first days, presents any biliary or feculent appearance. Vomiting at length supervenes, and in many instances, although the purging is readily controllable, the vomiting is obstinate, resisting every means for its relief.

The patient next becomes cold and pulseless; the tongue presents a most remarkable feeling of cold, and truly feels "like the back of a frog." In the rapid cases the patient becomes blue, and dies in from seven to eighteen hours. Suppression of urine is frequent in the severe stages, and sometimes continues throughout the stage of re-action; mostly, however, the suppression is resolved in the re-action. In some, the typhoid character of the fever is very manifest; in others, death takes place more apparently from exhaustion than from any manifest degree of fever. The intellectual faculties are generally preserved all through; but the "vox cholerica" has been a very general symptom in this epidemic.

Mortality.—With respect to the mortality, compared perhaps with the number of cases it will not appear great. The great bulk of the mortality was in the beginning of the epidemic, and it has been inferred from this that the malignancy of the disease has greatly abated. This is a very fatal error, and the circumstance is easily explained.

In this place, as every where else, the inhabitants, instead of providing against the visitation, disputed the possibility of any such event; and even when the first case was announced, would neither admit the fact, or make any provision. The poor were consequently left without any instructions, either as to the preliminary stage, its symptoms, or the necessity of applying for medical advice on their first appearance; consequently the medical gentlemen were not called upon till the period in which remedies could be successful had passed, and they therefore did not see the patient till fatal collapse had succeeded. I need hardly observe, that the mortality in such circumstances became frightful. Alarm succeeded to apathy; activity and exertion superseded the lethargy which paralysed the inhabitants. A Board of Health was formed, and instructions printed and circulated, directing the poor, on the first appearance of looseness of the bowels, to apply for medical aid.

The beneficial effect of these measures soon became apparent; Messrs. Boultbee and Smith, whose exertions cannot be too highly commended, and their services, were in requisition both night and day. Some idea may be formed of the anxiety of the poor to ward off the severity of the disease, when I state, that the

* There is no water at Ingram's End.

applications to Messrs Boultbee and Smith were so numerous, and the demand for remedies so great, that it occupied their two assistants, with Mr. Wilton, a chemist, who kindly volunteered his services, from eight o'clock in the morning till twelve at night, working as hard as ever they could, to dispense the prescriptions. This was during the warm days of last week, and had not the cold of Friday given a temporary check to the disease, and some respite to their labours, it is questionable if they could have continued their exertions.

The malignancy of the disease now appeared reduced; but, perhaps, the real fact is, that application was made at the beginning when the disease was manageable, and its progress easily controlled by remedial measures. The hot weather certainly seems to increase the disease, as during Friday, Saturday, and Sunday, the applications were comparatively few; but yesterday and to-day they have been more numerous.

Therapeutics.—With respect to the therapeutics, they are simple enough in the preliminary stage. Astringents, with opium, check the diarrhœa, and with care the patient generally goes on well. When vomiting accompanies the purging, the disease is more obstinate. The cretaceous mixture, with opium, so serviceable in the diarrhœa, frequently checks this symptom, while the vomiting without purging prevails, and the patient becomes gradually exhausted; when the purging at length reappears, and the patient sinks into collapse, which soon terminates in death. When vomiting supervenes, the face alters, the eyes sink, the countenance becomes haggard, and the patient assumes an appearance of age far beyond his real years.

When the vomiting continues obstinate, after the cessation of the purging, I have found a combination of gum, kino, and opium, as used in pyronosis, useful; inasmuch as, although not always decidedly successful, it relieves the urgency of the vomiting, and sometimes wholly stops it, enabling the stomach to retain other medicines. In full habits, and in phlogistic constitutions, bleeding is serviceable; and a combination of mercurials with opium, particularly Dover's powder, has been found very useful in the preliminary diarrhœa, particularly if the evacuations consist of a frothy-looking mucus, like yeast; of which I have seen two or three examples.

The fully-formed collapse, or even the advanced approaches to it, seems as unmanageable here as it has proved in most other places. I have, however, witnessed one recovery and one convalescence under these circumstances.

On Sunday last I was requested to visit a woman of the name of Good. The countenance pale and haggard, the eyes sunk, the extremities cold, the breathing slow and labouring, the fingers livid (especially under the nails), the tongue cold, and a sensible coldness from the breath. She was pulseless; even the carotids could not be felt. She had suffered from vomiting and purging for several days previously. A saturated solution of salt, warm, was given, and vomiting quickly succeeded. Forty drops of the liq. opii sed. were then given as a draught. This was about two o'clock P.M. Between five and six, on visiting, she was stated to be delirious, getting out of bed, and being guilty of several extravagances. I believed the previous dose insufficient, and I repeated it. She quickly fell asleep, and awoke in the morning convalescent. She is now perfectly well.

On Tuesday morning, a woman, Elizabeth Green, an attendant upon a man named Green, who died of cholera, and also upon his widow, a convalescent from cholera, though in bed, was seized with violent symptoms. Vomiting and purging of rice-water evacuations; she was cold, livid, and pulseless; she was so weak that she lay down in bed with the convalescent, Green; she was unable to go home till her husband procured a conveyance for her. Her countenance had altered so much in two hours, that I did not recognise her on seeing her at her own house. I directed her to take a saturated solution of salt, warm. She vomited almost immediately, and then took a drachm of liq. opii sed. In the afternoon she was in a profuse perspiration. She now took half a drachm more, and slept soundly. A little diarrhœa remaining, she took the chalk mixture; and this morning I found her eating an egg for her breakfast*.

The appearances of the body after

* I directed the same treatment in two other cases, but the patients would not submit to it: they both died.

death are similar to those described by me in the Cholera Gazette, and therefore need not be repeated here. I have not been able as yet to institute a "post-mortem examination."

I have the honour to be, &c.

(Signed) R. Venables, M.D.

DR. CHRISTISON ON THE NEW TREATMENT OF CHOLERA.

[We have been favoured with a copy of the following paper, drawn up by Dr. Christison for the guidance of the Dutch Government.]

Edinburgh, June 9, 1832.

Sir,

I have been requested by Mr. Aitchison this morning, to reply to a letter of yours, dated 6th instant, in which you beg him to supply, for the use of the Dutch Government, some information relative to a new mode of treatment which has lately been practised here for cholera; I do so with much pleasure, but regret that, in some respects, the information I have to communicate is necessarily defective.

I must begin by observing, that we have had an opportunity of trying all the previously recommended remedies, and that I am convinced they are all of very little use in genuine virulent cases of the epidemic cholera. In proof of this I may mention, that in one of the districts under the jurisdiction of our Board, a suburb village called the Water of Leith, from which I have now collected very full and accurate returns, and where every possible means were brought to bear on the disease, both in the way of treatment and for arresting its progress, it appears that all the customary remedies were resorted to, under the immediate directions of experienced surgeons, and with the advice of the most eminent members of the profession in the city; yet it turns out that scarcely any cures were accomplished amongst decidedly violent cases. Eighty-five cases of cholera, in all, occurred in the village, and thirty-eight were cured. Of these cases there were seventeen mild cases, nineteen severe cases, and only two violent cases. Undoubtedly several, probably many, of the cases, simply severe or mild, might have become violent, without attention, and the treatment being actually enforced. What I mean to urge merely is, that *when cases had once become violent*, the ordinary kinds of treatment were very rarely of much use. I have had an opportunity of personally witnessing the same fact throughout the whole course of our epidemic in the city; and I may perhaps be allowed to say that I am an impartial judge, because, as the department I was entrusted with—the collecting and keeping the medical returns, took up my whole time, I did not undertake the personal charge of the treatment of any cases; but, nevertheless, had extensive opportunities of seeing what was practised by my brethren in all quarters.

This much being premised, you will be better prepared to appreciate the probable value of the new treatment, and will not be surprised to find that, after all, the proportion of cures is not likely to be very much increased.

The last authorised account I have had an opportunity of hearing, is to the following effect:—In Leith nineteen had been treated, and five of these were considered in a fair way. In Edinburgh eighteen had been treated, and seven were either well or considered safe; several more were alive, but in a very doubtful state; and in every instance, without exception, where the treatment failed, there was found after death such extensive old organic disease of the visceral organs, especially of the liver and kidneys, as would in all probability have rendered a similar event inevitable in the instance of any other severe disease; such, for example, as fever. I cannot therefore but infer, that the ultimate result of the cases is such as to hold out the strongest encouragement to a further trial, although I am far from thinking that the utility of this remedy is fairly established by our experience. When it is considered that the trial of it was made in very bad cases only—in such cases, indeed, that, according to our previous experience, not more than two or three of the whole thirty-seven would have come round *under any other treatment*—that, in fatal cases, extensive disease of old standing was found invariably; and, in fact, that a very large proportion of our whole cases in the city have occurred in persons of dissipated habits, or broken-

down constitutions, the numerical results will certainly appear worthy of notice.

As to the immediate effect of the treatment, no question whatever can exist. No other remedy has any thing like the *immediate* effect of the injection of the saline solution into the veins. In a very large proportion of cases of a kind like those in which it was used, the other modes of treatment have entirely failed to restore the pulse and check the collapse; and in most of the cases where some re-action was brought about, it was imperfect and transient. The new treatment, on the other hand, has (with, I believe, no exception) been followed at once by restoration of the pulse and subsidence of collapse. Every medical man who has seen its effects in this respect, agrees in being astonished at its immediate results. An individual who lies pulseless, almost speechless, deadly cold and shrivelled, will, in thirty or forty minutes, present a good pulse, a general warm respiration, a full florid cheek, and an open lively eye. Nay, what is very generally remarked, when the body is moderately relieved from severe suffering of any kind, he begins to be talkative, and cracks his jokes with his attendants. This I have myself witnessed, and there is scarcely any medical man of note in the City who has not had occasion also to observe it. Several individuals who have been brought thus far round, have either died of the subsequent stages of reaction or have fallen again into the state of collapse, and, after repeatedly doing so, have at length died in that stage. At present, then, you will remark, I speak of its *immediate* effects, which are undoubted and most striking.

The dangers to be anticipated from this mode of treatment, are, so far as I see, three. Air may be introduced into the veins with the injected matter. This is a material difficulty, which must be guarded against by careful attention on the part of the operator. 2dly. The vein, roughly handled by the introduction of the tube, and its maintenance in that position for some considerable time, may inflame. This may be justly considered a formidable risk; in point of fact, the vein has inflamed more or less in several cases—I do not remember exactly in how many—but in none has the inflammation been fatal, or very serious.

I can scarcely doubt, however, if the operation were practised in many instances, and a considerable proportion of individuals to live long enough for the inflammation to run its usual course, we should find some deaths imputable to this cause—probably not a material number.

3d, The introduction of so much saline matter into the blood, although *the salts are the salts of the blood*, may be eventually followed by some constitutional injury, which cannot at present be anticipated. This is a conjectural objection, reasonable certainly, yet not borne out by observations hitherto made. The principle of the treatment is, that the blood is defective *in water*, and in its salts; and that, by supplying the deficiency, we keep up the circulation; and this may enable the system to throw off the disease. My own experiments, which have been extensive, agree with those of all former experimentalists, in shewing a great deficiency of water, and likewise with those of some who have found the salts too defective. In fact, in consequence of my analysis, I recommended one of our hospital physicians, Dr. Davidson, three months ago, to try the very remedy which Dr. Latta has actually employed, and should certainly have given it a trial, had I been at the time in charge of cholera patients.

The mixture used is different in Leith and Edinburgh. In Leith the quantity of saline matter employed has not been so large as in Edinburgh, where the mixture in use consists of 120 grains of common salt, and 40 of carbonate of soda, dissolved in five pounds of water; of this from five to six pounds are injected into a vein in the arm, in the course of thirty minutes; and ten pounds are frequently injected at this rate without stopping. The temperature is about 110°, or as high as 115° Fahrenheit, when rigor is apt to be induced. Rigor, indeed, is apt to follow in all circumstances, but is prolonged when the fluid is much under 110°. The injection is sometimes repeated twice, or oftener, in one day; in two days forty pounds have been injected.

The instrument used is Read's patent syringe, with a fine tube for the vein fitted to it. Severe vomiting often follows, against which the best preventive hitherto appears to be ten or fifteen drops (in each injection of seven or ten pounds) of a solution of muriate of morphia (one part of muriate to twenty-

five of water.) In the saline mixture some have occasionally added a little white of egg (albumen), on the supposition that albumen is defective in the blood. But it has not been found useful, and in point of fact, if I may trust my own experiments, the albumen of the blood is not defective at all, or at least immaterially. A fair trial has been made of warm water without the salts, but the immediate effect was obviously less marked and much less lasting. Along with the saline injection, means are used for warming the body by tin mattresses, into which steam is conveyed. No other treatment of material consequence is combined, except merely to palliate symptoms as they occur. Blood-letting is not practised; opium, if given at all, administered only in small doses, to check vomiting or purging. The spasms cease immediately the injection is begun. Several cases, by the way, have been recovered when the collapse was previously so deep that the spasms had ceased; a state always considered very alarming.

Such are the leading facts I have to communicate. Should you require further information, I beg you will address me direct for the purpose. I certainly think you may, with great propriety, recommend the Dutch Government to have the method of treatment tried.

I have the honour to be,
Your most obedient servant,
(Signed) R. Christison.

RESULTS OF THE INJECTIONS INTO THE VEINS,

In the Cholera Hospital, Drummond-Street, Edinburgh.

Extract of a Letter from a distinguished Physician of Edinburgh, dated June 14, 1832.

As to the injections, the result hitherto, in Drummond-Street, is six recoveries out of nineteen injected cases; and the last injection in any of these was more than a week ago. Of the six, one died subsequently of erysipelas, but after having been more than a fortnight free from any symptom of cholera. Of the nineteen cases, I believe the whole were strictly malignant, and if there had been three recoveries among them without the injections, probably that would have been all. Six or seven other cases have been admitted during the same time into the Drummond-Street Hospital, with milder symptoms, and got well without injections; which shews that the practice was reserved for the really bad cases. Of those who died, several (perhaps nearly half) sunk back into collapse, and died in it; but the majority got fairly into the third stage, and died comatose—several of them nearly or quite a week after the last injection; and the ischuria renalis was well marked, at least in two or three of these. The advantages of the injection are clearly confined to the second stage of the disease. There was only one death which could be supposed to depend on inflamed vein;—in that case I examined the vein carefully, and the appearance was slight. The inflammation did not extend two inches, and there was neither lymph nor pus effused. We have now very few cases, but no diminution of the malignity. I went two nights ago, with some of the young men, to try the injection in a case of complete collapse, but before we could get the injection begun, the woman died, though previously healthy and young, and only twelve hours ill.

CASE OF CHOLERA, TREATED BY SALINE INJECTIONS,

The only one out of five which survived the Operation for any length of time—Fatal at the end of eight days—Phlebitis.

To the Editor of the London Medical Gazette.

Sir,

Of five cases of saline injection, the enclosed is the only one that survived for any length of time, the history of which you can insert in the Gazette, if you think it worthy of a place.—I am, sir,

Yours, &c.
Charles C. Aldred.

H. M. S. Dover, July 5th, 1832.

June 26.—Robert Henderson, æt. 38.—Has had diarrhœa for four days; at 3 p.m. this day was attacked with severe vomiting.

7 p.m. time of admission.—Pulse scarcely perceptible; body cold, covered with a clammy perspiration; tongue cold; face livid; eyes appeared sunken.

Friction c. Lin. Sp. Tereb. et Liq. Ammon. Fort. Brandy and water. Cal. gr. x. Ext. Hyos. gr. v. stat.

Half an hour after admission his pulse was quite gone; in other respects the same.

Sodæ Mur. ʒiij. Sodæ Carb. ʒj. Aquæ, ℔iij. M.

Injected two pints of the mixture at 110° F., when his pulse became 90, rather full; body warm; and he appeared much better.

Cal. gr. v. Hyos. gr. v. f. Pil. ij. om. hor. sum. P. Friction. Brandy and water.

10 P.M.—Pulse has been flagging for the last half-hour; he is now nearly relapsed into his former condition; bowels open twice since admission; occasional vomiting.

I injected one pint and three-quarters with the same good effect as the former. Pulse 100, small and soft.

P. Cal. et Hyos. Friction. Brandy. Enema Sp. Camph. ʒiij. Aquæ, ℔ss.

12½ Midnight.—Had relapsed into the cold stage, when the injection was repeated to two pints, with the same good effect.

P. Cal. et Hyos. Brandy. Friction et Enema.

27th, 6 A.M.—Had again relapsed. The injection was repeated to two pints and a quarter, with the same good effect. Pulse 100, small and soft.

P. Cal. et Hyos. Brandy. Friction and Enema.

10 P.M.—Continued throughout the day in the same state; stools still of a choleric appearance.

P. Cal. et Hyos. Ung. Hydrarg. Camph. inter femora.

28th.—Appears in every respect better.

Omit. Cal. et Hyos.

29th.—Appears better; has made a little urine; some superficial inflammation around the orifice in the right arm; complains of pain, on pressure being made on the abdomen; pulse 90, rather full.

Catap. micæ panis brach. Hirud. xij. abdomini.

30th.—General health continues improving; inflammation of the arm appears extending.

Hirud. xij. brach. P. Cataplasm.

July 1st.—The inflammation much increased; extending from the orifice to the axillæ, with some hardness and tenderness of the upper arm; pulse full and hard; skin hot; is very restless; bowels not opened.

Hirud. xxv. brach. Cal. gr. iij. Ext. Colocynth. co. gr. v. Hst. Sennæ.

July 2d.—Appears much worse; features collapsed; is very restless; complains of great general uneasiness; superficial inflammation of the arm subsided; bowels opened once.

3d.—Passed a very restless night; some hours before death complained of pain in the region of the heart. Died at 8 A.M.

Autopsy eight hours after death.—Head not examined. Thoracic and abdominal viscera presented no unhealthy appearance whatever. The blood contained in the heart and large vessels perfectly natural. On tracing the cephalic vein in the right arm, it was found in a state of intense inflammation as far as the junction with the subclavian; the cellular tissue in immediate contact with it was filled with a deposition of fibrine, while the rest was infiltrated with serous fluid. The inflammatory condition of the veins extended about an inch below the orifice made for injecting in the median cephalic vein. The vein in the opposite arm was also inflamed for about three inches upwards from the orifice.

TIGHT BANDAGING IN CHOLERA.

To the Editor of the London Medical Gazette.

4th July, 1832.

Sir,

It is probably better to communicate to the profession at large, any hint, however crude, that promises to be useful in the treatment of the present intractable epidemic, than to wait till time and experience have set their seal to it.

For this reason I hasten to inform you that in *one* case of spasmodic cholera, the only one in which I have yet tried it, I have found a compress, and *tight* bandage to the whole abdominal surface, effectually restrain the *serous hæmorrhage* from the bowels; and as a further and more remote consequence, but an important one, perhaps, restore the secretion of urine. The spasms in the extremities had ceased, or it was my intention to have bandaged them also.

The other parts of the treatment were calomel and opium, Dr. Stevens' saline powders, blistering the epigastrium, and spirit lotion to the head.

With many apologies for this haste,

I am, Sir,

Your very obedient servant,

Edm. Bowden,

Surgeon.

135, Sloane-Street.

ACCOUNT OF CHOLERA IN COLD-BATH-FIELDS PRISON.

By Dr. Stevens.

"In the first irruption of cholera which occurred in the prison of Cold-Bath Fields, the disease was confined entirely to the males. It commenced in the beginning of April, and the last case was dismissed cured on the 30th of the same month. From this period up to the 3d of June, there were no new cases; but on that day it broke out a second time. In this instance it commenced amongst the females, and soon spread almost all over the whole establishment, and is now at this moment much more virulent, and I am sorry to add, more fatal, than it has ever been at any former period. In the first case that occurred the woman was attacked on the night of the 3d, and died on the 5th. Her sister, who attended her, was next taken ill, but recovered under the saline treatment.

"Soon after the commencement of this second irruption, I called at the prison, and there were then four cases. These were under the saline treatment, and as they were all doing well, I did not return. On the 21st of June, however, I received a note from Mr. Wakefield, requesting me to meet him at the prison as soon as possible. When I went there, I found about twenty patients with cholera, and out of this number five were actually dying. There was one obvious cause for this, which I do not feel myself at liberty to point out; suffice it to say, that it originated from either a mistake or neglect on the part of the nurses who administered the medicines.

"A saline fluid, similar to that which had been used at Leith, was injected in two cases into the veins; but the one died almost immediately, and the other, though he rallied for a time, yet he also ultimately died*. . . . .

"When the cholera was first raging in the prison at Cold-Bath Fields, the disease broke out about the same time amongst a colony of itinerant Italians who resided in the neighbourhood. The first cases were put under the care of a physician, who had charge of a cholera hospital in that part of London. These patients were first bled, and then most scientifically treated with opium and brandy, but the result was such that the other Italians, who were taken ill about the same time, refused to be treated by the cholera physician; and, fortunately for themselves, sent for Mr. Whitmore, an intelligent practitioner, who lives in that neighbourhood. This gentleman had seen the effects of the saline treatment in the prison, and afterwards trusted entirely to this, in every decided case of cholera which he attended. Since then he has had in his own private practice about thirty cholera patients, chiefly amongst the Italians, and out of this number he has lost only two cases, and saved about twenty-eight; and this surely is no common occurrence, particularly amongst the poorer classes, where we do not always see them early, and where we are not certain either that the medicines which we prescribe are properly administered, or that they are not occasionally used at the same time with other improper agents which counteract the beneficial effects of the alkaline salts. . . . .

"On the 29th, a woman, who was apparently perfectly well when she was locked up, about eleven o'clock, was attacked during the night; but as no communication of her illness had been made to the governor, she was found, about seven o'clock in the morning, in a state of complete collapse. Mr. Crooke then ordered some saline powders, and soon afterwards went for the injecting apparatus; but before he returned, the woman was dead. On the same day, one of the men in the cholera infirmary was so far gone in a state of collapse, that it was thought necessary to inject a saline fluid into the veins; but Mr. Crooke, in this case, could not succeed. He gave him, however, at one dose, about an ounce of muriate of soda, with ten grains of the chlorate of potass. Re-action almost immediately followed; and though this was also a case of relapse, yet he is now (sixteen hours afterwards) apparently nearly out of danger.

"We have now had, during the present irruption in the prison at Cold-Bath Fields, one hundred and five cases, and fifteen deaths; and there are at this

* "Should I have occasion to try this experiment again, I shall certainly add a portion of the chlorate of potass to the other ingredients. The muriate of soda answers very well for a time, but it is too apt to run off by the bowels; and then the blood is swamped by the large quantity of water."

moment in that establishment twenty-two patients who have recently recovered from a state of complete collapse. But though the proofs in favour of the saline treatment were as numerous as the sands on the sea-shore, still there are individuals who will deny its utility,—and that too in a manner which can neither do credit to themselves, nor good to humanity."

ANALYSES & NOTICES OF BOOKS.

" L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Dublin Journal of Medical and Chemical Science. Nos. II. and III.

THE third number of this clever periodical has just come out, containing an ingenious paper by Dr. Graves on Double and Single Vision, one or two chemical articles by the editor, and, among other things worthy of special mention, a very able review of Lyell's Geology: we must, however, reserve our more particular remarks on the number until next week, while we bring up our arrear with a notice of its predecessor. We know not how it has happened, but, owing to one circumstance or another, we have been obliged to defer our purpose of doing so earlier, until even now—the eleventh hour. Without farther preface or delay, then, we will take up

Mr. Campbell's Cases of Aneurism.

The three cases of aneurism, of which Mr. C. gives a very good account, were cases occurring after venesection (or what was intended for such in the management of awkward people): they were all, in the first instance at least, circumscribed; and two of them treated with much skill and success after Genga's method. In the third case, the false aneurism becoming diffused, an operation for tying the artery was required. We extract the principal part of Case I.:—

" John Miley, ætatis 27, was admitted with a circumscribed false aneurism of the brachial artery in the bend of the arm, produced by puncture in *venesection (?) He stated, that thirteen days previously*, in order to procure relief from a cough which he laboured under, *he permitted a blacksmith to abstract blood from his arm.* During the operation he was struck with the florid red colour of the blood, and the distance to which it was propelled; also, the difficulty experienced in suppressing the hæmorrhage: this was at length accomplished by means of a compress and bandage very tightly applied. On the following day the arm felt stiff, and he had the bandage loosed, but continued to wear it for several days. On its removal he observed the " beating tumor" in the arm, became alarmed, and applied to the hospital for relief. On inspection, a firm pulsatory tumor, about the size and shape of a pigeon's egg, was detected in the bend of the arm, along the course of the brachial artery, with which vessel it appeared intimately connected. It became smaller when compressed, but resumed its original size when the pressure was removed. The radial pulse on that side did not beat so fully as on the sound side. The integuments were perfectly healthy, and presented a small cicatrix, such as might be expected after venesection. The only vein which could be detected in the neighbourhood of the tumor, was the median basilic; it ran in front nearly an inch internal to the cicatrix, and obviously had not been opened in the operation. * * * The patient was immediately confined to bed, the limb placed upon a pillow, some cathartic mixture prescribed, and sixteen ounces of blood abstracted from the opposite arm.

" The next day a thin compress of wetted lint was laid upon the aneurism, and a roller bandage applied from the fingers to the bend of the arm, in the manner recommended by Genga, care being taken that the compression should not extend above the aneurism. The turns of the bandage over the aneurism were very loosely applied. The patient was desired to keep the compress constantly moistened with cold water, and to take a draught containing two drops of tincture of digitalis every sixth hour.

" Two days subsequently the bandage was opened and reapplied, the draught was continued, and he had sixteen ounces of blood taken from the opposite limb.

" On the 7th day the aneurism felt more solid, and did not diminish so sensibly when compressed. The bandage was re-applied, and a compress of sponge substituted for the lint; the digitalis continued, and other treatment as before.

" * * * On the 30th day there was no trace of the aneurism; the brachial artery could be felt pulsating strongly beneath the cicatrix in the integument, and the radial pulse appeared as full as that of the opposite limb.

" On examining the patient eighteen months subsequent to his dismissal from the hospital, no trace of the aneurism could be

detected; the artery pulsated beneath the cicatrix strongly, the pulse at the wrist felt perfectly natural, and he stated that he had not suffered the slightest inconvenience from the arm, in pursuing his very laborious occupation."

The second case is very similar: the tumor attained the size of a turkey's egg before the patient applied for relief; but the bandaging, as just described, completely removed it in between forty and fifty days.

Dr. Stokes's Observations on the Efficacy of Opium in subduing Inflammatory Action.

Opium has been recommended by various writers, Hamilton, Armstrong, and Gooch, among the number, in certain well-known circumstances; but Dr. Stokes gives the most flattering account of the great utility of this remedy that has yet appeared. The first form of disease in which he has found it to be peculiarly advantageous, is that of *peritonitis, in which bloodletting is counter-indicated;* as, for example, where it has arisen, 1st, from the escape of fecal matter into the peritoneal cavity, through a perforating ulcer of the intestine; 2d, from the bursting of an abscess into the serous cavity; and 3d, from paracentesis performed on a debilitated subject. To which he adds the low typhoid peritonitis occurring after delivery, and the peritonitis which results from rupture of the intestine induced by external violence.

Dr. Stokes gives three cases from the Dublin Hospital Reports, Vol. V., to which we think it only necessary to refer; but we extract the following as a sufficiently brief and original illustration:—

"The next case is one to which I confess I look back with great pleasure. It is that of a patient who was admitted in the beginning of February last into the Meath Hospital, complaining of sore throat and pain shooting through both ears; his countenance was haggard, his voice raucous, and the body emaciated. An extensive and unhealthy-looking ulcer, covered with a whitish matter, was found to occupy the left tonsil, the back of the pharynx, and left side of the uvula. The patient denied having had venereal, but circumstances led us to suspect this; he had, however, been frequently salivated in India for abdominal disease and fevers. He first felt the soreness of his throat six weeks before admission, which was the time when his vessel made the British Channel. We ordered the patient the sarsaparilla decoction, with nitro-muriatic acid, and touched the sore with a strong solution of nitrate of silver, which caustic was changed in some days for the butter of antimony. No good effect was produced by these means; the sore extended quite round the uvula, which it rapidly destroyed. The breath became fœtid; the cough laryngeal; the patient's appearance was still worse than on admission; his nights were sleepless, and he complained much of pain in the head. I now changed the plan of treatment; omitted the sarsaparilla and the lotion, and ordered a gargle of chloride of lime, with the internal use of six grains of opium daily, and an increase of his wine. At once the sore began to assume a more healthy appearance; the fœtor of breath diminished greatly, and in a few days wholly disappeared. After a short time, in consequence of want of sleep, we increased the dose to eight grains, on which he has been kept since the 20th of February. The sore is now healed, and the whole state of the patient singularly improved. This man has, in the course of a few days, taken upwards of a hundred grains of opium, without experiencing any of its poisonous effects. His slumbers have been light and interrupted, his intellects clear, and his bowels have not been constipated, as he has had one evacuation daily since the beginning of this treatment. He only complains of a slight difficulty in passing urine."

The following are the conclusions to which Dr. Stokes's experience has led him:—

"1. That in certain cases of inflammation of serous and mucous membranes, where depletion by blood-letting, or other antiphlogistic measures, are inadmissible, and the system in a state of collapse, the exhibition of opium has a powerful effect in controlling the disease. 2. That, under these circumstances, the remedy may be given in very large doses, with great benefit and safety. 3. That its effect then is to raise the powers of life, and remove the local disease. 4. That the poisonous effects of opium are rarely observed in these cases; the collapse and debility of the patient appearing to cause a tolerance of the remedy.

5. The cases in which the utility of this practice has been ascertained, are as follow:—Simple peritonitis, in a stage where bleeding cannot be performed; low puerperal peritonitis; peritonitis from perforation of the intestine—from the opening of an abscess into the sac—or, lastly, after the operation of paracentesis in debilitated subjects; violent diarrhœa, supervening in an exhausted subject; phagedenic ulceration of the throat, in a similar individual; and cases of chronic gastritis, and gastro-duodenitis, in patients exhausted by the long continuance of the disease. 6. The cases in which this mode of treatment would be probably useful, are—peritonitis from rupture of the bladder or uterus; traumatic rupture of the intestine, or after the operation for strangulated hernia."

Dr. Montgomery on Spontaneous Amputation of the Limbs of the Fœtus.

This is a learned paper illustrative of a curious subject. It is accompanied by an unpleasant looking print, which, however, tells its story plainly enough. Dr. M. found ligaments on the limbs of a five-months' miscarriage, which he thinks, if it had continued to grow in the uterus, would have had its said limbs spontaneously amputated. Those ligaments were of organized lymph, and of remarkable firmness, resembling the kind of thread called *ardis*.

Dr. Graves's Miscellaneous Observations.

Of these, which are continued through both the numbers before us, we shall reserve what we have to say until next week. We like the plan much of throwing together in this way a selection of striking cases, and we hope the stock from which the present supply is drawn will not soon be exhausted.

Mr. Newton's Case of Diseased Heart.

This paper need not have been so long: it is interesting, however, and carefully drawn up. It puts beyond question the efficacy of Hydrosulphuret of Ammonia in cases of Angina depending on organic disease of the heart. "Many objections," as Mr. Newton observes, "apply to the treatment by digitalis: it is always a dangerous and often an uncertain remedy, and even in those cases in which it succeeds best, it soon loses its efficacy; its tendency also to disorder the stomach is often such as to forbid its employment." It was for this reason that Mr. N., at Dr. Marsh's suggestion, determined to try the effects of the hydrosulphuret of ammonia, which had been so strongly recommended by Cruikshanks and Rollo; and it seems to turn out to be that medicinal substance which was so long a desideratum—a depressor of the circulatory function without producing permanent debility. It is also found useful in cutaneous cases. The best way of giving the hydrosulphuret, according to Mr. Newton, is in a state of great dilution,—a few drops in a tumbler of water, two or three times a day.

Dr. Townsend's Contributions to Pathological Anatomy.

The contributions in this paper relate to the diseases of the heart and great vessels. Of one of the shortest we shall make an extract, intending, at a future opportunity, to avail ourselves more at large of some of the others:—

"*Spontaneous Rupture of the Heart.*—The body of a very old woman was brought into the dead-room of the Whitworth Hospital, for anatomical examination, on the 30th of August, 1830. The external appearance of the body did not in any respect indicate previous disease. On removing the sternum, the pericardium appeared unusually prominent, and of a bluish white colour: when opened, it was found to contain more than half a pint of dark clotted blood, which completely enveloped the surface of the heart. When this coagulum was removed, the heart appeared of its natural size, but was enormously laden with fat, especially at its basis and over the right ventricle. On the anterior surface of the left ventricle, near its septum, and at the distance of about an inch from the apex, a longitudinal fissure, half an inch in length, was discovered, the edges of which were jagged, and had evidently been separated by tearing: there was a slight degree of ecchymosis under the serous membrane, in the immediate neighbourhood of the wound. On laying open the left ventricle, it was found that the fissure seen on the external surface extended through the fat and muscular substance, into the interior of the left ventricle. This cavity was quite empty, all its blood having previously escaped through the wound. The length of the

fissure on the internal surface of the ventricle was somewhat greater than on the external surface, but corresponded with it exactly in other respects, being a mere cleft or chink just wide enough to admit the handle of a scalpel. The lining membrane in the neighbourhood of the rupture was soft and friable; and the columnæ carneæ, for about the circumference of a shilling, were of a dull white colour, and so soft as to break down under the scalpel. The left ventricle was of its natural dimensions, and its parietes of their ordinary thickness, but the muscular fibres were pale, soft, and flabby. The other cavities of the heart presented their usual appearance, except that the muscular walls of the right ventricle were as thin as paper, and coated with a layer of fat nearly half an inch deep. The valves were all remarkably healthy for a subject so far advanced in life. The coronary arteries exhibited several patches of atheromatous deposit, sufficient in many points to diminish their calibre considerably. The arch of the aorta was dilated and atheromatous. The other viscera were all healthy, and the muscles of the trunk and extremities appeared even more firm and florid than is usually observed in persons of her great age.

"Upon inquiring into the previous history of this individual, it was ascertained that she was 90 years of age, and had been a servant in the house of industry for many years; her usual occupation was that of scouring the floors: she always enjoyed excellent health, with the exception of an occasional slight cough. In her 88th year she fractured her thigh near the trochanter, but completely recovered from the effects of that accident in the usual time.

"On the morning of her death, she went to the chapel in as good health and spirits as usual, and while in the act of saying her prayers, she suddenly dropped down dead without a struggle or a moan."

The original communications, on the whole, are valuable, and the scientific gleanings excellently made. Among the *Bibliographical notices* in this number, we observe that the Cyclopædia of Medicine is criticized *d'un manière bien tranchante:* Dr. Clutterbuck, we are sure, ought to be very much obliged to the gentleman who perused his article (Apoplexy) so attentively, and enriched it with so much valuable annotation.

MEDICAL GAZETTE.

Saturday, July 7, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

QUACKS AND MEDICAL REFORMERS.

A SUNDAY newspaper, which we happened to take up a few days ago, amused us not a little by the sapient advice which we found the editor giving the public—and that in the midst of a long and grave argument on a most important subject—to choose their legislators in the approaching crisis of perfectibility, *as they have been in the habit of choosing their physicians,* men of known character and acquirements, who have studied the profession which they would fain practise for the public benefit, and who shall be capable of acting for the best on their own sole responsibility. This is surely a very good joke; for we are bound to believe that it is a piece of pleasantry indulged in by the intelligent editor for the sake of diversifying the monotony of his lucubration; otherwise he plainly naps, and dreams of the thing that is not. But if the learned gentleman is really serious, we must only conclude that he is more of the abstract politician than observer of past and present occurrences—more of the book-worm and closet-counsellor—than of the man of the world. What a fund of simplicity there is in the advice, seriously considered! And if it were acted upon, what a goodly host of quack legislators should we presently have assembled within the walls of St. Stephen's! It is too absurd. In a country which is famous through the civilized world for its inveterate partiality for charlatans and impostors—where the grosser the bait the more greedily it is swallowed—who would think of any public instructor suggesting to the people to choose their representatives as they do their medical practitioners? Yet so it is: unless, as we are inclined to believe, the writer in-

tends some joke. But joking or earnest, his suggestion has brought us back to what we were saying a week or two ago on the legitimate objects of medical reform, a subject which it was our design to have followed up by a few further remarks on each particular head which we noticed in our brief list of those objects. First and foremost, in our enumeration, it will be remembered, we pointed out the restraining of quackery by bringing the proceedings of its professors more immediately under the cognizance of the legislature; for we hold, that any interference on the part of the government with the arrangements of the profession that does not thus go to the root of the evil, and at length extend this most wofully-wanted protection both to the public and the faculty, will prove to be worse than useless. The experiment has been tried now quite long enough, in all conscience, to convince even the most sceptical who will but think about it, that there is nothing to be expected from leaving ignorant people to themselves in this matter. It is intolerable folly to calculate upon the growing "good sense" of successive generations: good sense clearly is a plant of a very slow growth—if, indeed, we should not conclude, with respect to it, as an eminent writer did, who pronounced it to be quite as rare a quality even as genius. From a very early period of the history of our laws, divers provisions have been made to supply the place of this ever-wanting good sense. The statutes of Henry the Third, Henry the Eighth, &c. vouch for what we say; but nothing effectual has ever been done, through the mistaken indulgence constantly extended to the most mischievous of impostors. The statutes have been allowed to sleep, —to remain a dead letter—and the public to be grossly abused by the malpractices of every adventurer. In every age, medical quackery has been the great resource of men of broken fortunes and profligate characters; not unfrequently also has it been the refuge of otherwise helpless poverty, against which it would have been deemed a sin to level even the feeble force of the existing laws. A story is told that illustrates the facility with which this refuge was had recourse to. Among the non-conformists in the reign of Elizabeth, there was one who, upon being threatened with being deprived of his benefice, said, in his impatience, that if they (meaning the Commissioners) held this course, *it would cost many a man's life.* The words were thought treasonable and seditious, and the man was called back to answer for himself, why he should not receive the reward of a traitor. But he readily justified what he had spoken:—"You will deprive me (said he) of my benefice; I shall then have no other means left but to *turn physician;* and before I become master of that mystery, God he knows how many lives it will cost!" It was honestly spoken: the man knew that he had little or nothing to fear from the existing laws against quackery—the course which he was about to follow—and he doubted not for a moment that he should find in the people all the credulity that he required.

In the subsequent century, and particularly during the protectorate, there were numerous instances of disbanded soldiers, of the vilest character, taking upon them the practice of physic—for the consequences of which, no doubt, they were prepared by their sanguinary avocations during the civil wars: but why go farther back than modern times, and these our own enlightened days, when the "march of intellect" is supposed to have made such mighty progress—when the most difficult branches of knowledge, and those especially demanding the clearest judgment and good sense—politics, and political economy — are familiarly doled out to anxious auditors, even in the lowest classes? Yet never, perhaps, was common sense more egregiously outraged

than it now is, by the open, unblushing proceedings of notorious quacks, whose rights may be said to be held sacred by the law of the land—and who shall presume to bring their actions against whoever meddles with their *professional and moral character*—and who confidently reckon upon obtaining ample damages for any injurious aspersions that may be thrown out against them. No doubt this state of things, in time, might work its own cure; the constitution of society can bear still greater annoyances than any in this way that it has yet endured; but it would not seem very consistent with the philosophy of the age we live in, to wait for so tardy and expensive a process.

It is probably, then, with a design of anticipation, that some, who profess to be much indignant at the doings of the quack gentry, seriously recommend the adoption of measures of *free trade*—to beat them out of the field, perhaps, by a vigorous opposition on their own ground, and with their own weapons—and to cure society of the nuisance on something resembling the homœopathic, or Hahnemannian principle. We can in no other way account for the measures proposed to be tried by certain interested parties, who are most anxious to send out troops of quacks in disguise, whereby the present possessors of the field may be put to the rout. But the artificial disease, however eventually beneficial it might prove to the public, will, we suspect, be thought too unendurable to be ventured upon, at least until all other methods have failed. People have not as yet learned to repose so much confidence in the new system—nor, which is more essential before an experiment of this sort is adopted, have they so good an opinion of the parties who offer to perform it, as to allow them to take a step so hazardous, without further acquaintance, and more satisfactory references as to character. These, we fear, will not readily be forthcoming, and so this most notable and public-spirited scheme must fall to the ground.

Others there are, the advocates of strong measures, who hold that quackery in England can never be put down without potent means, and cite the authority of Dr. Granville, who, after relating the usages of Russia, comments with satisfaction on the achievements of the Autocrat, and expresses a warm wish that his own College at home were endowed with powers equally despotic and energetic. It is pleasant to find in Dr. Granville an advocate for giving the College unlimited power over quacks—especially if under that denomination we are to include (as unquestionably we must) all those who deal in nostrums and secret remedies: but of that at another time; and, indeed, we recollect that we have formerly touched upon it. With regard to the absolute control proposed to be vested in the College of Physicians, it is too idle a project to deserve a moment's consideration. The very limited powers at present possessed by that body cannot be exercised in almost any instance without exciting public odium; and surely we are not to be told that the endowing them with despotic authority would be an advisable proceeding. To us, we must confess, it appears to be little short of ridiculous; and we only wonder how a notion so absurd could be entertained, much less given to the world, by any sensible person.

Another party, as we think with infinitely more shew of reason, hold that this is a concern that immediately belongs to the administration: it is an affair in which the national character and the national weal are at stake; and one in which the government should act without the intermediate, and, as it happens, utterly inefficient agency of another body. The practitioners of medicine throughout the country—it is certainly not unreasonable to demand it—should be recognized by government; and the basis of that recognition should be the

qualification derived from the medical colleges. The diplomas of those colleges, when obtained, should be registered in an office connected with the state; and there should be a law subjecting to punishment, as for a misdemeanor at least, all those who should presume to practise without being so qualified. It is true that there may be—certain that there is—no small share of quackery carried on under the mask of the diploma: but this is a defect that attaches itself to all human institutions. Unprincipled persons and *roués* contrive to intrude themselves into every profession; and we fear there is no help for it in the present state of things. Nobody, however, will venture to say, that it is better to be without a diploma than with one, and that the public ought not to be better satisfied to have such a guarantee than none at all.

MEMORANDA ON CHOLERA.

In the present number will be found several very important papers on the subject of cholera. Some additional evidence of the disease being contagious is adduced by Dr. Venables*; but this, though interesting, is not now required to prove, that the malady is possessed of that property, as the fact of its occasionally spreading in the same manner as other transmissible diseases rests on precisely the same kind of evidence as applies to small-pox, measles, or scarlatina. In Edinburgh, where, from not being overwhelmed by the suddenness of the invasion, opportunity was afforded of tracing the cases, the evidence of its propagation by human intercourse has, we are informed, been perfectly demonstrative. This we state on the authority of a physician who held an important station in the corps of medical philanthropists—whose arrangements and efficient co-operation have been so creditable to the northern capital.

On the subject of injections into the veins, the papers of Dr. Laurie and others cannot fail to excite interest, though the results have not been of a satisfactory nature. The object, however, is to form as correct an estimate as may be of the value of the remedy, and not to advance more than experience will bear out in regard to any plan of treatment. Dr. Anderson, we understand, has pursued the practice, and with more fortunate results than attended the cases detailed in his paper, in our No. for June 23; thus leading to an inference considerably more favourable than that which Dr. Laurie has been led to draw. In the paper of Dr. Christison, too, some valuable information will be found, in reference to this and other points. It will be perceived that that distinguished physician is, upon the whole, favourable to injections into the veins under certain circumstances.

In Dr. Stevens's account of the second irruption of the disease in Cold-Bath-Fields prison, additional evidence is brought forward in support of the saline treatment. It is proper, however, to state, that a totally different report is made by others, and we know that the existence of any thing like the alleged number of cases of cholera in the prison alluded to has been positively denied. This is a question in which the public is deeply interested, and one on which they have a right to the fullest and most satisfactory information. We have given Dr. Stevens's statements in his own words (page 455), and it is no less imperative on the Board of Health to give the support of their authority to his views, if correct, than it is their bounden duty to expose them, if fallacious. We pledge ourselves to give immediate publicity to any *authenticated* information we may obtain on either side.

* Dr. Venables was lately employed, under the Central Board of Health, as medical superintendant of one of the metropolitan districts; but is now employed by the authorities of Whittlesea, in the Isle of Ely, at *their own expense*, to assist the local medical gentlemen in treating the cholera, at present prevailing at that place.

COLLEGE OF PHYSICIANS.

The last meeting for the season was held on Monday, June 25th, on which occasion a paper, entitled *Cursory Remarks on the present state of Medicine*, from the pen of Dr. Uwins, was read by the Registrar, Dr. F. Hawkins.

The learned author of the paper entered upon some general observations on the medical art, which he seemed to think was more advanced in polity than in practice, attributing the avowedly diminished mortality of the present day to the improvements in the economy of society. He dwelt upon the necessity of a liberal general education as essential to the formation of an accomplished physician, and entered upon the development of his views in a manner which renders an analysis difficult or impossible, and we have neither the means, nor could we afford space, to insert the paper in full.

We cannot conclude the series of notices which we have given of these meetings, without expressing our regret that they have not been more patronized by the Fellows of the College. With one or two exceptions, the interest they have possessed has been derived from foreign sources; and we could mention some, indeed many, of the most distinguished physicians in the metropolis, who have never been seen at any meeting during the whole season. We do not pretend to offer any explanation of this, but merely state it as a fact, and one which we think is much to be regretted. There has, however, been no lack of visitors; indeed, the rooms have always been well filled, and occasionally very much crowded; nor have there been wanting numerous eminent men in the other learned professions who have honoured these reunions with their presence.

BIOGRAPHICAL SKETCH OF SIR JAMES HALL*.

This eminent philosopher expired in Edinburgh on Saturday last, and his death extinguishes one of the last of a bright constellation of genius and science which marked the commencement of the present century. Sir James Hall, who devoted himself from his earliest years to the cultivation of science, and became eventually the President of the Royal Society of Edinburgh, was the friend and companion of Playfair, Wollaston, Davy, and of all the other prominent names of the day. He delighted to engage these master spirits in that amicable kind of controversy which has for its object the sincere purpose of elucidating truth;—he was ever ready, at any cost of patience or labour, to investigate the foundations of his opinions by the rigorous test of experiment. In this walk of philosophy Sir James Hall rendered himself particularly remarkable, not merely by the boldness and originality of his conceptions, and the penetrating sagacity by which he anticipated consequences, but by the ingenuity with which he devised his trials, so that the results might not merely satisfy himself, but carry irresistible conviction even to the minds of the most resolute of those who maintained scientific doctrines opposed to his own.

We have not room to instance more than one or two of these brilliant experiments. Dr. James Hutton, who may well be called the father of the present system of geology, in his well-known Theory of the Earth, had asserted that the great class of rocks called Trap or Basalt, owed their origin to fire—or, in other words, that they must all at one time have been in a state of fusion, like melted lava, with which in fact he conceived them to be identical. The opponents of this theory said, "This cannot be true, for if we melt a piece of Basalt, it becomes, on cooling, a mass of glass, not of stone; therefore it can never have been in a state of fusion."

Dr. Hutton at once perceived the want of keeping between the experiments tried by his opponents, and those stupendous operations carried on by nature, and he reasoned thus:—"If nature melts a mass of rock of great extent, it will as a matter of course cool very slowly. We cannot imitate nature in the scale of her proceedings, but we may in the rate at which she works." This argument the author of the theory considered sufficient. But his pupil, Sir James Hall, led, we believe, by some circumstances connected with melted lava which had fallen under his own observation at Vesuvius, determined to try what could be done. He accordingly took a piece of Basalt, melted it, and in

* By a correspondent of the Times.

the first instance cooled it rapidly. It became glass, as he had expected. He now melted this piece of glass, but instead of allowing it to cool quickly, gave it several hours, and in some cases many days, to adjust its mysterious arrangement of crystals, and in the end it came out a mass of stone so identical with the original rock that the closest scrutiny could scarcely detect the difference.

In like manner it was denied that Dr. Hutton's igneous theory of lime-stone could be founded in fact, since even a moderate temperature drives off the carbonic acid, and leaves only quick-lime, an infusible material. "Very true," said Dr. Hutton, "but why should we suppose that nature will permit the carbonic acid to fly off? Who knows that it may act as a ready flux to the quick-lime, when heated and forced to remain in union with it, under an adequate degree of pressure from the superincumbent ocean."

"Let us reduce this also to the test of experiment," said Sir James Hall. "No," answered Hutton, "our means are too small—we cannot hope to imitate nature on such a scale—we shall fail for want of power, and thus bring discredit on a theory which is already strong enough."

Sir James, though he did not share these doubts of his great preceptor, felt bound to acquiesce, and during Dr. Hutton's life-time the subject was not touched. But the instant his hands were free, to work he went, and after a series of varied trials, which furnish some of the most beautiful examples of experimental philosophy any where to be met with, Sir James Hall succeeded in confining the carbonic acid so effectually, that out of a common oyster-shell he formed, at a heat not greater than that of melting silver, such perfect specimens of marble, that an experienced ornamental carver in stone exclaimed, when he saw one of the specimens, "If the quarry from which this marble has been taken lies near a great town, it will prove a mine indeed!"

We could quote many other instances of our accomplished countryman's sagacity, ingenuity, and perseverance; and we particularly regret that the narrow compass of a paragraph like this denies the requisite space for doing any justice to Sir James Hall's *Theory of Gothic Architecture*, of which every body has heard, but which hardly any have ever had the means of fairly understanding; for the work published on that subject (by some mismanagement) was made so expensive, that it still remains far beyond the reach of those by whom it would be most appreciated. We trust that, ere long, we may be favoured with a single volume, containing not only a sketch of Sir James Hall's scientific life, but a connected series of his discoveries. None of his papers, indeed, run to any great length; and even his elegant and popular architectural speculations might easily be condensed into a compass suitable to the taste of the present day.

Sir James Hall, as we have said, associated during all his life with the most distinguished men of science and letters of his day, and by these men his conversation was very highly valued. His ingenuity, however, and his vigorous power of generalizing—great though they undoubtedly were—remained at all times subordinate to his love of truth; and, what was still more rare, his ardour in the pursuit of knowledge, and his irrepressible eagerness in seeking to convince the minds of others, never once disturbed his own equanimity, nor left the slightest trace of pain on the feelings of the most sensitive of his company. Every person who enjoyed the pleasure of his society, indeed, will bear us out in this praise. But it is only those who lived with him in the close and familiar intimacy of domestic life, who can duly bear testimony to that unequalled stability and sweetness of disposition, as well as generosity of purpose, which were so constantly present to his thoughts.

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0°. 3′ 51″ W. *of Greenwich.*

June 1832.	Thermometer.		Barometer.	
Thursday . 28	from 43 to	77	30·29 to	30·34
Friday. . . 29	51	77	30·35	Stat.
Saturday . 30	53	74	30·32	30·29
July.				
Sunday . . 1	41	77	30·27	30 23
Monday . . 2	41	76	30 14	Stat.
Tuesday . 3	41	71	30·10	30·04
Wednesday 4	46	79	29 93	29 94

Prevailing winds N.E. and S.E.

The particularly favourable weather of the past week has enabled many to secure a good crop of hay.

Charles Henry Adams.

NOTICE.

Mr. Arnott's communication was too late for the present number.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 14, 1832.

SUBSTANCE OF THE LUMLEIAN LECTURES,

Read before the Royal College of Physicians,

By Dr. Watson.

May 1832.

Lecture II.

Cause of the Black Colour of the Blood in Hæmatemesis — Intestinal Hæmorrhage — Hæmorrhage from the Urinary Organs.

Dr. Watson said that much of what had been stated in the last lecture concerning hæmorrhage from the stomach, was equally applicable, as might indeed be expected, to hæmorrhage from the intestines. The same mucous membrane clothing parts engaged in the same office, and bearing the same relations to the adjoining viscera, is subject, in its several parts, *mutatis mutandis*, to the same forms of hæmorrhage. Similar effects, in kind, are produced upon different portions of the continuous membrane by the action of corrosive poisons, or by the morbid obstructions of the neighbouring organs; and, in fact, effusions of blood from the stomach, and from the bowels, occur very frequently, either simultaneously, or in immediate succession, as mere symptoms of the same disease. He should not dwell, therefore, upon those circumstances which are common to the history and pathology of both these species of hæmorrhage, and which had been already considered. There were, however, a few points peculiar to *intestinal* hæmorrhage, which required a separate notice.

But before he proceeded to these, he desired to go back for a moment to the subject of hæmatemesis, and to repair an omission in the last lecture, of some importance, in regard to certain appearances of the evacuated blood.

In most cases of hæmatemesis, the nature of the fluid vomited is evident and unequivocal: in some it is less obvious. In speaking of the methods by which, in doubtful cases, we seek to distinguish between hæmoptysis and hæmatemesis, he had stated that in the latter the effused blood was always of a comparatively dark colour. This has been sometimes explained upon the supposition that blood from the lungs is rendered florid by the admixture of atmospheric air. But this is not the whole of the matter; nor is it true that the dark colour of the blood ejected in hæmatemesis is always, or solely, due to some morbid alteration effected in the blood while yet circulating in its proper vessels. There is another cause, which till of late has been much overlooked, but which frequently changes the colour and appearance of the blood, *after* it has been extravasated into the stomach; and that in so great a degree as sometimes to render doubtful, or to disguise altogether, the real nature of the fluid vomited: this is the *chemical agency of the gastric acid.* The effect of acids in blackening the blood out of the body, is well known; and it is somewhat singular that the ascertained existence of an acid secretion in the stomach, varying in quantity at different times, and under different circumstances, was not sooner applied in explanation of the dark colour of the blood, and its occasional blackness, when vomited. Dr. Prout has shewn that the fluid secreted, during digestion, by the healthy stomach, contains a sensible proportion of free muriatic acid; and that under certain conditions of disease, a large quantity of the same free acid is found to exist in the stomach. There are some cases also of dyspepsia in which the acetic acid, or some acid very like the acetic, prevails. The experiments of Tiedemann and Gmelin on Digestion, seem to prove also, that when the villous coat of the stomach is subjected neither to the stimulus of food, nor to the irritation of disease, the fluid by which it is lubricated possesses no acid properties. These statements enable us to understand why it is that blood thrown up from the stomach differs so much in its appearance at different times, and also why it is almost invariably of a darker colour than blood poured out from other parts. For if,

as Tiedemann declares, one of the circumstances determining the generation of the gastric acid be irritation of the mucous coat of the stomach, we may expect it to be present with that congested state of the membrane, or that unnatural condition, whatever it may be, which precedes the efflux of the blood. The degree of blackness of the blood when vomited will be in proportion to the relative quantity of acid which it meets with in the stomach, and the intimacy of their admixture. Sometimes the blood is clotted, and not very much altered in colour; sometimes it is grumous, brown, of a chocolate colour, or like coffee-grounds: this generally denotes the existence of organic disease; and the appearance of the blood is probably modified in some degree by the morbid process which leads to its effusion. There are good reasons for believing that in the black vomit of the yellow fever the colour of the blood is affected, even whilst circulating through the blood-vessels; but that the black appearance of the matter vomited is in great part owing to the chemical action of the gastric acid, may be inferred from the alleged fact that the fluid so discharged is always intensely acid. There is a disease well known, and often described of late years under the name of melanosis, consisting in a secretion, or a deposit of black matter, commonly in a solid form, in various parts of the body. Now Andral has described an effusion of black fluid into the stomach as belonging to this disease. He states at the same time that an accurate analysis of the fluid shews its composition to be very much the same with that of the blood. May we not suspect that this inky fluid really consists principally of blood that has been blackened subsequently to its extravasation by the acid with which it mixes in the stomach? Upon the same principle may be explained the dark brown, or almost black colour, of the spots which are sometimes seen (probably when there has been a great predominance of acid) in the substance of the mucous membrane of the stomach, or even beneath it, and which have also been set down as melanotic. They are so like, in all circumstances except in the single particular of colour, to the crimson spots which are obviously formed by minute extravasations of blood in the same parts, that we can scarcely refer them to any other source. It is probable, too, that in specific disease of the stomach, some of the appearances which have been ascribed to gangrene have, in truth, depended upon the blackening effect of the gastric acid upon the congested surface.

There can be no doubt that this acid, when intense in strength, or copious in quantity, is capable of changing the colour of the blood, after death, even while it is contained in the sub-mucous blood-vessels. In these cases it must be conveyed to the blood by imbibition. The very same thing takes place when strong acids are introduced into the stomach from without; when, for instance, the sulphuric acid—or what is perhaps more to the present purpose, the vegetable oxalic acid—has been taken as a poison, it has the effect of blackening, or as it were, charring the blood, with which the membrane becomes loaded, in consequence of the irritation produced by the poison. It does this where no destruction of the mucous membrane has been produced.

It is to Dr. Carswell that the credit is due of having been the first to perceive, and to explain, this cause of the blackened state of the blood while yet remaining in its proper vessels. In a valuable essay on perforations of the stomach, he has shewn how this doctrine may be employed in the elucidation of some disputed, and very important questions in pathology, and in the science of forensic medicine. It has been thought a difficult matter to determine, from a mere inspection of the stomach, whether a perforation of its coats has been occasioned by disease, by corrosive poisons, or by the action of the gastric juice after death. The efficacy of the latter agency, first alleged by Mr. Hunter in this country, has sometimes been denied altogether. Now Dr. Carswell has not only proved, experimentally, as indeed others had previously done, that perforations of the coats of the stomach may, and do result from the solvent power of the gastric juice, but he has also pointed out certain tests by which we may in general satisfy ourselves in any given case, whether this cause has or has not been in operation. The researches of Tiedemann led him to the conclusion that when the mucous coat of the stomach is not subjected to some stimulus, the fluid which lines it is not acid, and does not possess any particular solvent action. Dr. Carswell satisfied himself also that whilst the process of digestion is going on, the gastric secretion is always acid. Now whether this acidity be the consequence of some change that takes place in the composition of that fluid upon the introduction of food into the stomach, or whether there be no proper gastric juice secreted at other times, whichever supposition we adopt, we perceive a reason why the coats of the stomach do not always undergo solution from this cause after death. When the gastric juice is acid, it acts as a chemical solvent on the dead animal tissues, and it changes the colour of the blood contained in the neighbouring vessels. Dr. Carswell describes the following alterations in the blood, as having been observed in *all* of his experiments which were made upon rabbits. "The blood-vessels distributed on the softened parts, and also on every part where imbibition had occurred, no longer presented the red and blue ramifications

which are remarked in the healthy state when they contain venous and arterial blood. They formed brownish, brownish black, or almost pure black arborescences. In the great sac of the stomach the *first* colour was most common, while elsewhere, and in proportion as an approach was made to the origin of the vessels, the colour became deeper and deeper, a circumstance evidently depending on the greater quantity of blood contained in the latter situations. In those parts, on the contrary, where the liquids could not come in contact with the vessels, or be conveyed to them by imbibition, the vessels preserved their natural colour, and formed a very striking contrast, by their redness, with the deep tint of the others."

When, therefore, in cases otherwise doubtful, there having been no poison detected, nor any evidence from the history or symptoms that poison has been taken, we find that the secretions of the stomach impart a red colour to litmus paper, and also that the blood in the surrounding vessels is thus charred, we have strong grounds for ascribing this appearance, and the perforation that accompanies it, to the chemical agency of the gastric juice.

[Several drawings, by Dr. Carswell, were exhibited in illustration of the preceding remarks.]

When blood is poured out in the stomach, and is blackened there, and afterwards passes onwards through the intestines, it will exhibit more or less of the same dark colour on its expulsion from the body. But independantly of this, the admixture of the morbid secretions of the bowels with the extravasated blood, and the changes produced in its sensible qualities by the action of the digestive organs below the stomach, may so much disguise its natural colour as to render the nature of the dejections very questionable. It has been supposed that blood effused into the bowels may be blackened by the intestinal gases—by the sulphuretted hydrogen, for example, or the carbonic acid, which enters into their composition. It is well known that the ancients held that the dark tar-like discharges which sometimes proceed from the bowels in melœna, consisted of black bile; but when the fæcal matters are of a very dark or blackish colour in disease, they owe that appearance more frequently to the presence of blood than to that of vitiated bile, or any other depraved secretion. When we are satisfied that blood has been so excreted, the degree in which it differs from the ordinary appearance of that fluid will afford us some measure of the distance from the rectum at which it was extravasated.

Hæmorrhage from the mucous membrane of the bowels, vicarious of the menstrual discharge, does not occur so frequently as from that of the stomach, nor indeed at all, except from the vessels of the rectum in the form of hæmorrhois.

Sometimes intestinal hæmorrhage appears to result from a sort of passive congestion of the membrane, which is relieved by free purgation.

A large proportion of the cases of intestinal hæmorrhage, of those especially which are comprised under the general denomination of melœna, are dependant, in Dr. Watson's opinion, upon some organic disease in the neighbouring viscera of the abdomen, such as he had sufficiently discussed when speaking of hæmatemesis. These are often called splanenic hæmorrhages; and the effusion of blood is ultimately resolvable into some mechanical impediment in the abdominal venous circulation. Pinel has explained the rationale of these hæmorrhages very clearly. He says the gorged, weighty, compact organs, can scarcely permit the blood to permeate their tissues, or they press upon the adjacent blood-vessels; the blood regurgitates in the veins of the stomach and intestines, and at length escapes through their exhalants.

However, melœna may, and probably does occur, without any complication with splanenic disease, for we see persons get well who are afflicted with it, and in whom there are no other marks of such disease. These persons are mostly spirit-drinkers; and the continual stimulus thus applied to the membrane of the alimentary canal may induce such a sanguine congestion of its sub-mucous capillary system, as leads at last to the effusion of blood.

Still, when we consider that it is in such persons that induration of the liver is proverbially frequent—that patients live long with that kind of liver disease—and that it often fails to disclose itself by unequivocal signs, we may be permitted to doubt whether many of these cases of recovery from attacks of melœna do not come within the category of splanenic hæmorrhages.

Intestinal hæmorrhage sometimes occurs as one of the accidental symptoms of continued fever; and there is scarcely any other combination in which it becomes a subject of so much interest to the practical physician.

There are at least three conditions of fever with which hæmorrhage from the intestines may be associated.

In certain cases, a considerable quantity of dark-coloured blood is discharged from the bowels, by one or two copious evacuations. The hæmorrhage does not continue, nor return, and the state of the patient is improved after it; he loses, perhaps, a sense of heat, or uneasiness, or aching, or heaviness, which up to that period of the disease had been constantly present; and not unfrequently the hæmorrhage is immediately followed by such rapid convalescence, as to justify the appellation of critical.

This kind of bleeding commonly takes place in the earlier stages of continued fever —and by the way of exhalation—relieving

the congestion of the sub-mucous system of blood-vessels, and therewith many of the more pressing symptoms of the complaint.

But hæmorrhage from the bowels is still more apt to occur, in the progress of continued fever, under very different circumstances. It then takes place unexpectedly, sometimes in considerable quantity, and rapidly exhausts the patient; or it recurs at intervals, to a smaller amount, wasting his strength as effectually, though more slowly. This kind of hæmorrhage is most commonly seen in the advanced stages of the fever; and after death we find the mucous membrane of the bowels, in some part or other, and especially in the inferior portions of the ileum, strewed with ulcerations; and the bleeding seems to have been the result of the division or opening of some of the mesenteric veins by the ulcerating process. It is not always that hæmorrhage ensues from this consequence of the ulceration, for the vessels are often obliterated previously to their erosion; but it is reasonable to attribute the extravasation of blood to this cause, when it occurs under such circumstances as have just been mentioned. Andral relates an example of this kind, in which the effused blood was not voided. A man died suddenly and unexpectedly, in an advanced period of continued fever. Large clots of black blood filled the lower two-thirds of the small intestines, which were crowded with patches of ulceration. No part of the blood had passed the valve of the cœcum.—*Clin. Med.* vol. iii. p. 177.

Hæmorrhage from the bowels is frequently present also in that most fatal form of continued fever which is attended, usually towards its close, by what have been called *putrid* symptoms—petechiæ, vibices, bleedings from various parts of the body, and an extreme depression of the vital powers. In these cases the hæmorrhage is strictly of the passive kind, and it is a symptom of the worst omen. It appears to be connected with an altered quality of the fluids of the body, and especially of the blood, and to be analogous to those effusions of blood from the same parts which occur in scurvy or purpura, and which will be further considered hereafter.

Dr. Watson observed, in closing his remarks on hæmorrhages from the alimentary canal, that this would be the proper place for some account of that very common species of intestinal bleeding which proceeds from the vessels near the extremity of the rectum, and is connected generally with the formation of the tumors called piles, but that he must content himself with repeating, that this hæmorrhage (hæmorrhois) affords the best instance we have of *the constitutional hæmorrhages;* that it occurs frequently in the male sex, with almost as much regularity as to period and quantity as the catamenia in the female; and that it becomes an object of interest to the physician, chiefly on account of this further and important point of resemblance between them—namely, that when thus habitual and constitutional, its accidental interruption is almost uniformly the cause or the effect of some derangement of the health, and often generates a tendency to the production of supplemental hæmorrhage in other parts of the body. The diagnosis of this species of hæmorrhage is easy, and its treatment, as far as the loss of blood is concerned, is simple and well-known.

The examples of internal hæmorrhage hitherto spoken of, have all, with the exception of that which takes place within the cranium, been drawn from that large extent of mucous membrane which forms the surface of the respiratory apparatus, and of the digestive cavity. The second of the two great divisions of these membranes established by Bichât, that, namely, which clothes the inner surfaces of the urinary passages, and of the organs of generation, presents the same character as the former, in being a very common seat of hæmorrhage. Dr. Watson proposed to consider, next, some of the phenomena which occur in connexion with *hæmaturia*, or bleeding from the urinary organs.

That this is a symptom which must needs be of much interest to the subject of it, to his friends, and to his medical attendants, will be obvious, when we reflect—that a passage of blood from the urethra may, and often does, betoken the existence of disease, lingering and distressful in its nature; either irremediable by art, or inducing the necessity of painful and dangerous operations for its cure, and tending to shorten, and almost sure to embitter, the remainder of life. And the interest which belongs to hæmaturia as a symptom is still further heightened, by the doubt and uncertainty in which the nature and signification of such hæmorrhage is not unfrequently involved; for the exact diagnosis of the disease of which the hæmaturia is a sign, though sometimes easy, is sometimes also very difficult. The blood may have come from various parts of a long and somewhat complex tract of mucous membrane. Thus it may proceed from one or the other, or both of the kidneys—from each or either ureter—from the bladder—or from the urethra.

One very common source of hæmorrhage from the kidneys, is the formation of calculous matter in those glands. The pressure occasioned by the aggregation of the earthy mass, or by its accidental change of position, gives rise to ulceration of, or actually lacerates, the blood-vessels with which it may be in contact. And in those cases in which the calculus descends into the bladder, and is ultimately voided, it may, in succession, give rise to hæmorrhage—1st, from the kid-

ney from which it is separated; 2dly, from the narrow tube of the ureter through which it is forced; 3dly, from the bladder, which it enters and wounds, or irritates; and, 4thly, from the urethra, in the last stage of its progress out of the body. There will be the same liability to hæmaturia if the calculus, instead of descending from the kidney into the bladder, is formed originally in the latter organ. The appearance of blood in the urine will therefore, in many cases, warrant the suspicion that there is, or is likely to be, a stone in the bladder. Dr. Heberden, in his Commentaries, says, "Urine made of a deep coffee colour, or manifestly mixed with a large quantity of blood, has within my experience been very rarely the effect of any thing but a stone in the urinary passages. I therefore suppose a strong probability of this cause, whenever I see this appearance."

Again, blood may proceed from the kidney or the bladder, in consequence of malignant fungous disease, to which those parts are liable, and which, though perhaps more surely fatal even than the former, is scarcely, to the unhappy subject of it, so appalling. Another occasional source of hæmaturia is disease of the prostate gland.

Blood is poured out from the mucous surface of these organs under inflammation; but renal hæmorrhage may occur, independently of inflammatory action, and without any discoverable disease, or change of texture, in the kidneys themselves. It sometimes appears to be the consequence of a determination of blood to those organs, taking place without any obvious or intelligible cause.

Dr. Watson here detailed a fatal case of this kind, which had fallen under his observation in the Middlesex Hospital; and after expressing a doubt whether it could justly be considered an example of idiopathic hæmorrhage from the kidneys, he proceeded to state that, at all events, idiopathic hæmaturia was extremely uncommon. Cullen remarks that neither he nor any of his friends had met with an instance of it. He alludes, however, to alleged periodical discharges of blood from the urethra, unconnected with any other symptom of disease in the kidneys or urinary passages. Systematic writers describe hæmaturia as being, not very seldom, vicarious of bleeding from the hæmorrhoidal vessels; it will be proper, therefore, in all obscure cases, to inquire whether the patient has been habitually subject to hæmorrhage from the rectum, and whether that hæmorrhage is suspended. These cases have even been called hæmorrhoides vesicæ. Authors speak also of constitutional hæmaturia—*i. e.* of hæmaturia recurring at certain periods, for years together, without impairing the health, and leaving no traces of disease in any part of the urinary organs after death: and they affirm that an accidental suspension of this form of hæmaturia may be attended with serious constitutional disturbance. Dr. Watson said, that although he had never witnessed any thing like this with regard to hæmorrhage by the urethra, he did not doubt that cases warranting the establishment of this species of urinary hæmorrhage occasionally happened, without, perhaps, being much attended to. The existence of such a form of disease is noticed even by Aretæus.

Hæmaturia also occurs, independently of any strictly local complaint, in the course of certain diseases which affect the system at large; especially in scurvy, or purpura hæmorrhagica. Indeed all the forms of hæmorrhage which have yet been spoken of, are liable to constitute a part of that singular complaint. Bloody urine is sometimes, too, a symptom, and one of the most fatal augury, in typhus fever, small-pox, measles, scarlatina, and the plague.

Blood, or its colouring matter altered somewhat in hue, is excreted with the urine in some cases of dropsy; and especially in that kind of dropsy which is apt to supervene during the convalescence from scarlet fever.

There can be no doubt that an accurate diagnosis of all these varieties of hæmaturia is of the greatest moment.

Now we judge of the seat and nature of the hæmorrhage partly by the nature and appearances of the effused blood, and the mode of its excretion, and partly by the symptoms that precede or accompany the hæmorrhage.

Dr. Prout, whose accuracy of observation and extensive experience of such cases are well known, states, that "when blood is derived from the *kidney*, it is generally equally diffused throughout the whole urine; on the contrary, when derived from the bladder, the blood, for the most part, comes away in greater or less quantity at the termination only of the urinary discharge, the urine having previously flowed off nearly pure."

There are also certain modifications of the visible qualities of the exuded blood, by means of which the same distinguished physician is, in general, able to pronounce with considerable confidence that the hæmorrhage is owing to malignant disease. It is in the reddish sediment which the urine throws down in these cases that the distinctive characters appear; it is scarcely possible to describe them in words, but they strike the eye, and are readily recognized after they have been once observed or pointed out.

There is one appearance which, whenever it occurs, is very characteristic of hæmorrhage from the kidney or the commencement of the ureter—viz. the expulsion with the urine of small cylindrical portions of fibrin, which have evidently been moulded in the ureter, and subsequently washed down

into the bladder by the descending urine. These little coagula are commonly of a whitish colour, the red particles of the blood having been removed; and they often look very like slender maggots, or small worms; and this resemblance is increased when, as sometimes happens, these portions of fibrin are hollow, and contain a portion of red blood, which is visible through their external whitish surface. Cullen states, that he had sometimes observed the blood which had seemingly been coagulated in the ureter, come away in an almost dry state, resembling the half-burnt wick of a candle.

When coagula of this kind accompany the discharge of blood by the urethra, there can be little doubt that the hæmorrhage is renal.

These are some of the points of diagnosis furnished by the appearance of the exuded fluid itself.

And before proceeding to any of the other signs from which our diagnosis receives support, Dr. Watson proposed to consider by what means we satisfy ourselves that the secreted fluid is really sanguineous.

We know that the admixture of a small quantity of recently effused blood will impart a bright red tinge to the urine. We know also, that, in many cases of unquestionable hæmorrhage from some part of the urinary passages, the urine assumes a dark hue, becomes brown like coffee, or even approaches in its colour to blackness. We are apt, therefore, to presume, without further inquiry, that urine which is distinctly of a red colour, or which is so very dark as to appear almost black, derives its peculiar tint from a portion of blood that has been mixed with it.

But the urine may be perfectly red, or almost black, and yet be also perfectly free from blood. There are certain substances which, when taken as food, invariably impart a red colour to the urine. One of these is the prickly pear, or Indian fig, as it is sometimes called—the cactus opuntia of botanists. When the Spaniards first took possession of America, many of them were alarmed by observing that they made what they supposed to be bloody urine; but it was soon discovered that the red colour of the excretion was owing to the abundant use which they made of that fruit. Hennen, in his Military Surgery, quotes a precisely similar example from Ellicot's Journal of his Travels for determining the Boundary of the United States. He says that his people ate very plentifully of this substance, at an island of the Mississipi (Kayo-ani), and were not a little surprised the next morning at finding their urine appear as if it had been highly tinged with cochineal. No inconvenience resulted from it. It would appear (he continues) that the juice of this plant may be analysed into a crimson dye by other processes besides that of the cochineal insect.

Another vegetable substance, with which we are more familiar, and which will produce the same effect, is the beet-root. Desault relates the case of a person who observed that he every morning passed urine of a deep red colour—exactly like that which results from mingling blood with that fluid. No deposit took place, however. The man became alarmed at this, and consulted M. Roux, who, after some examination of the urine, began to suspect that it owed its red colour to some other cause than admixture with blood. In fact, it turned out that this man was in the habit of supping every night upon the red beet-root; and as soon as, on Roux's suggestion, he relinquished this article of his diet, his urine resumed its natural appearance.

A similar change in the colour of the urine is said to be produced by the use of madder as food—by some species of strawberries, and by drinks made of sorrel.

It is necessary to be aware of such facts, because, by eating large quantities of these substances, and by complaining of sensations that do not exist, impostors may seek, without any difficulty, pain, or danger to themselves, to deceive others into a belief that they are suffering from some serious and disqualifying disease; and also because the knowledge of such effects as have just been mentioned will enable us, as in the case related by Desault, to remove unfounded alarm from the minds of those who thus give themselves red urine unconsciously, and without any thought of deceit.

The natural colour of the urine will incline towards redness, independently of any admixture of blood, in many cases of fever or acute inflammation; and, less frequently, urine of a pink colour is found to occur, according to the experience of Dr. Prout, in cases of obstinate dyspepsia, accompanied by organic disease: this tint will be most readily perceptible when the urine is contained in an opaque, shallow, white vessel.

Again, urine of so deep a colour as to be called, in common parlance, *black*, may or may not owe that colour to the presence of blood. Probably when blood *is* the cause of this tint, the blackness may in some degree be attributable to the chemical action of some free acid upon the blood; as was shewn to be frequently the case in hæmorrhage from the stomach.

The urine sometimes *appears* black (especially when collected in a considerable quantity) in jaundice: this is, however, nothing more than a concentration or intense degree of *yellowness*, produced by a large admixture of bile with the urine—as may be seen when a small portion of such urine is diluted with water. It then assumes a *bright* yellow colour. The aspect of the skin in icterus will always be sufficient to prevent this cause of the black appearance of the urine from being overlooked or mistaken.

There has been observed also, though

very rarely, a form of black urine depending upon the presence in that secretion of a peculiar principle, to which Dr. Marcet gave the name of melanic acid. With these two exceptions, almost all urine which is of a very dark or blackish colour, owes that quality to the circumstance of its containing blood; the blood being more or less altered, by various causes, from its original appearance.

[A specimen of black urine was handed round, which Dr. Prout had lent to the lecturer for that purpose. Dr. P. had not seen more than one other example of such urine—that, viz. described by Dr. Marcet. When suffered to be at rest, this specimen deposited a large quantity of a fine, black, crystalline powder, much resembling fine coal-dust in appearance. No account was given of the circumstances under which the urine was passed.]

Now it is not very difficult, in general, to decide whether urine which may be suspected to contain blood really does so or not. In the first place, when blood is present in any considerable quantity, a portion of it subsides to the bottom of the vessel; and even when there is not enough to give a marked or characteristic deposit, the admixture of but a small quantity of blood will be found to disturb the natural transparency of the urine; whereas, the reddish or pink urine, which contains no blood, is clear and untroubled; and if, on cooling, it throws down a sediment, that sediment may be redissolved by heating the urine; a result which does not take place in those cases in which a portion of the blood is deposited. Another test is, that a mixture of urine and blood tinges a piece of white linen dipped into it of a red colour; but the simplest and most satisfactory criterion is afforded by raising the suspected urine to the boiling temperature. If it contains blood, it will furnish a brown coagulum, while that part which remains fluid will regain the natural colour of urine.

To return, however, to the consideration of the *symptoms*, which, accompanying unequivocal hæmaturia, enable or assist us to determine the nature and origin of the hæmorrhage.

The bleeding may be presumed to come from the kidney, or the commencement of the ureter, when it is accompanied by a sensation of heat, or of weight, or of some degree of pain in the situation of the kidney; especially if these uneasy feelings are confined to one side of the body: and this presumption will of course be strengthened if calculi have previously been known to descend from the kidney, and if there be no symptom of stone, or of disease, in the bladder.

We become still more certain of the source of the bleeding when the patient suffers, together with the hæmaturia, what is commonly called a fit of the gravel; the symptoms viz. that are believed to denote the passage of a calculus from the kidney through the ureter, towards the bladder. Sharp pains of the back and abdomen, following the course of the ureter; numbness of the thigh, and retraction of the testicle of the same side, with nausea and vomiting

Again: when no symptoms referable to the kidney or ureter are present, whilst there *are* symptoms of stone or disease in the bladder, or of diseased prostate, we conclude that the blood comes from the surface of the bladder.

The symptoms which accompany strangury, produced by the oil of turpentine, or by cantharides, seem to mark a degree of inflammation of the mucous membrane of the bladder and urethra; and amongst these symptoms there is frequently the excretion of blood in small quantity, producing slight redness of the water, and some few streaks of blood in the mucus. Symptoms, very much of the same kind, have sometimes been observed to occur epidemically, especially in hot climates. M. Renoult describes a troublesome and obstinate hæmaturia which affected numbers of the French troops in Egypt, and particularly the cavalry. It was attended with much pain in the situation of the bladder, and extending along the urethra to the extremity of the glans penis; and with a frequent and urgent inclination to make water. The last drops excreted were often of pure blood, and their expulsion was accompanied by acute pain. M. Renoult had opportunities of examining the bladder, and found its inner membrane inflamed. The same disease appeared among the horses also. Of course, in cases of this kind, the diagnosis of the seat of the hæmorrhage is plain.

Hæmorrhage from the surface of the bladder may take place from chronic disease of that membrane, and without the presence of a calculus there. Mr. Howship has recorded an instance of this kind, which occurred to the late Mr. Heaviside. An old East-Indian, who had long been subject to nephritic complaints, was suddenly seized with what was thought to be retention of urine. A catheter was passed, but, as no urine flowed, it was supposed that it had not entered the bladder, in the situation of which there was a manifest tumor. The patient died the next day, and the bladder was found distended by a very large coagulum of blood, which had come from the diseased mucous membrane. There was no trace of hæmorrhage in the kidneys or ureters.

When pure blood comes away, either guttatim or in a stream, unmixed with urine, and neither preceded nor accompanied by any desire to make water, it is probable that the urethra is the locus of the hæmorrhage.

Bleeding from the surface of the urethra

doubtless may, and often does, proceed from some injury done to the membrane—as in the passage outwards of a calculus, or inwards of surgical instruments. But it is probable that blood is sometimes exhaled from the same membrane, in considerable quantity, under circumstances that favour or produce a strong determination of blood to those organs. Thus a young man came to the Middlesex Hospital with hæmorrhage from the urethra, and said that he had lost a considerable quantity of blood in this manner in the course of a few hours. The hæmorrhage appeared to have been the consequence of an excessive indulgence in sexual intercourse. His own account of the matter was, that he had passed the night with a female, in whom the monthly period had just returned, and he supposed that the hæmorrhage in his own person was the result of a sort of contagion. However, the bleeding was permanently arrested by the introduction of a bougie, which was allowed to remain a short time in the urethra.

Examples of hæmaturia occurring under very similar circumstances, are to be found in some of the periodical journals of medicine. When the hæmorrhage is originally from the urethra, the blood will sometimes regurgitate into the bladder, and coagulate there, and so the case become confounded with hæmorrhage from the bladder itself.

It appears, then, that in many instance certain local symptoms accompany the bleeding, and point distinctly to the part of the urinary apparatus in which the blood has been effused.

But many cases are very obscure. Blood will appear mixed, in greater or less quantity, with the urine, when there is no pain nor other sign which would lead us to fix upon one part rather than another as the source of the hæmorrhage. Dr. Watson believed that hæmaturia bearing this indeterminate character, would generally be found to be *renal*, and to depend upon calculous disease. The hæmorrhages which proceed from *malignant* disease, either of the kidneys or bladder, are often painless; but such disease is comparatively rare, and the nature of the case may frequently be ascertained by those peculiar appearances in the urine which were before alluded to, as being characteristic of malignant disease.

A calculus can seldom remain long in the bladder—or, at any rate, will seldom produce bloody urine—without giving some other evidence of its presence there; but calculi form in the kidney, often in great numbers, and reach a considerable size, without affording any ground for suspecting their existence. We know this, first because they are frequently met with in the kidneys of persons who have never suffered any pain, or obvious derangement of the urinary organs during life; and, secondly, because in persons in whom these calculi descend from the kidney into the bladder, the first notice of their existence is often given by the suffering they occasion during their passage through the narrow ureter. There seems to be an analogy in this respect between renal calculi and biliary concretions. We know that the latter are found in the gall-bladders of persons who, while alive, were never known to have suffered any pain referable to that part, and who never had any symptoms of jaundice. We conclude from this, that while they remain in the gall-bladder, these concretions are harmless, and that all the mischief which is done by them, is done as they are passing through the gall-ducts. In the same way it appears that renal calculi will, sometimes at least, cause no pain or annoyance so long as they remain within the kidney. We draw these conclusions from the suddenness of the pain they first occasion, which often comes on in a moment—and from its remission after a time, when the calculus may be supposed to have passed through the ureter into the bladder; and then, often, attentive observation will begin to detect indications of the presence of a calculus in the bladder, though the irritation which it produces there is at first comparatively trifling.

Yet though calculi may remain in the infundibula, or in the pelvis of the kidney, without manifesting themselves by pain, it is very conceivable that by progressive enlargement they may lay open, by absorption—or by accidental change of position they may lacerate—some of the smaller blood-vessels of the part, and so give rise to painless hæmorrhage. It will strengthen the presumption that such is the source of the bleeding, if it has succeeded (as hæmorrhage from the urinary passages often does succeed) a fall, shock, or jar of the body—or jolting on horseback, or in a carriage. Similar causes will occasion bloody urine when there is stone or disease in the bladder; but then the irritation will be felt in that sensible part—the hæmaturia will *not* be painless—the bleeding will not be the only symptom.

Very many of the obscure cases of hæmaturia may then, in all probability, be referred to this cause; and if this view of the subject be correct, it will render it likely that the alleged instances of idiopathic hæmorrhage from the kidney ought thereby to be reduced in number.

The expulsion of the blood in hæmaturia, whether it be accompanied by much pain or not, is sometimes attended with severe rigors. It is well known that in some persons almost any irritation of the urethra—the passing of a bougie, for example, will occasion a shivering fit. Dr. Watson had lately under his care in the hospital a patient who had hæmaturia of an obscure kind, and

the discharge of the blood was always marked by a smart rigor. Dr. Prout mentions an instance of obstinate hæmaturia, in which the bleeding was constantly preceded by a shaking fit. Dr. Elliotson, too, gives an account, in one of his lectures recently published, of a case of intermittent hæmaturia which happened in one of his patients in St. Thomas's Hospital, who had formerly had the Walcheren fever. He was admitted into the hospital with ague, and he discharged from the urethra every time the cold fit came on a quantity of pure blood. He was cured by quinine, losing the cold fit and the hæmaturia together.

Examples of *periodical* hæmaturia are not very uncommon if we may trust to the records of physic.

There remains one circumstance yet worth noticing in connexion with hæmaturia, and it depends upon the hæmorhage itself rather than upon the disease of which the bleeding is a sign—the coagulation, namely, of the effused blood in the bladder—whether it has originally been poured out there—or has descended from the kidney—or has regurgitated from the urethra. This circumstance is sometimes the source of much inconvenience and suffering, and even of danger to the patient. It may produce retention of urine, and all its evil consequences; and a still worse event is, that it sometimes supplies a nucleus, round which calculous matter is deposited; and thus gives occasion to that horrible malady, the stone in the bladder.

We have reason to believe that a coagulum has formed in the bladder when, after the passage of recently effused blood by the urethra, the patient continues to void brown or dark urine, which deposits a chocolate coloured sediment; and the size of the coagulum may be expected to bear some proportion to the degree of irritation which declares itself in the bladder.

FACTS

RELATIVE TO THE

RISE AND PROGRESS OF CHOLERA AT PLYMOUTH.

Reported to the Editor of the Medical Gazette,

By Edward Blackmore, M.D.

July 7, 1832.

After the abundant communications which have enriched the Gazette, I should not obtrude the present slight narrative on the attention of your readers, but that any authentic information which shall display the march of the disease over various parts of the kingdom—the modification in its symptoms in various seasons, with the variety in the opinions of different observers on its nature and treatment—cannot fail to interest those who shall desire to take a complete view of the phenomena and reign of a malady more awfully interesting than any which it has pleased Divine Providence for centuries to inflict on this empire. The cases which have come under my notice I shall arrange in two classes, in order to exhibit the distinct types which the specific disorder assumes, in which it presents an analogy to other epidemics, particularly the continued fevers of 1819 and 1825, and the scarlet fever of 1829.

Class I.—*Cases of Cholera Mitior, Inflammatoria, Indigena, vel Anglicana.*

Case 1.—Rowter, aged 44, after exposure to the evening cold on Christmas day, was seized in the night with severe cramp in the limbs, vomiting, and purging, which continued the whole of the next day; the dejections said to be serous. At midnight he was seen by Mr. Rendle in a state of collapse, complaining of severe rigors. Three grains of opium, with nine of calomel, were instantly given; and in a few hours the former was repeated, with six grains of the latter. At nine A.M. of the 27th I saw him; the cramp in the legs was severe; his belly tender; the vomiting and purging had abated; he had thirst, headache, and giddiness (from the medicine?); pulse 100. He was bled to twelve ounces; eighteen leeches and a blister were applied on the belly. The blood; clot was florid, but soft. The next day he passed good stools, and was in all respects better. Salines, laxatives, and calomel, were repeated, and for the next six days he was recovering; but on January 4th I found him with symptoms of enteritis. Leeches, hydrarg. c. creta, and ipecacuanha, gave relief. On the 16th, however, after some camphor julep, the disease recurred, and was shortly cured by the same remedies, with colchicum. Had cordials been given, I am certain that the inflammation would have been very serious.

Case 2.—Andrews, aged 21, on May 18th felt weak at the stomach, but rose at three A.M. to clean pigs' bellies for

the market, and was quickly attacked with shivering, vomiting, purging, and severe cramp in the legs; in two hours she was so exhausted, as to appear to be dying. At seven A.M. I found the pulse tense but low; the tongue white. She was faint, and in cold perspiration. Sinapisms were applied to the stomach and thighs, six ounces of blood abstracted (the clot dark, soft—its red particles separated), and calomel gr. ij., magnes. gr. vj. o. h. At ten A.M. there was high fever; the other symptoms abated. Nitre and colchicum given, and a linseed poultice on the stomach. From noon to six P.M. the purging was again severe (six dejections like dirty water); some cramp; little fever; griping in the bowels, and headache; free perspiration and diureses. Blood (six ounces) was again drawn. The next day she was convalescent! About this time many severe cases of disorder of the stomach and bowels occurred to me, which I regarded as gastro-enteritis, but with unusual and early prostration and cramp. All did well under similar treatment.

On Saturday, June 23d, I saw an old man dying of cholera asphyxia, after thirty hours illness. The next day a malignant case occurred to Dr. Corkworthy, in a remote part of the town; when he announced to the authorities that the epidemic had reached Plymouth.

Case 3.—Andrews, aged 35, on June 26th had pain in the epigastrium; at four A.M. of the 28th she was seized with shivering, severe cramp in the left flank and legs, and nausea. In the day she took salts and senna, which purged her much, and then she felt better. At four A.M. of the 29th, a similar paroxysm (four stools passed in two hours), and a feeling of deadness in the limbs. She had suffered much from anxiety and poverty, but could assign no cause of her disease. In a few hours she was bled by Mr. Rendle to ℔j. (a spot of green jelly on the clot), and calomel gr. vj., magnes. ʒss. were given—the latter repeated every two hours; sinapisms to the limbs. At two P.M. I found her relieved; the tongue white and sticky; much urine passed. In the first paroxysm she felt "as if she wanted to make a bucket of urine, and could not void a drop! The pain was not like common belly-ache." The next day she was well. A few yards from this patient I saw a similar case on the same day. The fatal case above happened in an adjoining street. In a remote district I also saw a fine child in a state of pallid collapse, after vomiting and purging for fourteen hours. Reaction was so high the next day, after the use of sinapisms, and calomel with soda, as to require bleeding. He rapidly recovered. At Millbay, near to a fatal case of the malignant disease, on July 2d I saw another case, in which the pain at the stomach, cramp, and general prostration, were severe. She was attacked in a place of worship the day before. Similar treatment speedily restored her. Many other like cases occurred at the same time.

CLASS II.—*Cholera Gravior, Maligna, Spasmodica, Advena, vel Indica.*

Case 1.—Bolton, a pedlar, aged 37 (who had chronic diarrhœa five years ago, with gangrene of the toe, which was cured by the accidental severe use of mercury), on January 24th complained, after coming from Torpoint, of pain in the head and stomach, which was soon followed by severe vomiting and purging. These symptoms persisted until the evening of the 26th, when a surgeon, Mr. Rattenburg, gave him calomel and opium, and castor oil the next day. The vomiting, &c. abated, but intense cramp supervened, the extremities became cold and livid, and at night he died, 72 hours from the attack.

Inspection, 16 hours after death.—The body livid and rigid; serum oozed from the mouth; the abdomen distended by fetid air in the peritoneum; the stomach and intestines also contained much air and serum, and were livid and petechial; the liver small, emulged of all its fluids; the urinary bladder contracted. The horrid fetor prevented the opening of the alimentary canal; and three of the dissectors became ill in the course of the evening,—one had severe pain in the bowels, and two a slight purging! Is not this a sporadic case of malignant spasmodic cholera?—the appearances were unlike those of ordinary inflammation. That the disorder did not then spread in the town is no proof that it is not as truly a case of *the* cholera as any on record. See Bell's book, pp. 85 and 88:—"A sporadic and epidemic case are similar in their symptoms, progress, and issue." The spasms seem to be the

effect of a peculiar irritation of the nerves of the intestines from miasms (animalcular or gaseous,) either received *ab extra*, or generated in the part by the morbid vascular arteries.

Case 2.—Speer, aged 47, of sober habits, had for many days complained of her head, and in the morning of the 4th of May she felt pain in the belly, (then menstruating,) but walked to the quay for coals; after which she had shivers, and the pain in the back and bowels became acute: the next day the crampish pain increased, with vomiting and purging, which continued the whole of the following day; and on the 7th, at 3 P.M. Mr. Readle thought her in an advanced stage of cholera. Heat was applied to the abdomen; and Calomel gr. x. and two of Opium, were given in two doses, with Magnesia and Træ. Calumbæ. At 8 P.M. I found her voice changed, the pulse gone, the body cold and damp; yet the senses were clear, and she complained of pain and noise in the head, intense thirst, and of not having voided water for the day; the tongue white; belly tender and knotted, as if from spasm; the discharges like peas-broth. The sickness had been stopped by the first dose of calomel, &c. and the purging by the second. Death ensued in six hours.

Case 3.—Borbear, aged 6, after being in a hay-field on June 29, having taken only a little bread, water, and fried potatoes, during the day, was attacked at 11 P.M. with vomiting and purging, which continued through the night. At 9 A.M. she was seen by Dr. Corkworthy (to whom similar fatal cases had occurred in the neighbourhood) in a state of livid pulseless collapse; the heart's pulsation 160; pressure on the belly seemed not to excite feeling; the dejections were like rice-water; no cramps were ascertained, but aches in the back and hands had been complained of. After the application of hot bricks and sinapisms, there was at 11 A.M. less discoloration and more sense: Calomel and Soda were then given, but death ensued in three hours. The mother of this child drank cider when heated in the hay-field, and was seized with shivers, pain at the stomach, and vomiting, which continued until the next forenoon, when she became very faint. Carb. Sodæ ʒj was given, with speedy relief.

Case 4.—Pools, aged 46 (?) an intemperate marine, on June 23, supped on pickled conger, after which, in the night, diarrhœa came on, and vomiting the next day; but he again eat of the fish, and took brandy. On the 25th he went to work, and drank ℔ij. of beer, after which his disorder increased. Early the next morning, some more brandy was taken; Dr. Corkworthy (who had zealously exerted himself in other recent cases) then saw him in a state of blue collapse, and instantly applied heated bricks and sinapisms, and gave calomel, with soda and opium. The next day he was better, but there was a comatose tendency, with pain, wildness, flushing, &c. I saw him on June 30th, in a typhoid state, evidently dying of the brain affection. Death ensued the next morning; the corpse was very rigid.

Case 5.—At 7 A.M. of July 2d, the wife of the above had diarrhœa and sickness, without pain: these symptoms increased; but at noon she walked from Millbog into Plymouth (three-quarters of a mile,) and back again, under a burning sun: then severe cramps in the limbs came on, and was followed by copious vomiting and purging of fluid like distilled water, with a little boiled rice at the bottom! Dr. Budd, a disciple of Broussais, saw her in an hour after in pulseless livid collapse; he opened a vein, but no blood flowed; cold water was then given, and a sinapism put on the back. At 3 P.M. I saw her; the body cold and damp, the tongue like ice, yet her senses entire, and she could describe her feelings. At 8 P.M. there was less lividity, but no healthy re action; she was restless, and desired to sleep. (Her dead husband was on the floor.) I saw her the next morning in a similar state. Strong acetic acid on the skin had failed to produce feeling or discoloration. She expired in a few hours. Was there in this case a congested state of the abdominal viscera, which the use of proper medicines, during the first five hours of the disorder, might have cured? In the last four days six other cases of the present class have ended fatally; but no accurate returns are obtainable, as no legalized Board of Health existed here before this day.

Case 6.—*Salvation by Blood-letting, after the stage of collapse!*—Haynes, aged 13, a fine girl, residing near the house of the above, on July 3d, at 10 A.M. had vomiting and purging, which subsided. A hot foot-bath and

spear-mint tea were given. She ate nothing that day; the next two days she seemed recovering, but complained of back-ache. At 2 A.M. of the 6th, severe vomiting and purging of rice-water, with dreadful cramps and pain in the belly, ensued. Two hours after, Mr. Rattenburg, a surgeon, gave her three small doses of calomel, with opium. The pulse was then low, but no marked collapse. At 10 A.M. I found her in a cold pallid collapse, the skin, limbs, and lips becoming livid; no pulse at the wrists, the tongue white, thirst intense, the countenance much sunken, voice a whisper; the abdominal pain gone; other symptoms remaining. A vein was opened, and only ℥j. of blood like treacle was obtained. I thought it a hopeless case. Sinapisms were applied to the stomach and thighs, oil of turpentine and water of ammonia to the limbs; calomel, gr. j.; magnesia, gr. iij. every hour; and ʒj. of carb. of soda, in ℔ss. of water.

At 2 P.M. I was delighted to find her better! She had been sick only once; passed ℔ij. of rice water in a pot, and some in the bed; felt hungry, and eat a biscuit; had some gruel. The tongue was clean; the extremities less cold, and had regained a little colour and feeling; the pulse was distinct at the wrist; and her voice and manner much improved. Blood was again abstracted (℥iij.); and a grain of opium, with two of calomel, given in two doses.

At 8 P.M. her pulse was excited; the tongue brown and dry; the body and extremities of natural heat and feeling; she complained of pain at the head, and was sleepy; there had been a little vomiting, and copious purging of more feculent stools. An oily clyster was then given.

Cal. gr. j. Rhei gr. ij. o. h. in 8 doses. Salines. The head shaven and blistered; and ℥iij. by cupping, drawn from the neck; some blue ointment rubbed on the thighs.

The next day she was convalescent!

Reflections.—May not all the cases herein narrated be referred to a choleric constitution of the atmosphere?—(in the phrase of Sydenham;)—a peculiar condition of its electrical properties, which has existed for some time in many parts of the eastern hemisphere? or to a specific miasm which is capable of remaining in a latent state,—(Mr. Bell, however, denies that it remains latent in the human body, like other miasms,)—and becomes productive of a malignant epidemic only when other causes concur to bring it into energetic action? In all the cases is the essential exciting cause the same; its effects in individual cases being modified by the condition of the subject,—his good or evil habits, original constitution, &c.; the neglect or abuse of medicine; and the severity of the occasional causes? Or is the malignant class of cases to be referred to a cause essentially different from that of the milder class? Does one class present a disorder analogous to other ordinary diseases,—a local malady,—the effect of common, remote causes, explicable on general physiological principles, curable by ordinary medicine;—while the other class constitutes a specific malady, of which no cause is assignable but that presented by the theology of revelation,—that the Angel of pestilence has been commissioned to breathe in the face of the foredoomed, (see Isaiah, xxxvii. 36, and Lord Byron's Hebrew Melody on the text;) that, as Magendie emphatically said, "it is a disorder which begins in death?" The prevalent malady illustrates what I inserted in the Transactions of the Plymouth Institution, "that epidemic diseases prevail at particular seasons only, and with different rates of mortality, and having run their course, decline and expire. Why they are not at all times equally prevalent, is a mystery; they come without human anticipation, and repel the best-directed efforts of human skill. Is it unphilosophical to resolve their revolutions into the good pleasure of the Almighty? There are many disorders whose rise and fall cannot be referred to any previous abundance or poverty of the means of subsistence, to a state of public calamity, or an extraordinary course of the seasons." The plan of Divine Providence respecting human life and death is a principle which must with reason be admitted.

It is my belief, however, that a powerful occasional, and therefore unavoidable cause, will be found to have given to the specific exciting cause its most deadly virulence;—starvation, intemperance, exposure to the sun, followed by an evening chill, and delay in the use of remedies, are undoubted causes of its fatal malignity. The milder of the above classes will supply useful ana-

logies for reasoning on the nature and cure of the graver cases. Improper treatment, or simply neglect, would have converted the former into the latter, as truly as the mild small-pox has been changed into a confluent disorder by the stimulant regimen. And the mild and malignant cholera exhibit no greater diversity than cases of mild and confluent small-pox, both of which arise from the same epidemic influence. Of course, a variety in the symptoms of the several cases, and particularly the stage of the disorder, require a modification in the treatment proper to the essential disease; but cordials would have been as hurtful in the case of Tort as in that of Andrews (*supra*); and bleeding would have been as proper, if not as successful, in the former, a few hours after the attack, as in the latter. This judgment is in opposition, indeed, to the records of Indian medicine; but those records supply ample evidence of the abuse of medicine.

The premonitory symptoms, it will be observed, were various; in one headache, in another faintness, in a third shivers, in a fourth nausea and vomiting after costiveness, in a fifth purging and acute pain in the stomach, and cramp in the limbs. In all the cases, the rapid prostration and the want of harmony in the local and constitutional symptoms (which De Haen regarded as a characteristic of malignity), plainly distinguish them from the ordinary endemic cholera of former years. The most constant symptom is the peculiar state of the blood, its component parts being easily decomposed. Similar blood I have seen in no other disease where bleeding was salutary, except in typhus fever. This, as well as the state of the nervous system, suggests the conclusion that this cholera is a general or constitutional disorder (although the French ridicule such an idea), with a local determination; its essential cause, a sedative miasm, being applied either (*a*) to the "nervous system of organic life," (to the ganglionic nerves primarily—the spinal nerves being affected sympathetically), or (*b*) to the sanguiferous system, by absorption from the skin, the pulmonic and gastro-intestinal mucous membranes; thus vitiating the entire mass of the blood: as when putrid animal or other poisons are injected in the blood-vessels. See Magendie's Journ. of Physiol. tome i.

In some cases, indeed, no adequate exciting cause can be discovered. The extremes of heat and cold, intemperance and starvation, have been antecedents; persons in the best health, and in confirmed cachexy, have been alike attacked.

It deserves remark, that the fatal cases are confined to particular districts, which seem, in their physical character, to agree only in being little above the level of the sea.

As to the nature of the symptoms, is not the flux a natural effort to eliminate a poison from the vascular system? The loss of the serum and saline parts of the blood is not the essential cause of the collapse, but an effect of the miasmatic influence, which seems first to excite and then resolve the capillary exhalants (just as bleeding to syncope relaxes those of the skin), and in some cases suspends the peristaltic action of the canal (which has been found filled with serum after death); but the profusion renders the blood less fit for circulation, and therefore to restore its saline fluid is a rational part of practice. Venous congestion may not be the whole of the proximate of the serous flux; there may be increase of action in the capillaries of the mesenteric arteries, while the heart and large arteries are in a state of atony.

The cases ended fatally in various modes besides that of simple collapse, or the asphyxia of Mr. Bell; first, by a primitive sympathetic irritation in the brain, or, secondary, after the subsidence of the choleric symptoms. See Bell's book, p. 65, 129, and 131. To this there has been a particular disposition here lately, probably from the heat of the summer. In some cases it is fairly attributable to the treatment, namely, the excess of saline injections and cordials, with opium. 2dly, by gastro-enteritis, or hepatitis, with typhoid symptoms. This was observed in Scotland, and by Sir D. Barry in Russia. See also Bell, page 133.

The remedies which have here been found eminently successful are, in the early stage, blood-letting, antacids, as soda or magnesia, with calomel, and external stimuli, which make a revulsion and assist the extreme circulation: of this class, the mustard, with ammonia and essential oils, are the best. The warm-bath and blisters are certainly bad; they lessen the tone of the ner-

vous system. The stage in which these remedies are chiefly useful, sometimes unhappily passes away in five hours from the first symptoms of disorder. In India the disease has run its course in three hours. Some physicians here are prejudiced against bleeding: let them ponder on this fact—that all under my care who were bled recovered, and in none who died was blood drawn. Opium I regard as an adjuvant, when pain or flux, from irritability of the stomach and bowels, requires it, but in simple passive exudation from resolution of the capillaries of the intestines, it is useless. It is not directly curative on the principle of the disease being spasmodic and it antispasmodic. In Haynes's case it may have done harm. I have found it useful in two cases; but the brain suffered afterward. In the latter stage, in conjunction with the former remedies (Bell, p. 129, bleeds in the blue state), saline injections are rational, and the evidence of their utility is unquestionable. The sanguiferous system requires a direct stimulus—none other is so generally and instantly diffused. In the livid, pulseless state, the interior of the blood-vessels is probably the only or the most sensible part; the skin has lost its vital properties; even nitric acid fails to vesicate. The volume of fluid supplied by injection is an important stimulus, and its saline qualities are useful for changing (in colour at least) the venous blood into arterial, and probably for decomposing the morbid matter in the system. It is silly to ascribe their virtues to their diuretic effects; there is no need of diuresis—the serous flux is sufficient to relieve the supposed venous congestion. The saline injection, however, may relieve the collapse of the vascular system, and yet maintain the disposition to flux. Is the high temperature of the injected fluid necessary, or even innoxious? Mr. Bell, p. 118, remarks, that "a bath at 100 degrees is insupportable in the latter stage!" The powers of the system are low; the body is like a frost-bitten part. We know the effect of putting such a part into hot water. The Indian practitioners use cordials; they admit congestion of the internal parts—that causes which repel the blood from the surface dispose to cholera—they bleed to make revulsion: surely, then, internal stimuli must aggravate this state. The cool lemonade, or nitric-acid water, which Annesley uses, is more rational than ether. See Bell, p. 126, for an instance of the bad effect of stimuli. Has galvanism been used? My first admonition is, that no one remedy must be relied on; a combined system of means, directed to a simple end, is requisite, and the early use of medicine, as if every case might prove fatal.

FURTHER REMARKS ON THE USE OF CALAMINE IN CASES OF CONFLUENT SMALL-POX.

To the Editor of the London Medical Gazette.

Sir,

In communications which I made to you a few months ago, I expressed a belief that the aggravation of disease, which in cases of confluent small-pox occurs from the ninth to the twelfth day, is attributable to the demand made on the powers of the system for the completion of the extensive suppurating processes going on, and the irritation produced by the exposure of large portions of the cutis to atmospheric influence: the following case, which I offer for insertion in your Gazette, though fatal in its termination, appears to me to corroborate the opinion.

John Grimslay, aged 40, on the 16th of February was seized with rigor and sickness, &c.: on the morning of the 18th an eruption appeared on his forehead and limbs; he was removed to Kensington Workhouse: on the 21st I first saw him: the face was much swollen, and covered with the eruption of small-pox, in the confluent form, over the whole surface of the body, excepting the abdomen: the pustules were innumerable; large, livid patches of eruption were to be seen here and there. Such was his general appearance. His bowels were soluble; his urine plentiful; his mind collected; pulse 110; his throat dreadfully sore, swallowing with the greatest difficulty. With the view of ascertaining whether the swelling about the face would be diminished by the application of the calamine, I covered with it the forehead and one half the face, and ordered the following medi-

cines; allowing him for diet, milk, tea, broth, &c. and at his earnest request half a pint of porter.

℞ Liq. Ammon. Acet. ℥iss. Sp. Æther. Nit. ʒiij. Sp. Amm. comp. ʒiss. Tr. Hyos. ʒij. Mist. Camph. ℥iiiss. M. ft. Mist. 4tam part. 6tis horis.

℞ Hyd. Sub. gr. ij. P. Jalap. gr. viij. M. ft. pulv. h. s. s.

22d.—Where the calamine is applied to the face, the swelling is much diminished; the tongue is covered with pustules; pulse 116; expresses himself confidently; the powder operated violently; great flatulence in the abdomen, with distention; urine plentiful; great discharge of viscid saliva; frequent cough. Allowed him in addition a glass of wine.

Rep. Mistur. cum addit. Conf. Arom. ℈iv.

℞ P. Ipecac. comp. gr. x. ft. pul. h. s. s.

23d.—Passed a comfortable day yesterday, taking plenty of nourishment; his night was very restless,—was delirious; fell asleep, and is now collected and composed. The face one mask of eruption; but that portion covered with the calamine much less swollen; urine plentiful; pulse 120.

Rep. Mist. sed addr. Amm. Carb. ʒss. loco Sp. Amm. c. Pulv. Laxat. h.s.s.

24th.—Passed a tolerable day yesterday, but his night was very restless, delirious, but again fell asleep towards morning, and is now collected and composed. The whole body since yesterday has been kept covered with the calamine; bowels twice relieved; gave him an additional half-pint of porter.

℞ Amm. Carbon. ℈j.; Sp. Æther. Nit. ℥ss.; Tr. Hyoscyam. ʒij.; Tr. Cinch. Comp. ℥ss.; Decoct. Cinchon. ℥ivss. M. ft. Mist. cujus cap. 4tam part. 6tis horis.

25th.—Passed a comfortable day yesterday; slept the whole night; appears in every respect better; the confluent patches of eruption noticed in the early stage of the disease not to be observed now; the parts are dry and florid.

Cont. Mistur.

℞ Liq. Opii Sed. gtt. xx.; Inf. Rosæ, ʒx.; Tr. Card. c. ʒj. M. ft. L. h.s.s.

26th.—Going on well; the cuticle of each pustule appears thickened, and little pus to be found in any of them.

Contin. Mistur. et haust. Anod.

27th.—Passed a comfortable night; is tranquil and composed; *complains* of hunger, but is incapable of swallowing any thing but liquids; a hurried attempt to drink a wine glass of porter nearly produced suffocation; from the effort an incredible quantity of viscid saliva was discharged, which enabled him to swallow with comparative comfort. Pulse quick and weak; urine plentiful.

Contin. Medic.

28th.—Slept almost the whole night; general appearances as yesterday.

Rep. Mistur.

℞ Decoct. Aloes c. ʒx.; Tr. Sennæ ʒiss.; Sp. Æther. Nit. ʒj.; Aq. Puræ ʒij. M. ft. L. mane primo s.

29th.—Slept well; no swelling of the extremities; bowels relieved; every broken pustule on the body healed; (the calamine has been constantly applied); expresses himself confidently; urine plentiful; appetite good; pulse quick and weak.

℞ Ext. Cinchon. ℈ij. Ammon. Carb. ℈j. Tr. Hyos. ʒij. Decoct. Cinch. ℥v. Tr. Cinch. c. ℥ss. M. ft. Mist. Cap. 4tam part 6tis horis. Rep. Haust. Anod. Lax.

30th.—Going on well; is collected; pulse quick and weak; no relief from the bowels; gave an injection.

Contin. Mistur.

March 1st.—Slept almost the whole night; complains of hunger, but is unable to gratify it. Pulse quick and weak; mind steady; every broken pustule is healed. With a pair of dissecting forceps, I was two hours engaged in separating the cuticle from the pustules: this might might have been done some days ago, but I shrunk from the task. Twenty-four hours such labour would, in all probability, have destroyed every pustule on the body: such a proceeding requires a very friendly or a very mercenary hand.

Contin. Medic.

P.M.—No relief from the bowels since the 29th. Gave an injection, and ordered the usual laxative draught to be given early in the morning, unless the bowels were relieved.

2d.—The aperient draught (which was given) has produced five liquid motions. He is restless; his extremities cold; his pulse very feeble; his mind wandering, but still answers collectedly to any question; when left to himself it is that the functions of his brain fail him. I tried to remove some of the incrustations from the face, and succeeded with such unexpected ease that I exposed (without causing a complaint) the cutis of the whole face, and covered it with the calamine. To prevent any more purging, I gave him twenty drops of laudanum in brandy and water.

10 o'clock, P.M.—Dead. The whole surface has been kept constantly covered with calamine to this hour.

The general appearance of the body is extraordinary. Countless pustules remain; the cuticle of each thickened, tough, and containing little or no pus; four points or dots of matter are to be seen laying on the cutis. I may say countless pustules are to be seen perfectly healed, and there is not an *ulcerating* spot any where. The legs are not, nor ever have been, swollen. Though large confluent patches are to be seen every where, it is curious to observe how distinct the eruption keeps in other parts, though the pustules every where almost touch each other. This morning the florid colour of the whole surface was very striking; even those livid patches noticed in the early stage are perfectly healed and red. I cannot but refer these healthful (if I may use the expression) appearances to the generous and stimulating diet allowed, and to the medicines employed. I cannot but refer his death to the action of the purgative. I made no post-mortem examination, under a belief that morbid dissections cannot determine such a question.

There are some peculiarities attending this case which I can neither refrain from again noticing, nor can offer any probable conclusions as deducible from them. 1st. The absence of pus in the majority of the pustules in the advanced stage of the disease; and, 2d, the thickened state of the cuticle of each pustule at that period.

The existence of appetite to the last, which was even painful from his inability to gratify it; the due, and even increased secretion from the kidneys; the refreshing natural sleep which he often obtained; the moist and expanded state of tongue; are all circumstances opposed to the belief that the constitutional disturbance is fever in this disease, particularly when we consider the kind of diet allowed, and the course of medicine employed from the commencement of the attack, which in cases of fever even of the typhoid character, would have quickly led to the loss of life, or the immediate embarrassment of any organ failing in its function.

I am induced to believe that this poor fellow's life was considerably lengthened by the remedies employed; and, in all probability, would have been saved had not his mouth and fauces been so thickly covered with the eruption.

HENRY GEORGE.

Phillimore-Place, Kensington,
July 7, 1832.

OBSERVATIONS ON THE SARGASSO, OR GULF WEED.

By GEORGE BENNETT, F.L.S. &c. &c.

THE range of the Sargasso or gulf weed is considered as between the parallels of about 18 or 34 degrees of latitude: the cause for its being found in those latitudes has been thus accounted for. "The sea of Sargasso may be considered as an eddy, situated, in point of latitude, between the regular Equinoctial current, setting to the westward, and those easterly currents put in motion by the westerly winds commencing a little to the northward of the parallel in which the trade winds begin to blow; into this eddy the fucus is thrown out of the gulf stream, as wreck is thrown into the eddies of rivers, where, by variable winds and calms, and partial currents, it floats about on this wide-expanded surface *."

This species of fucus is seen growing abundantly on the rocks along the gulf of Paria, and on the coasts of Caraccas and Tortugas; it also is said to abound more particularly on the Florida Kays and Reefs. Torn from its attachment to the rocks, this fucus floats about borne by the currents far distant from land, continuing its vegetative process; numerous specimens I picked up about

* Purdy's Memoir on the Atlantic Ocean.

the ship were found throwing out new shoots. At the Sandwich Islands, this, or a species of it, has been found.

I can see no reason why sea-weed should not continue the vegetative process detached from the rocks, as its attachment in a majority of instances cannot be considered as forwarding its growth, but for the prevention of its being thrown, by the force of the waves, on the beach, where it would be left to perish. To be hypothetical, do not the saline particles of sea-water and the slime of marine insects, &c. conduce to the nourishment of the plant *? and therefore, whether in an attached or floating state, as long as it is immersed in the sea, vegetation still continues, and this species of fucus forms an illustration of the point; numerous of the mollusca, as well as testaceous and crustaceous animals, are found, having their habitât among the floating masses of this weed.

On the 3d of March (1831), in latitude 20° 12′ N., longitude 35° 39′ W., several plants of the fucus natans were seen about the ship. It is stated by Horsburgh, in his Directory, as being usually first seen in latitude 24° or 25° N., and it extends as far to the northward as latitude 40° or 42° north.

This species of fucus is of a yellowish green colour; the leaves are long, narrow, scattered, irregularly pointed at the edges, and the stems studded with numerous air-vesicles in different stages of growth, each attached by a short pedicle to the stem; in many instances a young leaf would be seen emerging from the air vesicle; on the leaves also grow two beautiful and delicate species of conferva, covering them with their minute and delicate vegetation. On several of the specimens many of the leaves would have the air vesicle (instead of being, as usual, pendant from the stems,) at the summit, forming a rounded termination to the leaf; and some of the vesicles would have a longer and broader pedicle than usual, having the appearance of an embryo leaf.

Among this fucus is frequently captured the syngnathus, or pipe fish, scylleæa pelagica, small nereis, minute brachyurous crustacea, &c. In support of the opinion that the attachment of the fuci to rocks is not absolutely necessary for its nourishment, it has been observed of the fucus nodosus, that "this and some other fuci have no dependence on their root for nourishment, and therefore, instead of being ramified, it is merely a disc or button, by the adhesion of which, assisted perhaps by atmospherical pressure, the weed keeps an uncommonly firm hold of the rock to which it is attached." The air vesicles on a plant of course render it more buoyant than those destitute of them. I have found, that detaching the air vesicles from a plant, and placing it in shallow water, it sank, but where the depth of water was great, the bulk of water was in itself sufficient to keep the weed afloat: the use of the air vesicles is most probably intended for the purpose of bringing some portion of the plant in contact with atmospheric air, or, by keeping the plant to the surface of the water, to receive a greater benefit from air and light, or to prevent the young shoots or other parts of the plant being injured by the violent action of the waves. We find in the fucus buccinalis, or trumpet weed of the Cape, that the stem is hollow, accommodating itself in length to the depth of water in which it grows; is attached to the ground by ramified roots; the stem terminating in a crown of broad leaves expanded on the surface of the water, and kept in that situation by a broad air bladder, in which the stem terminates under the crown; it may aid the plant also in maintaining itself against the force of the waves in the exposed situations in which it is usually found.

Air-bladders are not confined to sea weeds; several plants growing in fresh water are similarly provided. Of these, the jussiœa tenella, found in the rivers of Amboyna, is a remarkable example. Along its stalk are many large oval tubercles, full of air, and each of these is compounded of many others, so that the injury which the plant might sustain from foreign bodies striking against it and breaking the bladders, is obviated *.

In latitude 37° 53′ N., longitude 35° 32′ W., we were out of the range of the gulf weed; but, in latitude 24° 16′ N., longitude 36° 55′ W., we passed it in very large quantities. The presence of the gulf weed in a higher or lower lati-

* We find that, if a cutting of a shrub (the willow, for instance,) is placed in water, it soon throws out new shoots, and afterwards roots, the plant deriving its nourishment from the earthy particles contained in the water.

* Vide Labillardiere's Voyages, Vol. I. p. 334.

tude may depend on the distance of the ship to the east or west, for, the largest quantities of the weed being found to the westward, if a ship has steered her course far in that direction, she may fall in with the weed in an earlier latitude than one that may be in the same latitude, but further to the eastward.

London, 1832.

MEDICAL GAZETTE.

Saturday, July 14, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

MEDICAL REFORM—COLLEGE OF PHYSICIANS.

In the pursuit of our inquiry into what appears to us to be the parts of our professional economy which most require some change—the legitimate objects, in short, of that much-abused term, "reform" in medicine—we last week offered some remarks on quacks and quackery in general. We then held that the law of the land, which protects the public against theft, or swindling, with regard to their goods and chattels, should also step between them and the spoliation of still more important property—namely, at once their money and their health. The system of free trade may, for aught we know, be abundantly applicable in the case of silks and sugars—at all events, these are commodities with respect to which most persons can judge whether they have been well served or otherwise—but to form a correct estimate in reference to a complicated science, wherein causes and effects are often so obscured that the most penetrating eye fails in the attempt to trace their connexion, is to attribute to mankind a degree of sagacity which daily observation shews that they do not possess. On the same principle which has led to the interposition of the law between the imbecile in mind and those who would practise on their weakness, in one instance, we would urge the propriety of similar interposition in all analogous cases. Charlatans who delude the ignorant among the lower orders with pretensions to fortune-telling and jugglery, are treated as swindlers, and visited with the penalties of the law; and surely the not less credulous in the higher ranks of life ought to be equally protected against the more dangerous juggleries of empiricism, and its professors subjected to the discipline of the treadmill along with their kindred vagabonds.

The second point to which we alluded, in our general remarks upon professional reform, had reference to the state of our medical corporations; and here we would premise that the constant cry of those who look with bitter but unavailing regret on the increasing success of this journal — the cry, we say — is, that the Medical Gazette was intended and is bound to support the powers that be, against all innovation—that it is under the influence of the "conservatives," and that freedom of discussion is incompatible with its principles. All this is but a weak contrivance of the enemy, and scarcely deserves notice; more especially now that we can appeal to *volumes* of proof that it is utterly without foundation. We shall not expatiate on the folly of assuming men to be ultra-tories in medicine who never were known as tories in politics, nor dwell upon the Quixotism of running the gauntlet in support of institutions of not one of which they are members; these circumstances relate to us, as individuals, and therefore it were impertinent to force them upon our readers. True that, but for the systematic slander of another, this journal might never have existed; and true, that they who undertook to conduct it, did not also incur the risk attending its infant

existence; but that any influence over our sentiments was ever exerted, or that any attempt was ever made to employ our pages either in the support of any individual or any institution, is false. It is but bare justice—and tardy justice, too—to declare that not one of those with whom the idea of starting this journal originated, ever attempted, either directly or indirectly, to convert it into an instrument for serving any personal purpose; although it was established at a time when our opponents were week after week lauding to the very echo, as monopolizing all science and all skill, certain practitioners whom they extravagantly held up as superior to all others—persons, by the way, whose names are now either mentioned only to be abused, or passed by in neglect as absolute as if the grave had long closed over them*. For ourselves, thus far assuredly are we "conservatives:" that we would rather have our medical institutions, with all their imperfections, perpetuated, than see them levelled with the ground, and science humiliated in the very dust, as would necessarily happen were they submitted to the guidance of needy men, who, having failed to make an income by the legitimate objects of our profession—namely, practice—strive to empress it into the service of politics; and who, affecting to view with horror the "corruption" of the present system, modestly advertise the sale of their own diplomas to all the world; which same precious document, if any purchaser be ever found, is to bear the signatures of a self-constituted "Senate!" and the seal of a "College" which has no existence!! So much for the respective "purity" and "independence" of the parties. But to return:—

We have always thought, and repeatedly given expression to the sentiment, that our medical corporations were defective in some essential particulars, and required considerable changes to accommodate them to the state of the age, with respect to scientific knowledge and political opinion. Take the *College of Physicians;* and who shall say that it admits not of improvement? The charter was granted in the time of Henry the Eighth: is not that alone sufficient to shew that it is antiquated? Can that which suited the dawn of learning, and the despotic reign of a tyrant, be adapted to the growth of science and freedom in the present day? Summary powers of various kinds were bestowed by the government of the period, which now have one and all fallen into disuse—not because they have been formally withdrawn, but because their execution is abhorrent to the spirit and opinions of the present times. The combination of scientific with executive authority is looked upon as unnatural, and can no longer be endured; it was the uncouth progeny of antiquated legislation and infant science, which, to say the least of it, is now superannuated, and which, though it may, perhaps, still breathe in the secret conclave of the College, cannot venture forth into society, nor enforce its chartered privileges against even the most contemptible assailants. In fact, since the slippery subterfuge by which Dr. Harrison escaped was put in practice (alledging that he was a surgeon, not a physician), a mode of baffling the College, so easy and so safe, has been pointed out to any who will condescend to use it, that there is little likelihood of their again attempting to maintain their authority by an action at law.

But there is another and far more influential tribunal to which they may appeal, in support of the honour and dignity of their profession—that of science. Let knowledge and learning, and

* Time was, when no No. of a certain contemporary journal appeared without recording some feat of Mr. Wardrop's: who could tell, from any thing that it has contained for the last three or four years, whether that gentleman still lived?

those only, be the passport to their Halls, and they may view with indifference the decay of their summary jurisdiction. At present, however (as it appears to us,) the attributes and powers which formed the chief strength of their establishment at an earlier period, have become obsolete, without their place being supplied by any influence more suited to the present day;—we mean such influence as learning and character exercise over the minds of men. Education has advanced so much in every class of society, that a very high standard of acquirement becomes necessary to secure any competent degree of superiority. The general practitioner now goes through a scholastic course little inferior, and with regard to medicine in some respects superior, to what a physician did but a few years ago. It is in vain, therefore, for any one, at the present day, to hope that he can maintain a superior grade, without proportionally increasing the amount of his acquirements. Now to this point we suspect that the College of Physicians have not sufficiently attended. In our opinion, no one ought to receive his degree as Doctor in Physic, or his license from the College here, who has not attended, and who does not prove that he has attended with advantage, the practice of some large public institution, during certainly not less than three years—perhaps five would be a more fitting period—and who does not demonstrate that a due length of time has been spent in general and professional studies, and that the extent of his acquirements has been in proportion on all those points on which admission to the College of Physicians ought to depend. Nor should the university at which such knowledge has been attained be inquired into further than to ascertain that it has afforded sufficient opportunities of study. But the system at present adopted, of making those alone who have been incorporated at Oxford or Cambridge, Fellows, and those educated elsewhere, Licentiates, is tacitly to acknowledge that the fellowship does but indicate the place of graduation, and is not what it ought to be—a mark either of superior acquirement or of longer standing. The only circumstance at all calculated to soften down such a conclusion is the plan which has been adopted during the last few years, of transplanting the most distinguished Licentiates into the ranks of the Fellows; but the number of these promotions, limited at the most to one a year, bears so small a proportion to the general body, that the character of the fellowship can scarcely be affected by it farther than giving room for the remark, that some of the most distinguished among those who hold it have acquired their celebrity as Licentiates; and when men like Babington or Prout, Roget or Bright*, are added to the list, just under some youthful graduate fresh from Oxford or Cambridge, it may be matter of question, whether the honour they receive from the College is not lost in the greater brilliancy of that which they bestow upon it.

We are so far from having any feelings hostile to the College, that we would wish to see it take a much higher place in public estimation and professional influence than it now possesses. It has an independent power of creating physicians; why then be subservient, which at present we hold it is, to the English universities? why not lay down an enlarged and comprehensive system of medical education, and exact a very high standard of acquirement, and these being exhibited, grant their letters testimonial without requiring a diploma of the doctorate at all? *The Irish College of Physicians has already taken this step.* On the grades to be established, and the designations to be applied, we will

* See last page.

not venture at present to offer an opinion, farther than this — that the present division into Fellows and Licentiates ought to be abolished. Might not all physicians, admitted as members, have certain privileges; and, among others, that of electing a council or governing body from among those of a certain standing or other specified qualification? Would not a College of Physicians, if constituted in a mode more or less analogous to that which we have supposed—with knowledge and experience, not place and privilege, as the passport to its honours—would not such an institution possess a far higher name in science, and far greater moral influence in the state, than it does at present? To become a member of the London College of Physicians would then be an object of ambition—admission to its honours would be the guarantee of having received a first-rate medical education, and being possessed of the highest professional qualifications. The College would then resume its proper place among the institutions of the country; the judicial power, of which the progress of opinion has deprived it, would be vastly more than compensated by the increase which would result to its moral dignity and weight: and while, perhaps, it would lose a nominal supremacy, it would obtain the first place in the republic of medicine.

CHOLERA IN LONDON.

A considerable number of cases of cholera have occurred in London during the last fortnight, and the disease is still slowly on the increase; though there is nothing at present which can be regarded as amounting to an epidemic return of the pestilence. The influence of a diet consisting of an unusual proportion of fruit and vegetables, has been manifested here as elsewhere, and the effects have clearly shewn that, where the predisposition is generated, the subtle causes of the cholera, whatever these may be, still exist around us in sufficient intensity to produce the disease in its malignant form. Our readers cannot be too energetic in their efforts to enforce the most rigorous attention to regimen at the present moment; the safety of the metropolis may depend upon it. In all the countries which cholera has visited, its most fatal irruptions have been among those whose diet is at once impoverished and stimulated; nor is it possible to conceive any thing possessed of these characteristics more strongly than salads, or other raw vegetables, and half-ripe fruits, qualified with copious libations of adulterated gin. Of how many among our poorer brethren do these constitute a part at least, and often a very large part, of their daily subsistence? If their pittance were spent exclusively upon meat and bread, how different would be their state of health—how much greater their comfort and their safety!

PROVINCIAL MEDICAL ASSOCIATION.

It is contemplated to establish, at Worcester, a Medical and Surgical Association, the objects of which are,

1st. The collection of useful information, whether speculative or practical, through original essays or reports of Provincial Hospitals, Infirmaries, or Dispensaries, or of private practice.

2dly. Increase of knowledge of the medical topography of England, through statistical, meteorological, geological, and botanical inquiries.

3dly. Investigation of the modification of endemic and epidemic diseases, in different situations and at various periods; so as to trace, so far as the present imperfect state of the art will permit, their connexions with peculiarities of soil or climate, or with the localities, habits, or occupations of the people.

4thly. Advancement of medico-legal science, through succinct reports of whatever cases may occur in the provincial courts of judicature.

5thly. Maintenance of the honour and respectability of the profession generally, in the provinces, by promoting friendly intercourse and free communication of its members; and by establishing among them the harmony and good feeling which ought ever to characterize a liberal profession *.

CUVIER'S LAST ILLNESS.

M. Magendie has been giving a course of lectures on cholera at the College of France, in one of which he alluded thus to M. Cuvier :—

"There is also another species of the disorder, that may be called the *insidious cholera*, which puts on the appearance of other complaints—cerebral congestion, for example, and apoplexy. It was to this that the celebrated Cuvier fell a victim, while he was supposed to be affected with cerebral hæmorrhage. It is not attended with blueness of skin, chilliness, nor change in the condition of the blood."

M. Ricord, in a letter to one of the French journals, confirms Magendie's views. "Having called on M. Cuvier (says M. R.) a few days before his last illness was developed, I scarcely entered his room when he said to me, with abrupt impatience, 'I am sick: what must I do?' I then learned that his stomach and bowels were much out of order, and that he had done nothing for them except in the way of regimen and rice-water. To my advice, that he should have leeches applied to the anus, he answered that he was sure he had no inflammation in his intestines: on the contrary, that *he felt rather a sensation of cold there, for which he thought warm applications would be good.* While saying so, he kept his hands pressed on his abdomen, and his whole figure betokened illness. He added that he dreaded leeches, as they always had a tendency to bring on erysipelas in him. We talked of the prevailing epidemic, about which M. Cuvier was evidently very uneasy; he asked me a number of questions about it, and, as I recollect, seemed inclined to attribute his indisposition to it. I have no hesitation in saying that I consider the indisposition under which he then laboured to have been but the prelude of the *masked cholera* under which he sunk."

AN AWKWARD QUESTION.

A patient in one of the French hospitals tells a curious story of the way in which he received his wound. He met a man in arms, in the street, who commanded him to *cry what he thought.* Taken by surprise, he did not know whether to cry for *the King* or *the Republic*; and, in consequence of his hesitation, received a thrust of a bayonet in the side!

The whole number of wounded taken to the Paris hospitals, in the late disturbances of the 5th and 6th of June, was 293. There were 57 killed. Of the wounded, more than one-half have died.

LETTER FROM MR. ARNOTT TO DR. JAMES JOHNSON.

To the Editor of the London Medical Gazette.

Mr. Arnott solicits the Editor of the Medical Gazette to do him the favour of giving insertion to the enclosed letter. When the conductor of a Quarterly Journal descends to personal attacks, either to gratify his own feelings or those of others, the weekly medical press is naturally looked to for aid in promptly repelling those calumnious charges.

To Dr. James Johnson, Editor of the Medico-Chirurgical Review.

Sir,

An article in your journal for the present month has been pointed out to my notice, in which, assuming the representations of a writer in India to be correct, you think fit to accuse me of plagiarism, and of purloining the property of another. To your part in this transaction I shall advert subsequently; I first reply to the statements upon which you presume to make these charges.

From your account, it would appear

* The letter was not received in time to be noticed last week.—E. G.

that a Dr. Wise has recently published, at Calcutta, a pamphlet upon the Diseases of the Vascular System; and that, not in the printed essay, but in a letter accompanying a copy of it, to you or some one else, he takes the liberty of asserting that to him, while house-surgeon at St. Bartholomew's, I am indebted for my attention being first drawn to the subject of phlebitis—that I published as my own "opinions," "cases" to which he first directed my attention—and that the three first cases which I have related were those alluded to. Without commenting on the length of time which has elapsed between the publication of my paper on the Secondary Effects of Inflammation of the Veins, and the advancement of these charges, I at once answer that they are unfounded. My attention was *not* called to the subject of phlebitis by Dr. Wise; I did *not* derive any cases, materials, facts, or opinions, from him; nor do I know what were, or may be, his opinions thereon.

I was in the habit of attending Mr. Lawrence's visit at the hospital before I ever saw or heard of Dr. Wise. The three first cases published in my paper, which that gentleman is pleased to insinuate that I stole from him, occurred in the wards of that hospital, under the care of Mr. Lawrence. *The dissections were made by Mr. Lawrence; and from Mr. Lawrence's notes, which he had the kindness to lend me, my account was derived.* I was in attendance at the hospital when these cases happened; I witnessed their daily progress, and it was the extraordinary circumstances accompanying them which arrested my attention; *not the suggestion of any one.* I never saw a dissection by Dr. Wise of a patient who died from phlebitis, or any drawing of his on that subject. I am not aware of what is meant by "the result of Dr. Wise's labours on the pathology of the vascular system:" the only opinions of his that came to my knowledge were some speculations analogous to those already promulgated by several French writers, upon a supposed affection of the veins in typhoid fevers and erysipelas, and certain changes which had been observed in the blood of the veins in persons who had died of malignant disease.

Thus much for the charges; and now, sir, let me acknowledge, as I ought, the sense I entertain of the manner in which you have brought them forward. You set out by proclaiming that you are going to do "an act of justice;" you expatiate on the cruelty of a man's having any credit or emolument wrested from him by the grasp of another; you mention my name in connexion with plagiarism, and with purloining the property of another; and, after all this, you have the assurance or simplicity to pretend that you are perfectly impartial—that you espouse neither side, and are no party in a quarrel of which I now hear for the first time. I will answer for it that there is not another editor of a medical journal who would have published portions of a letter, containing such allegations against any individual, without making some inquiries as to their veracity—more especially if conscious that three months must intervene before any answer thereto could appear in his pages. For you, sir, there is no excuse in giving currency to accusations the truth or falsehood of which you had the means of ascertaining. You noticed my paper at some length, and therefore ought to have known that the three cases which I am accused of having assumed as my own, I did *not* cite as such, but as belonging to the distinguished surgeon in whose practice they occurred, and to whom, in relating them, I acknowledged my obligation.

I am, sir,
Your obedient servant,
JAMES M. ARNOTT.

2, New Burlington-Street,
July 5, 1832.

CHOLERA IN COLD-BATH-FIELDS PRISON (?)—SALINE TREATMENT.

Official Papers transmitted by Sir David Barry to the Editor of the London Medical Gazette.

HAVING lately seen various contradictory published reports, tending to produce vague notions and undefined alarm as to the cholera cases in Cold-Bath-Fields prison; having recently inspected that establishment on two successive days—viz. the 27th and 28th ult.—and being anxious to avoid all suspicion of having made uncandid or unfair statements; I feel myself called upon to lay before the public and the profession all the facts which have come under my observation, connected with that subject.

On the night of the 25th of June ult.

in a casual conversation with Dr. Wm. Stevens, at the College of Physicians, I learned, with no small astonishment, that he had seen upwards of 40 cases of cholera, in Cold-Bath-Fields prison, within the preceding twenty-four hours. Struck with this formidable announcement, I requested permission to see these cases with the Doctor next morning, but could not obtain an appointment with him earlier than for the 27th.

On that day I proceeded to the prison, rather in a private than official capacity, accompanied by Dr. O'Shaughnessy, whose "Report on the Chemical Pathology of Cholera" entitles him to such high consideration in every thing connected with what has been lately denominated the "saline treatment" of that disease.

Dr. Stevens conducted us round all the wards appropriated to cholera patients. On leaving the prison, at about half-past two o'clock P.M., I observed to him, in presence of Dr. O'Shaughnessy, that I had seen no case of cholera in the prison that day—meaning, as Dr. Stevens appeared to allow at the time, that I had seen none actually labouring under the characteristic symptoms of the disease.

On the morning of the 28th, the following letter (No. 1) was addressed to the Governor of the prison, and, immediately on the receipt of the Governor's answer (No. 2), I received the orders of the Central Board to proceed forthwith to examine into and report officially upon the sanitary state of the prison with reference to cholera, agreeably to the instructions contained in Sir William Pym's letter to me (No. 3).

It will be perceived by my letter (No. 4), and by the notes taken in the prison (No. 5), which I transmitted enclosed, that in this inspection I was accompanied by Mr. Maling, deputy-inspector-general of hospitals, and by staff-surgeon Dr. Macann, who have also signed the notes (No. 5).

The nominal return, marked A, reached the Central Board after the inspection just mentioned had been completed. The return marked B, was received on the 30th ult.; and as no return has been since received from Cold-Bath-Fields prison, it is evident from this circumstance, connected with the letter from the Privy Council to the Governor (marked No. 6), that no new case has occurred in that establishment since the 29th ult.

In transmitting these documents for publication, I beg it to be understood that I am actuated by no wish to impede, and indeed I have no motives to oppose, the full and fair development of the merits of any medicine, but more especially of culinary salt in cholera, having myself given a favourable report of its use in that disease so long ago as the 30th July last year *. No one will rejoice more sincerely than I shall, at the discovery of a really efficient remedy for that dreadful disease.

With regard to the number of cholera cases which occurred in Cold-Bath-Fields prison, from the 2d to the 26th April this year, or during what has been called the first irruption of the disease into that establishment, I find that twenty-four cases only and seven deaths were reported to the Central Board. Not having had an opportunity of seeing any of those cases myself, I shall abstain from making any further remark upon them than this—that the prison, during the period referred to, was repeatedly visited by Dr. Macann, the medical superintendent of the district, and that he has officially reported to the Central Board, that he had every reason to believe that no greater number of cases of cholera than twenty-four had occurred in the prison at that time. But this gentleman may, of course, be himself referred to on this subject, if necessary. D. BARRY.

Central Board of Health,
July 12, 1832.

No. 1.

"Council Office, Whitehall,
June 28, 1832.

"Sir,—A paragraph having appeared in *The Globe* of last night, stating that the cholera has raged with great violence in Cold-Bath-Fields Prison, and that upwards of one hundred had been attacked within the last twelve days, I am directed by the Lords of his Majesty's Privy Council to request, that should there be any foundation for the above report, you will cause to be made out by the medical attendant, with as little delay as possible, a nominal list of all persons attacked by the disease within the prison, during the 26th, 27th, and 28th instant, agreeably to the enclosed form.

(Signed) "W. L. BATHURST.

"*To the Governor of Cold-Bath Fields Prison.*"

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* See Official Reports on Cholera, p. 108.

No. 2.

"Cold-Bath-Fields, June 28, 1832.

"Sir,—I have the honour to acknowledge the receipt of your letter of this date, and in reply beg to acquaint you, for the information of the Lords of his Majesty's Most Honourable Privy Council, that unfortunately much of the statement from *The Globe* newspaper is correct. The medical attendant of the prison is not now in the way. In the course of a short time he will return, when your instructions shall be complied with.

"I have the honour to be, &c.

(Signed) "G. L. CHESTERTON, Governor.

"*To the Hon. W. L. Bathurst, &c.*

"P.S.—The number of persons now labouring under the disease, in its various degrees, is about seventy."

No. 3.

"Council Office, Whitehall, June 28, 1832.

"Sir,—I am directed by the Lords of the Council to deliver to you the enclosed letter, from the Governor of Cold-Bath-Fields Prison, addressed to the Hon. William Bathurst, in which it is stated that the number of persons now labouring under cholera (in its various degrees) in that prison is about seventy; and to request that you will immediately visit that establishment, taking with you one or more medical gentlemen, for the purpose of inquiring into the particulars of the disease said to prevail there, and report upon the same to the Clerk of the Council in waiting.

"I am, sir,

"Your obedient humble servant,

"W. PYM.

"*To Sir David Barry.*"

No. 4.

"June 29, 1832.

"Sir,—Agreeably to the instructions contained in your letter of yesterday, I have the honour to state, that I lost no time in proceeding to the Cold-Bath-Fields Prison, in company with Deputy Inspector General of Hospitals, John Maling, and Staff-Surgeon, Francis Macann, with the view of examining the persons said to be labouring under cholera in that establishment. Enclosed I transmit the notes taken by me on the spot. I return the governor's letter to Mr. Bathurst, and have the honour to be, sir,

"Your most obedient humble servant,

"D. BARRY.

"*To Sir William Pym.*"

No. 5.

"Cold-Bath-Fields Prison, June 28, 1832.

"*Notes.*—Visited the wards appropriated to cholera patients in this establishment at half-past four o'clock, accompanied by Deputy Inspector General of Hospitals, Maling, and Staff-Surgeon Macann, conducted by the governor of the prison, and two visiting magistrates.

"Saw all the wards in which persons said to be labouring under cholera were treated, and examined individually all those said to be on the sick list then present.

"*1st Ward visited.*—Nine patients. One man who had been four days under the saline treatment for premonitory symptoms, had been attacked this morning, after having been discharged from hospital. A genuine case. Attempts were making, by a young man of colour, to introduce the tube for saline injection into one of the veins at the bend of the arm, under the direction of Mr. Wakefield. Tube could not be introduced, as I learned afterwards. Fluttering pulse; livid and sunk countenance. This case will most probably prove fatal. Another man in this ward, looking thin, pale, depressed, hollow eyes, but good pulse, is under saline treatment. When I saw him about half an hour afterwards, his tongue was cold, with a weak slow pulse. Ward small for the number of beds; close, hot, and oppressive, with a very large fire.

"*2d Ward visited.*—Eighteen persons said to be on the sick list in this ward. Two only present, boys, apparently well. This ward consists of two rooms; the inner a narrow slip. The sixteen not present were said to be out walking.

"*3d Ward, called 'No. 5.'*—Six patients on the *Cholera Hospital Book.* Five present. One man complains of constipation of the bowels. One boy has had pain in his side and head, now better; no vomiting or purging. No appearance of cholera in the others at present.

"*4th Ward visited.*—Eleven patients; all present. One boy with slow pulse, and depression of look and spirits; may have an attack in the course of the night. All the others looking well, with no appearance of disease of any kind. Informed by Mr. Wakefield, the surgeon of the establishment, that the diet of the cholera patients consists of arrow-root, tapioca, beef-tea, coffee, and Seltzer-water for drink, *ad libitum*; a wine-glassful at a time.

"*Convalescent Ward.*—Fourteen patients, all looking well.

"*Female Ward, No. 1.*—Nine patients, all looking well. One young woman apparently *simulating cholera*; warm skin, good pulse and tongue. Said to be a very troublesome perverse character.

"*Female Ward, No. 2.*—Nine patients. One now in mild fever. Said to have been a severe case of cholera. One young woman with bad toothache.

"The two men in No. 1, already mentioned, are the only cases which I saw with the appearance of cholera. Yet the Governor assured me repeatedly, that he had

shewn me *all* the persons considered by the medical gentlemen as labouring under any stage of the disease, and referred to in his letter of this day to Mr. Bathurst.

"The utmost cleanliness, regularity, and discipline, appear to prevail in every part of the prison, as far as I was able to judge; and the visiting magistrates, who went round the wards with us, seemed to be actuated by the most humane feelings, and to devote much time and attention to the health and comfort of the prisoners.

"No new case admitted this day.

(Signed) "D. Barry.
"John Maling.
"F. Macann."

It is almost needless to observe, that no part of the preceding notes or statements is meant to refer to any time or circumstances connected with the patients anterior or posterior to the moments at which they were seen by Dr. O'Shaughnessy, Mr. Maling, Dr. Macann, and myself.

D. B.

No. 6.

"Council Office, Whitehall, June 29, 1832.

"Sir,—I am directed to acknowledge the receipt this day of the return signed by Mr. Wakefield, of cholera cases in Cold-Bath-Fields prison, and to request that you will cause Mr. Wakefield to transmit, for the information of the Lords of his Majesty's Most Honourable Privy Council, a *daily* return of such cases as may occur in the prison, instructing him, at the same time, to include in such daily return cases of confirmed cholera only.

(Signed) "W. L. Bathurst.

"*To the Governor of Cold-Bath-Fields Prison.*"

Nominal Return (A) *of Persons attacked with Cholera in Cold Bath-Fields Prison, from the 26th to the 29th instant inclusive.*

Name.	Date of Attack.	Age.	With Diarrhœa, or other Premonitory Symptoms.	Collapse without Pulse.	Remarks.
Jos. Catanach	June 26	25	Confirmed cholera	—	In relating these cases for the information of the Privy Council, I beg leave to observe, that the Cholera made its reappearance in this prison on the 3d instant, with a degree of malignancy far surpassing the violence of the former attack; and that during the period between its arrival until the present time, upwards of 100 cases have occurred with different degrees of severity, out of which number 12 cases have terminated fatally; but I am happy to add, that the remainder are at this moment in different stages of convalescence, and that the disease appears to be gradually subsiding.
John Moore	do.	46	Premon. symptoms	—	
John S. Gower	do.	37	do.	—	
James Sullivan	do.	18	Confirmed cholera	—	
James Gravener ...	do.	37	Premon. symptoms	—	
Thomas Brenner ...	do.	16	do.	—	
Thomas Green	do.	19	do.	—	
Thomas Francis	do.	17	do.	—	
Charles Morley	do.	30	do.	—	
John Brown	do.	20	do.	—	
Georgiana Harris ...	do.	18	do.	—	
Ann Morris	do.	19	do.	—	
William Ring	27th	34	do.	—	
William Thorn	do.	16	do.	—	
Godfrey Nokes	do.	19	Confirmed cholera	—	
Richard Stewart ...	do.	29	Premon. symptoms	—	
Henry King	do.	14	do.	—	
Henry Butcher	do.	10	do.	—	
Joseph Jones	do.	27	Confirmed cholera	—	
Thomas Collins	do.	24	Premon. symptoms	—	
John Pratt	do.	19	Confirmed cholera	—	
William White	do.	21	Premon. symptoms	—	
Caroline Thornton...	do.	24	do.	—	
Elizabeth Leach	do.	19	do.	—	
Mary Akermann ...	do.	29	do.	—	
Mary Smith	do	29	do.	—	
Elizabeth Isaacs ...	do.	30	do.	—	
Mary Bloomfield ...	do.	22	do.	—	
Eliza Groves	do.	26	do.	—	

(Signed) HENRY WAKEFIELD, *Surgeon.*

Return (B).

Ann Smith	Night of the 28th	21	—	Yes	Died half past 10 A.M. 29th inst.
Joseph Allen	29th	32	—	Yes	Improving.

(Signed) HENRY WAKEFIELD, *Surgeon.*

ACCOUNT GIVEN BY THE MAGISTRATES OF THE CHOLERA AT COLD-BATH-FIELDS PRISON,

AND OF THE

EFFECTS OF THE SALINE TREATMENT.

At the Quarterly Meeting of the Magistrates of the County of Middlesex, held at the Sessions-House, Clerkenwell, on Thursday the 12th instant, T. Const, Esq. in the Chair, the following observations were made relative to the late prevalence of cholera in the House of Correction, Cold-Bath-Fields.

The Deputy-Clerk of the Peace read the Report of the Visiting Justices to the House of Correction, dated July 5th, which stated in reference to this subject, that "The visiting justices have to report, that the epidemic called cholera appeared again among the prisoners in the House of Correction on the 3d of last month, and its attack on several has proved fatal. The precautions and arrangements adopted on the former occasion, together with additional measures, have been taken, with a view to mitigate the evils of the disease. The visiting justices have again availed themselves of the assistance of Mr. Crook, as resident in the prison, since the 21st ult., and Dr. Stevens has almost daily attended upon the sick prisoners; and the visiting justices are happy in adding, that the disease is fast declining. The following mentioned deaths have taken place from cholera." The report then detailed the names, ages, and offences, of the persons who had died, being twelve in number; and went on to state, "That having strong reason to believe that cholera had been communicated by the blankets and clothes which had been used by the cholera patients, the visiting justices resolved that the same should be put under ground and covered with quick-lime, in order that they should be effectually destroyed, and infection prevented. That the surgeon has reported (June 28,) that there are in the infirmaries of the House of Correction appropriated to cholera, 57 males, and 18 females."

Sir Peter Laurie wished to know when the report was dated?

The Deputy-Clerk of the Peace replied, on the 5th July.

Sir Peter Laurie said it was important that they should know the state of the prison up to the present moment. The public were very anxious on the subject. His reason for wishing for the information arose from the circumstance of the disease having broken out in Bridewell, which had caused some alarm. On his (Sir P. L.'s) suggestion, Dr. Stevens had been called in to assist the surgeon of Bridewell, and the report of the Doctor's treatment was highly favourable. If they could adopt the same mode of treatment throughout the metropolis, he thought they might subdue the disease.

The Governor of the House of Correction had hoped that he should have had the pleasure of being able to state, that there were no symptoms of the disease now left, but since yesterday they had had one of the officers, and also one of the prisoners, in the new prison, attacked. The gardener in the House of Correction was slightly, but he trusted not materially affected. Indeed, in all the cases good hopes of recovery were entertained, in consequence of the admirable and able treatment of Dr. Stevens. (Hear.) They had had, since the disease occurred a second time, not less than 105 cases; and of those, 36 actually sunk into the state called by medical men *collapse*—a stage of the complaint that was considered by many medical men to be beyond recovery; but of these 36 extreme cases, 20 had been restored to health. (Hear.) That circumstance had encouraged them to persevere in the treatment adopted. It was of the utmost importance that the cases should be watched in the very beginning—that patients should submit to the application of remedies as soon as possible—and that they should be separated entirely from others. It was also very important to destroy at once the clothes which had been used, without any attempt to purify them. He was sorry to say that, in several of the cases lost in this prison, they had every reason to suppose that the infection had been conveyed to the parties from scouring blankets, which blankets had been previously washed in the field; and he observed that when the cholera broke out a second time, it was on that side of the prison nearest to the spot where the blankets had been washed. He attributed the occurrence of the disease in some measure to the blankets, and also to a drain having been stopped. The state of the prison now was really healthy; and he trusted that in future, from the divine blessing on the means used, they would avoid the recurrence of the disease.

Sir Peter Laurie felt obliged by the statement with which they had just been furnished. He was delighted to find that the visiting justices had had the courage to introduce Dr. Stevens, because it was a well-known fact that the faculty considered Dr. Stevens's treatment a plan not to be recommended. How satisfactory it must be to his brother magistrates to find the beneficial results which had accrued from Dr. Stevens's treatment. He hoped the same benefits would be experienced in Bridewell that had been enjoyed at Cold-Bath-Fields.

Sir James Williams (we believe) said that he could not allow that opportunity to pass without adding his testimony relative to the great success which had followed Dr. Stevens's plan of treatment. He thought

that if the court occupied a few moments in dwelling on this subject, and a fair report of their proceedings appeared before the public, through the medium of the press, it might be attended with the greatest advantages. When they looked at the various towns in this kingdom in which the disease prevailed, and saw that usually the first thirty patients died, they must be satisfied that proper remedial measures had not been resorted to. Dr. Stevens had positively asserted, that the administration of opium and brandy was attended with the most baneful effects; and it ought to go forth to the public that by the treatment adopted in the House of Correction, out of 105 patients only fourteen or fifteen had been lost. This was a proof that Dr. Stevens's plan was attended by more success than any remedy which had been had recourse to in any part of London or any where else.

Mr. Rotch would feel that he had ill discharged his duty, after the paragraphs which had appeared in various papers, so contradictory in their nature, if he did not endeavour to explain what had passed under his own observation. He could not but feel, that, as contradictory statements had been furnished to the public, the statement of the visiting magistrates was entitled to as much credit as any published elsewhere. It must have been observed that a letter was published in the *Times*, purporting to be a copy of a letter from the visiting justices to the magistrates. The original was written, in the exercise of a sound judgment, to those who were in office. It was thought necessary to inform them that the cholera had broken out in a dangerous degree in the prison, in order that they might act as they thought proper. In consequence of the letter being published in the *Times*, it was not wonderful that a subject fraught with so great an interest as cholera should attract attention, and an inquiry was set on foot by the Privy Council. It so happened that he (Mr. Rotch) was at Cold-Bath-Fields prison when three gentlemen, one apparently with an authority from the Privy Council, demanded an admission to see the patients. At the outset, he must beg to state, that, from the manner and conduct of these gentlemen, it was quite evident to him that they came to the prison decidedly with the preconceived idea in their minds that there was no cholera there, and that the reports of the visiting magistrates were perfectly erroneous, and consequently not entitled to the slightest credit. Such a circumstance was not very pleasant to the feelings of gentlemen who had devoted great time to the subject (and he might say with some little risk to themselves,) and he was anxious to know what account was to be laid before the Privy Council in opposition to that given the previous day. The report first made to the Privy Council was forty-two cases. Those gentlemen visited the prison on Thursday, the 28th June, and were, he believed, all medical men. He felt it his duty to attend them round the wards, that he might hear their observations, and really know with what view they came to the prison. It would naturally be expected that, on such an occasion, gentlemen sent by the authority of the Privy Council to inquire into the actual state of the prison, would be exceedingly minute in their inquiries, in order to ascertain whether the cases reported were severe cholera, or mild cholera, or not cholera at all. It was, however, a duty which he owed to the country, and also to his brother magistrates, to state how the gentlemen conducted themselves. He wished not to cast any imputation upon them; he was not sufficiently skilled in medical science to know whether they had done their duty or not; but he would appeal to common sense as to whether the facts elicited, and the manner in which they were elicited, were sufficient to give authority for contradicting, in the Times of the following day, the statements contained in the published letter. At the period when the gentlemen visited the prison, they were shewn 16 cases of persons who had been in a state of collapse, and had recovered from it. He would mention one instance to shew the sort of examination that was entered into. There was a female named Clara King, æt. 18, lying in bed in one of the wards, who had naturally a rosy hue. On that day in the preceding week the girl had been in the stage of collapse. One of the three gentlemen looked at her, and turning to him (Mr. R.) not knowing who he was, said, "Here is a case of cholera!" with a smile on his countenance. He (Mr. R.) immediately said, "You presume that she has not been a cholera patient?" "Oh, absurd! ridiculous!" was the reply. At that time the gentleman never asked one question. He (Mr. R.) inquired whether he was distinctly to understand that from the appearance of the girl the gentleman was satisfied that she had never had the cholera; to which he replied, "Most decidedly; certainly never." He (Mr. R.) told the gentleman that he was extremely glad to hear him make the statement he did; by which he presumed the gentleman thought he meant he was glad to find they had been mistaken about the cholera, for he again said, "Perfectly ridiculous!" He then called the attention of the gentleman to the case of Sarah Pooley, and turning to the girl, he asked some off-hand questions. "Are you sick?" "No." "Do you vomit at all?" "No." "Ah!" said the gentleman, "and this is a cholera case!" He (Mr. R.) inquired whether the gentleman meant to say that she had never had the cholera at all; to which he replied,

"Certainly." Now this girl's case, ten days before, was one of the worst in the prison. Those being two such flagrant cases, he took the opportunity of saying that he was extremely glad that the gentleman was so decided, because it corroborated the excellency of Dr. Stevens's treatment. In one case ten days, and in the other only a week, had elapsed since the patients were in a state of collapse, and yet the gentleman pronounced they had never had cholera. That remark seemed to startle them, for they were not prepared for his (Mr. R.'s) deduction, and they asked him who he was. He must say that their observations, from the beginning to the end, were of the most transient kind. He then walked with them into a ward where there were two men, one of whom was just going into a state of collapse, and the other was in the last stage of the complaint, and afterwards died. The gentleman said that was a case of cholera, and the only one in the prison. He then called the attention of the gentlemen to the other case, one of whom said that the patient had no more cholera than he had. He informed them that the medical men belonging to the prison believed they had had upwards of a hundred cases, and they thought that patient would be in a state of collapse in two hours. "Ah! he is under the saline treatment," said one of them, laughing. Now these were facts that had taken place; but when he found that misrepresentations were published in the newspapers, and it was alleged that a false report had been made from the prison, he felt it was time to state facts to the public, and let them know on what grounds the prison report ought to be believed. He (Mr. R.) took it for granted that the observations made in the newspaper came from the Privy Council on the authority of those gentlemen, because they appeared the day after they had visited the prison. He (Mr. R.) then turned to the first gentleman, and asked him whether he had looked at a case to which he then called his attention. The gentleman looked over his shoulder, and said, "Nothing at all; the man has got the bellyache. Let us go into another ward." He was about to leave the room, when he spoke to the third gentleman, who felt the patient's pulse, and said, "That is a case of cholera, I have no doubt." He (Mr. R.) then spoke to the first gentleman again, who reluctantly went to the patient, and then said, "Perhaps it may be a case of cholera." They then went into a ward where there were sixteen male patients, all in a state of convalescence, but some of whom had been in a state of collapse, and recovered. The gentleman laughed, and said, "I suppose these are all cholera patients." He told the gentleman that they were shewing them all the patients who had had cholera in any degree. The gentleman went to one boy with a ruddy face, and said, "Had you any sickness?" "No."—"Any pain in your legs?" "No." The boy thought he meant at that moment, for he had been in a state of collapse. That was the way the questions were put, jeering and laughing from the beginning to the end; that was to say, believing they (the Magistrates) were frightening the public. The gentlemen came there satisfied there was no cholera. It was due to Dr. Stevens and all connected with the establishment, that the public should know how much value to put on the report laid before the Privy Council. He was happy to bear his testimony to the fact, that though some persons did not believe Dr. Stevens's treatment had any value, yet abroad it was acknowledged to be the best. He held in his hand a letter from Warrington, containing the strongest testimony in favour of Dr. Stevens's mode of treatment.—[We have furnished two extracts from this letter at the end of the report.] He (Mr. R.) hoped that in spite of all the visits from all the medical men whom the Privy Council might send, Mr. Wakefield would continue to do his duty to the public and to himself, and report not such cases as they dictated, but such as he was satisfied were cholera.

In reply to a question from Sir John Gibbon, Mr. Rotch again rose, and confirmed what he had before said respecting the superiority of the saline treatment over every other method.

A Magistrate inquired, whether the gentlemen from the Privy Council did not ask questions of the medical men?

Mr. Rotch replied, that the medical men and nurses were all present, but no questions were asked of them. He did not think that the medical men were treated with the respect which they ought to have been; and one of them left the prison before they had gone through the whole of the wards. He (Mr. R.) felt for him, and could excuse any man for acting as he did.

Sir Peter Laurie.—Are not the names of all visitors taken down? You know who these gentlemen were.

Mr. Rotch replied that he did not know their names. He understood that one of them was Sir David Barry.

Sir Peter Laurie.—They went sneering and laughing through the whole prison?

Mr. Rotch replied that he did not say so. Let him not be misunderstood, or misrepresented. His complaint against the three gentlemen was this—that they came to the prison—that they went through it with haste incompatible with the importance of the mission they were sent upon—that they did not ask those questions that in his opinion were calculated to draw forth pro-

per data to enable them to go back to the Privy Council, and state that they had only seen two cases of cholera in the prison.

The following are extracts from the letter to which Mr. Rotch referred. The first is part of a circular letter from Dr. Kendrick, of Warrington, addressed to the magistrates, clergy, and members of the medical profession residing in the districts where the lately imported pestilential epidemic has already appeared, or is likely to appear.

"Gentlemen,—I take as early an opportunity as is prudent of expressing my conviction, grounded not only on my own experience, but on that of others more competent to judge than myself, that the plan of treating the above-named disease, which was first suggested by Dr. Stevens, and adopted by Mr. Wakefield at the Cold-Bath-Fields Prison, with so much success, *is, if punctually followed*, the most effective mode of practice employed since its arrival in this country, and fully equal to the management of all but the more malignant cases. In all such cases, if I am not mistaken, additional measures must be resorted to. I have taken the liberty of ordering, for the purpose of circulation, a reprint of Mr. Wakefield's letter, first published in the *Medical Gazette* for April 28th, 1832.

"I have been induced to take this step from the marked want of success attending the use of opium and stimulants in the treatment of the disease."

From Mr. Thomas K. Glazebrook to Mr. Wakefield.

"Oxford Lodge, Warrington,
July 9, 1832.

"Sir,—The Board of Health having done me the honour to appoint me their secretary, the medical returns necessarily pass through my hands; and I have the highest gratification in stating, that, from every inquiry I have been able to make, the recoveries have been mainly attributable to the system adopted, and so properly panegyrised by my sincere and esteemed friend, Dr. Kendrick, At a future period a list will be made out, when I hope and trust that the Board will express their thanks to you for your admirable letter."

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

LONDON HOSPITAL.

Stone impacted in the Neck of the Bladder—Operation.

Edward Royren, aged 41, was admitted January 29th, complaining of inability to retain his urine, which was constantly passing from him, mixed with blood and flakes of lymph. On introducing a sound, the instrument was felt to grate over a stone in the neighbourhood of the cervix vesicæ. He said that he had experienced some difficulty in passing his urine for six weeks, previous to which he had no complaint of these parts. Subsequently to this period he found that he could not contain his urine, but had a continual desire to pass it, which he was compelled to do immediately. The day after his admission, Mr. Andrews endeavoured to pass an instrument, but the stone appeared to have been moved and prevented its introduction. The stone could neither be felt by the finger introduced per anum nor by the perineum.

February 1st.—Mr. Andrews having procured Weiss's instrument for extracting small calculi from the bladder, passed it down to the calculus and seized it, when it glided from its grasp. After several unsuccessful attempts the man was sent to bed, and

Ordered Baln. tepid. Ol. Ricini, ℥j. Tinct. Opii, gtts. xxx. h. s.

3d.—Has been better the last two days, but is still obliged to make water frequently.

Ordered Sod. Carbon. ℈j. ter die. Baln. tepid. Rep. Ol. Ricini.

The man subsequently had an attack of pain in the perinæum, accompanied with swelling and hardness, indicating the formation of an abscess. It was thought, therefore, advisable immediately to proceed to the removal of the stone.

8th.—Mr. Adams proceeded to perform the operation, in the absence of Mr. Andrews. On introducing the staff, the stone was not distinctly felt by the instrument, but on passing the finger by the rectum it was evidently distinguished towards the right side of the perinæum, and anterior to the prostate gland. An incision was made as in the lateral operation, and after a few strokes with the knife the stone was removed. The man appeared going on tolerably well for some days, but the ravages of the disease upon the constitution were such that little hopes were entertained of his recovery, and he sunk about the sixth day after the operation.

Sectio Cadaveris.—Evident marks of extensive peritonitis, with effusion of semipurulent fluid into the cavity of the abdomen. The inflammation had radiated from the fundus of the bladder, which was in a sloughy state. Extensive suppuration of one kidney, with a calculus impacted in one of the calices; softening of the other kidney, with dilatation of the ureters of both. Bladder thickened, and mucous membrane highly inflamed and coated with flakes of coagulable lymph. An abscess on the side

of the prostate gland, communicating with the urethra and with the abscess in perinæo.

This man stated, after the operation, that he had been cut by Mr. Cline, in St. Thomas's Hospital, above thirty years ago, when he was only eight years of age.

Strangulated direct Inguinal Hernia—Operation.

William Bouge, aged 39, a bargeman, was sent into the hospital from the country February 1st, between 12 and 1. He states that he was in perfect health until Sunday, Jan. 29th, when, as he was raising himself to reach some pailings, he suddenly felt something give way at the lower part of his belly; he was seized with great pain over the abdomen, and passed a scanty motion on the same day; soon afterwards he was sick, and unable to retain any thing upon his stomach. On examination, there was a small tumor, about the size of a walnut, on the left side, proceeding through the external ring, and leaving the inguinal canal perfectly free, having the spermatic cord distinctly to the outside. His countenance was sunk, and pulse weak. Mr. Andrews immediately performed the operation. A great number of layers were divided before arriving at the sack, which contained no fluid, and the intestine was small, and of a dark brown colour, with one or two patches of a darker colour than the rest. The stricture, which was exceedingly tight, was divided, and the gut returned. About three hours after the operation he passed a fluid evacuation mixed with scybala, and felt perfectly easy. At 9 P. M. he had passed another motion, having taken ʒj. of Epsom salts in peppermint water. He complained of slight pain on pressure, and felt a great rumbling of wind; pulse rather quicker.

Ordered — Magnes. Sulph. ʒj. ex Aq. Menth. Pip. 4tis horis. Hirud. xx. abdomini. Pil. Hydr. gr. v. 8tis horis.

Feb. 2d.—Complained of pain in the abdomen, with great anxiety of countenance; pulse rather quick, but not full.

Ordered—Hirud. xxx. V.S. ad ℥xvj. Cal. gr. ii. 4tis horis. Enema.

In the evening he was rather better, but still complained of pain.

Rep. Hirud. Cal. ij. 2dis horis.

3d.—Is better to-day.

Hirud. xii. in the vicinity of the part operated on. Enema commune.

Leeches were applied daily as long as any tenderness of the abdomen remained, and as his mouth became affected with the mercury the symptoms abated.

An abscess formed just above the wound, which was opened, and a considerable quantity of the superficial fascia sloughed; after which the parts healed, and the man returned to the country, March 15th, perfectly well.

Tic Douloureux of a Stump, after Amputation of the Arm had twice been performed, cured by the removal of a portion of the Median Nerve.

Elizabeth Burkett, aged 20, was admitted into the hospital April 21st. She stated that about three years and a half since, she was taken into St. Thomas's Hospital in consequence of severe injury to the left hand, occasioned by a fall. Every attempt to save it having been made, after two years amputation became necessary, on account of her general health suffering considerably from the excessive suppuration and painful sensations in the arm; the stump did not heal favourably; she suffered distressing agony, and her health again became affected. She was discharged from the hospital, and afterwards admitted into Cripplegate workhouse, under the care of Mr. Langstaff*. At that time the surface of the stump presented an unfavourable appearance; the skin covering the ends of the radius and ulna was very thin, excessively vascular, and the ends of these bones seemed likely to cause its absorption. There was also a constant state of convulsive action of the muscles of the stump, accompanied with agonizing pain. Poulticing, opiate lotions, belladonna, and gentle pressure by bandage, were employed to lessen her sufferings, but without any good effect.

Mr. Langstaff afterwards performed amputation above the elbow-joint by the flap operation; and previously to securing the arteries he drew out each nerve to the extent of half an inch from the surface of the stump with a tenaculum, and cut through them to prevent their interrupting the progress of cicatrization of the integumental parts. For some time she was relieved of all the painful sensations she had so long been distressed with; had no recurrence of hysteria or convulsion, and her health improved.

About two months after the last operation she applied at this hospital, complaining of severe pain in the stump, the extremity of which was inflamed and affected with spasmodic twitchings; she was constantly subject to headache, and was unable to obtain rest night or day; altogether her life was rendered so miserable that if relief could be procured, she was willing to undergo ampu-

* The early history of the case is partly taken from a paper by Mr. Langstaff, on the Healthy and Morbid condition of Stumps after Amputation, in the Medico Chirurgical Transactions.

tation at the shoulder-joint. The carbonate of iron, iodine, acetate of morphia, blisters in the axilla, leeches to the extremity of the stump, cupping at the nape of the neck, and various other remedies were employed, but without any beneficial effect, except the cupping, which partly relieved the pain in the head. She continued her attendance as an out-patient for about four months, subsequently to which an exfoliation of bone took place, and she applied at St. Bartholomew's Hospital, where Mr. Earle cut down upon the extremity of the stump. She returned to this hospital without any diminution of her sufferings; and after attending a short time, Mr. Luke having ascertained that the pain proceeded from that part of the stump where the median nerve appeared to terminate, proposed to remove a portion of the nerve, to which operation she consented.

April 21st.—Mr. Luke made an incision in the course of the median nerve just below the axilla; and, after a little dissection, exposed and removed nearly half an inch. The woman thought that something was pulled, and complained of a sensation of numbness at the extremity of the stump: she immediately expressed herself entirely relieved from the pain, and could bear firm pressure upon it. In performing the operation, a small artery was divided, which required a ligature.

22d.—Complains of severe pain in the head, and is feverish; skin hot.

Mist. Cathart. ter die.
Vespere.—Træ. Hyoscyami gtts. xl.

23d.—She has passed a restless night; still complains of severe headache; feverish.

V.S. ad ℥x. Mist. Salin. Rep. Tr.Hyosc.

After the bleeding, her headache was relieved, but she is unable to void her urine, which is required to be drawn off twice a day.

June 12th.—Her health has greatly improved; the wound has healed; and she is entirely free from pain. The retention of urine, however, continues; blisters have been applied to the sacrum, but without relief.

NOTE FROM DR. BUCHANAN, OF HULL.

To the Editor of the London Medical Gazette.

Hull, July 10, 1832.

Sir,

I beg leave to remark, that in your No. for July 7th, containing a paper of mine on the treatment of cholera, there appears a small typographical error in the formulæ of the Tincture of Iodine; where a drachm of the iodine is marked, instead of a *scruple*, to the ounce of rectified spirits.

The strength of the tincture used was the same as in Cooper's Surgical Dictionary, 6th edit. p. 764; which is a drachm of iodine to three ounces of spirit.

Your insertion of the above will much oblige, sir, most respectfully,

Your constant reader,
Thos. Buchanan, C.M.

TRAVELLING FELLOWSHIP.

At a meeting of the Trustees, under Dr. Radcliffe's will, at the House of Lords, 27th ultimo, Dr. David Badham was elected to the Radcliffe travelling fellowship from the University of Oxford.

THE NEW "FELLOW."

Dr. Bright has been appointed a Fellow of the College of Physicians, according to the annual custom of selecting one of the Licenciates for that honour. Sir Henry Halford, with whom, as President, the nomination rests, has in this, as in the preceding instances, made an unexceptionable choice, and one in the propriety of which the profession at large must entirely concur, as we are sure they will agree with us in thinking that the selections have been uniformly most skilful.

REPORT OF CHOLERA, UP TO FRIDAY, JULY 13, 1832.

New cases in Great Britain (exclusive of London) since June 24, the date of our last report	2832
Deaths.....	616
Total number of cases throughout Great Britain (inclusive of London) since the commencement ...	20197
Deaths.....	7929

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

July 1832.	Thermometer.		Barometer.	
Thursday . 5	from 52 to	74	29·95	Stat.
Friday. . . 6	51	72	29·89 to	29·76
Saturday . 7	45	71	29·73	29·84
Sunday . . 8	40	71	29·86	29·81
Monday . . 9	50	73	29·83	Stat.
Tuesday . 10	55	73	29·84	29·87
Wednesday 11	55	77	29·71	29·67

Prevailing wind S.W.

Except the 5th and 10th, generally cloudy; showers at times.

Rain fallen ·05 of an inch.

Charles Henry Adams.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 21, 1832.

SUBSTANCE

OF THE

LUMLEIAN LECTURES,

Read before the Royal College of Physicians,

By Dr. Watson.

May 1832.

Lecture III.

Cutaneous Hæmorrhage—Purpura.

The circumstances under which bleeding is apt to occur from the mucous surface of the urinary organs, were considered in the last lecture. Another part of the same continuous membrane, in the female. is also the frequent source or seat of hæmorrhage. Dr. Watson said he could do no more than thus *mention* the subject of *uterine* hæmorrhage. Not that he omitted it as being a subject of less importance, or of less interest, than those of which he had already spoken or had yet to speak; but because its extent made it impossible for him to do it any thing like justice in these lectures. Some of the most serious varieties of uterine hæmorrhage were, moreover, so essentially connected with the processes of gestation and child-birth, as scarcely to admit of a separate consideration.

Dr. Watson proceeded, therefore, following the order of parts enumerated in his first lecture, to those hæmorrhages which belong to the skin. Cutaneous hæmorrhages are certainly rare, yet they have been observed sufficiently often to remove all doubt as to their occasional occurrence from the minds even of those who may not have witnessed them. Although they possess no great degree of pathological importance, they are curious, and in some respects interesting.

Sometimes cutaneous hæmorrhage assumes the appearance of a sort of bloody perspiration, exuding from the entire surface of the body. One of the kings of France (Charles the Ninth) is recorded to have exhibited this phenomenon during the last moments of his life. The blood is described as having oozed out at all the pores of his skin.

More commonly, cutaneous hæmorrhage is partial, or local; and then it may either proceed from the cicatrized surface of former sores, or from a portion of skin which presents no discoverable alteration of texture or appearance. The face, the fore-part of the chest, the region of the liver, the fingers, the toes, the palms of the hands, the soles of the feet—all these parts have occasionally been known to be the seat of this kind of hæmorrhage.

Cutaneous hæmorrhage has happened at all ages and in both sexes, but by far the most frequently in the female. It is, in fact, in hysterical girls that these hæmorrhagic freaks are principally met with. Usually the bleeding takes the place of some habitual hæmorrhage. In some cases it seems to have been the result of a violent bodily effort. It has also occurred immediately after strong mental emotion—fright, for example.

In the majority of cases the following are the phenomena observed:—The surface of the skin becomes covered, in the part affected, by a dew of blood; if this be wiped away, no unnatural appearance of the skin is perceptible, but the blood presently exudes afresh. Sometimes, though no alteration of *texture* can be seen, the *colour* of the skin undergoes some modification. In a case related by Dr. Whytt, the hæmorrhage took place from the extremity of the middle finger of the left hand, and was preceded and accompanied by a spot of redness, and by slight pain.

As these bleedings from the skin are, in most instances, supplemental of some other habitual hæmorrhage, so they are generally transient, the blood soon resuming the less singular channel from which for a time it had deviated. In certain cases, however, cutaneous hæmorrhage is said to have occurred monthly, for a long time together, observing

the regular period of the catamenia, of which it was vicarious. Examples of periodical hæmorrhage from the skin have also been recorded as having happened in the male subject. Mayer gives a case where this kind of hæmorrhage recurred *yearly*, about the time of the vernal equinox, from the arm; the exudation of blood was then capable of being determined by the mere contraction of the muscles of the part. It would even appear, from a curious history given by Dr. Musgrave, in the Philosophical Transactions, that hæmorrhage from the surface may take place in the male sex periodically, to a considerable amount, and possess moreover that distinguishing character of constitutional hæmorrhage, of leading to a marked derangement of the general health, if its stated returns be any way prevented or interfered with. In this case the hæmorrhage was something more than a mere oozing from the skin*.

To this head of cutaneous hæmorrhage may, perhaps, be referred two very remarkable cases, differing greatly from those hitherto spoken of, which fell under the personal observation of M. Rostan, in which the skin became altogether, and almost suddenly, black. The subjects of these observations were both of them females; and the immediate cause of the change of colour seemed in each case to have been strong mental agitation—intense terror. In one of these women the blackness came on in the course of one night. She had seen her daughter throw herself, with two infant children, from a window into the street. The other woman, amid the horrors of the first revolution in France, narrowly escaped decapitation by the guillotine; the intelligence that she had been redeemed from this expected death reached her while the catamenial discharge was present; menstruation stopped all at once, and from being a fair woman she became, says Rostan, as black as a negress, and remained so till her death. In both these cases, Rostan had the opportunity of examining the body after death: he states, that he carefully dissected the skin, and found that the change of colour had been produced in the rete mucosum; and he concludes that it depended upon a sort of bloody exhalation into, or ecchymosis of, that tissue. The cuticle and the true skin were easily separated, and the colour of neither of them was altered.

The only remaining kind of hæmorrhage which the lecturer proposed to notice was one of a very peculiar nature, and more general in its effects than any that had hitherto been considered. It includes, indeed, as far as relates to *situation*, not only the several local hæmorrhages already mentioned, but every form of hæmorrhage of which the body is capable; and it is marked, for the most part, by those characters which were formerly described as belonging to passive hæmorrhage. He spoke of that complaint best known by the appellation of purpura, and usually, though very incorrectly, classed among cutaneous disorders, apparently because its most obvious tokens are those which are visible upon the surface of the body. That it cannot be regarded as a cutaneous disease, even in the loose sense in which that term is sometimes applied to affections that are really *beneath* the skin, though perceptible through it, is evident from this—that the effusion of blood (which, strictly a hæmorrhage in all parts, takes the form of red or purple spots where the quantity poured out is but a drop) is not confined to the skin, nor to the subcutaneous tissues, but is observed, occasionally, on all the internal surfaces also, and in the parenchymatous substance of the several viscera. Dr. Watson had seen these spots of purpura on the mucous surface of the mouth, the throat, the stomach, and the intestines; on the pleuræ and pericardium in the chest; on the peritoneal investment of the several viscera of the abdomen; in the substance of the muscles; and even upon the membranes of the brain; and in the sheaths of the larger nerves; and he had known them to be accompanied with large extravasations of blood, in most of the vital organs of the body. The disease, therefore, is properly a hæmorrhage, but it is not properly, or merely, a cutaneous hæmorrhage. Though it has always excited much interest, and furnished a fruitful subject of speculation among physicians, its pathology is yet but imperfectly understood, and various opinions are held, and some real difficulties exist, in regard to its most appropriate mode of cure.

The external phenomena of the disease, from which it derives its name, are so well known as to render any formal description of them unnecessary. They depend, as the simple division of the skin by a scalpel will shew, upon the presence of a small portion of extravasated blood, sometimes in the subcutaneous cellular membrane, sometimes pervading, more or less completely, the texture of the cutis vera itself; and sometimes, though less frequently, intervening between it and the cuticle, and sensibly elevating the latter above the level of the neighbouring surface.

Various hypotheses have been broached, with the view of explaining the immediate cause of these multiplied effusions of blood. Thus they have been ascribed to an increased action of the heart and arteries, overcoming the natural resistance of the extreme vessels in their healthy state—an opinion which is quite untenable; 1st, because purpura frequently, more frequently indeed than otherwise, goes along with a totally opposite state

* Phil. Trans. Abr. Vol. v. Part 1. p. 351.

of the circulation—is accompanied by a feeble action, and diminished force in the heart and arteries; and, 2dly, because in cases where the impetus of the circulation reaches its highest pitch, as in certain inflammations, the peculiar phenomena of purpura do not occur.

Again, the hæmorrhage has been attributed to a want of tone—to an unnatural degree of passive dilatation of the extreme vessels themselves, so that they allow a passage to the red blood, which, so long as they are in a healthy condition, they refuse to admit. Against this hypothesis we have the negative presumption that—supposing the channels of the hæmorrhage to be those outlets which we call exhalants—no evidence is furnished of their extraordinary patency by any extraordinary escape of their proper fluids, which might be expected if this hypothesis were true.

Another supposition is, that the coats of the minute vessels themselves, somehow or other lose their consistence, become tender and fragile, and unable to sustain the ordinary impetus of the healthy blood. This notion carries with it at first sight a greater degree of probability than the last, for it is concordant with the well-known fact, that in many cases of purpura slight pressure upon the skin is soon followed by the appearance of a bruise, or by actual ecchymosis, a consequence, it might seem, of the breaking down of the fine vascular texture of the part upon which the pressure was made.

But of all the solutions that have been offered with the view of accounting for the escape of the blood from its containing vessels in purpura, that appears to be the most simple, and the most probable, which ascribes it to some morbid alteration in the blood itself. This supposition is not inconsistent with any of the observed phenomena of the disease, and it seems the only one which is capable of explaining them all; and (what is strongly confirmatory of its truth) the blood in many, perhaps in all instances of the disease, in which it can be examined, is found actually to have undergone a change, and not merely a change which may be ascertained by nice or elaborate chemical research, but such an alteration of its sensible qualities as is evident to the eye, and forces itself upon our notice.

There are some persons who think that with this change in the composition of the blood, there is combined a diminution of cohesion and resistance in the coats of the minute blood-vessels; that these two circumstances often co-exist, and that the one has been the cause of the other; but as to which is to be regarded as the cause, and which the effect, upon this point also opinion is divided. It is very conceivable that the altered blood may exert a morbid and destructive influence upon the ultimate ramifications of the sanguiferous system, rendering them weak and frangible; and the spongy and tender state of the gums, so constantly a symptom of the disease, may be thought to afford a presumption in favour of this idea. On the other hand, it is easy to imagine that some derangement (of the nature of which, supposing it to exist at all, we know nothing) in that part of the circulating system which is strictly capillary, and in which those changes take place whereby the blood, from being scarlet or arterial, becomes purple or venous—it is very supposeable that some morbid condition of the apparatus in which so remarkable a change is effected, should modify or interfere with that change itself, and so come to affect the quality of the blood.

But, however this may be, it is certain that in this disease the entire mass of the blood undergoes a sensible alteration. And if we may consider, as the close resemblance between their symptoms so strongly persuades, that purpura hæmorrhagica and sea scurvy are essentially the same disease, we shall be prepared to go a step further, and to admit that in purpura, as it may almost be proved to be the case in scurvy, the blood is *primarily* diseased, and that this primitive alteration in that fluid is the cause of all the phenomena which constitute the nosological description of the complaint. Now the chief grounds which have been assigned for making a distinction between the two diseases are, that purpura often shews itself when there is no apparent cause for its production, and does not always yield to that mode of medication which experience has proved to be specific in the cure of scurvy. But, on the other hand, a similar condition of the blood may easily be conceived to be produced by causes that altogether escape our limited means of observation; and these causes, not being detected, are not so likely to be obviated or avoided, and therefore may persist and baffle those measures which are known to be efficacious, under more favourable circumstances, in restoring the blood to a healthy condition. And when to these considerations are added the strong facts, that purpura does *often* result from causes precisely similar in their nature to those which generate sea-scurvy, and does often also yield at once to the remedies which cure it, the general identity of the two forms of disease seems sufficiently complete to justify our looking upon them as both proceeding from a primitive morbid condition of the blood; a consideration which invests these disorders with a peculiar degree of interest.

Every one knows that the older physiologists attributed most of the diseases to which the human frame is obnoxious, to some change in its humours; and this fundamental notion, branched out into so many wild and absurd theories, led to so many explanations

of disease which a little more knowledge shewed to be absolutely false, and gave rise to so many plans of treatment that were pernicious and even fatal, that what was called the humeral pathology fell at length into entire and undeserved neglect. Succeeding observers passed, as is common, into the opposite extreme—ascribed all disorders to some change in the solid constituents of the body, and overlooked altogether the morbid conditions of which the fluids, either as the effects or the causes of disease, are unquestionably sometimes the seat. At present the attention of pathologists is again begining to direct itself towards this most interesting, though difficult line of inquiry. It is sufficient to mention the writings of Dr. Prout, and the more recent speculations of Dr. Stevens, in regard to the state of the blood in the yellow fever, and the simple means by which that state may be remedied, to shew that researches of this kind promise to be as useful as they are curious and interesting; and Dr. Watson stated his entire concurrence in the opinion recently expressed from the same place by Dr. Roupell—that it is to a more exact acquaintance with the chemical changes to which the animal fluids are subject in disease, that we are chiefly to look for the future advancement of physic as a science.

If we contemplate for a moment the intimate relation in which the blood and the solids of the body stand to each other as parts of the same whole, we shall find it scarcely possible to conceive how the one can undergo any material alteration without the production of a corresponding and proportional change in the other. We know that the continued circulation of arterial blood is essential to the life of every part of the frame; that by means of the blood those particles which are useless, or superfluous, or worn out, are taken away, and new particles are laid down in the ceaseless process of growth, or of sustentation, or of repair, which the constitution and economy of the animal machine requires. It is impossible, therefore, not to perceive that the blood itself is liable to foreign admixture by diseases that primarily affect the solids. This admixture, indeed, does not *necessarily* imply any morbid alteration of the blood, for there are many extraneous substances which that fluid may receive and convey without any marked effect upon its sensible or vital qualities. The mere addition, whether from within or without, of materials which do not alter the composition, nor, so to speak, affect the structure of the blood, may have no hurtful consequences. On the other hand, some of the changes that may be induced can scarcely fail to exercise an injurious influence upon the several tissues which are built up by the blood: it is thus, as was observed before, that disease of the blood may come to render weak and brittle the vessels which contain it; it is thus that we can best conceive and explain the existence of certain *general* diseases as distinct from those which are, more or less strictly, local.

One source, then, by which the blood evidently may, and no doubt often does, become *secondarily* diseased, lies in its function of being the recipient of certain portions of the solid textures of the body, themselves in a morbid state. It is, perhaps, to this that the appearance of petechiæ, in the *advanced stage* of some fevers, is to be referred. But we can see many ways in which the blood may also become *primitively* the seat of disease, and then convey its baneful influence to every part of the system. It is necessary that the blood be brought into frequent contact with atmospheric air; and this is accomplished in the lungs, through the function of respiration. Now if the air which is breathed be itself loaded with morbific principles, it is reasonable to expect, *à priori*, that the blood will suffer from that impurity. Still more certain is the injurious effect of a continual use of unwholesome food upon the composition of the blood, which comes, in fact, to be a slow kind of poisoning. Experiments on animals have taught us, that certain substances introduced directly into the blood will destroy life very quickly; and *that*, sometimes, in virtue of their effects upon the structure of the blood itself. And a change, analogous in its nature, though different in rapidity and in kind, will result from the continued ingestion of alimentary matters, which do not afford the necessary pabulum for the blood, or which yield deleterious materials. If the chyme and chyle be, from such causes, faulty or imperfect, so necessarily will the blood. And it seems to be in this way that scurvy is produced.

But that the blood is liable to such changes as may be said to constitute disease in it, we know, not merely by conjecture of the processes by which it may become so changed, but by unquestionable evidence afforded by that fluid itself.

We are convinced, in the first place, that the blood is susceptible of alterations which seem to be inconsistent with full health, by the observation of certain changes in its sensible qualities, when it is drawn from its proper vessels. Sometimes these changes relate to the proportion of the separate gross parts of which it may be said to be composed. The fibrin may be in excess when compared with the serum, the serum when compared with the crassamentum; or the relative quantities of water and albumen, of which the serum itself is formed, may be so deranged as to cause an alteration in its consistence perceptible by the sense of touch, when that fluid is rubbed between the finger and thumb. Changes also occur in the degree and manner of the act of coagulation itself; evincing

a modification of the *vital properties* of the blood: it may coagulate quickly and strongly, or slowly and imperfectly; or it may not coagulate at all. The blood may even assume the solid form whilst contained in its vessels in the living body; either by spontaneous coagulation, from an approach to a state of quiescence; or in consequence of disease or irritation of the vessels themselves: and this will give rise to new, and sometimes fatal symptoms. Of all this, numerous illustrations might be given.

A still more convincing proof of a diseased state of the blood, or of a departure from the normal conditions of health, is afforded by chemical analysis: shewing us something more than a varying proportion between its *proximate* constituents, or an increase or diminution of its vital properties; proving, in short, a great change in its very composition. The most recent, and accessible, and satisfactory illustration to which Dr. Watson could refer, of these chemical changes in the blood while it is yet circulating in the body, was to be found in Dr. O'Shaughnessy's able analysis of blood taken from persons who were labouring under the epidemic cholera. He proves that in that disease there is not only a great deficiency of the water of the blood, but a remarkable diminution also of all its usual saline ingredients, and a total disappearance of some of them.

But, perhaps, the most striking and decided manifestation of disease affecting the blood, is seen in its acquisition of new and deleterious properties, which render it a poison to others, as well as to the subject that contains it.

Very numerous experiments have shewn that blood may be directly transferred, without any injurious consequence, from the vessels of one healthy body into those of another; and the knowledge of this fact has led to the operation of transfusion, with the event of saving lives which were in peril from the effects of accidental hæmorrhage. There is abundant evidence, however, that the blood, under disease, cannot be thus safely transfused, but becomes, in many instances, an active poison.

M. Gendrin, in a work on Fever, gives an account of a patient of his, whose employment was that of flaying hides, and who laboured under putrid fever, with gangrenous pustules. An ounce of this man's blood, taken from a vein, was injected into the cellular tissue in the groin of a cat. This was followed by copious vomiting of green and yellow bile; by dyspnœa; frequency, irregularity, and feebleness of the pulse; dryness and blackness of the tongue; increasing debility; slight, occasional convulsions; and the death of the animal in about seven hours. Among other morbid appearances, the blood in the vessels was found to be every where black and fluid; the lungs were sprinkled over with brownish black spots; and two ounces of thin dark-coloured blood were effused into the cavity of the left pleura. Similar symptoms and appearances resulted when some blood, which had flowed spontaneously from the nose of the same patient, was introduced into the crural vein of a dog.

There is a very curious case, still better illustrating the same point, related by Duhamel. He states that an over-driven bullock was slaughtered at a certain inn. The butcher placed in his mouth, for a few seconds only, the knife with which he had just cut the animal's throat. A few hours afterwards his tongue began to swell; he breathed with difficulty; blackish pustules appeared all over his body; and in four days he died. The innkeeper happened to scratch the palm of his hand with one of the bones of the same bullock. Mortification of the arm, and death at the end of seven days, were the consequences. Some drops of the animal's blood had fallen upon two women who were bystanders—upon the hand of the one, and upon the cheek of the other; and these parts respectively became affected with gangrenous inflammation*.

The severe constitutional disorder, too often fatal to members of our profession, and known sometimes, and sometimes only, to follow wounds made during dissection, supplies farther evidence of the poisonous nature of some of the animal fluids under disease: it is probably the *diseased blood* which in such cases acts as a poison. This becomes the more likely when we consider, that the danger is the greatest the sooner after death the inoculation takes place. The noxious qualities seem to be destroyed or diminished by the process of decomposition. It is seldom that butchers suffer from similar accidents; apparently because they seldom are employed upon animals which are otherwise than healthy.

Now with regard to purpura—certainly with regard to that modification of it which constitutes sea-scurvy—we have two, at least, of those presumptive evidences of a diseased state of the blood, which have just been considered. We see that disease break out in great bodies of men who have long been restricted to a peculiar and unnatural kind of food; we see them remain exempt from it when, though similarly situated in all other conceivable respects, this supposed cause of the disease is corrected; and we see them cured by a particular article of diet. Besides this we find, whether in scurvy strictly so called, or in the other forms of purpura, that the blood is actually of unusual appearance and consistence. This cannot be better described than in the words of Huxham, who has given a most faithful and

* Vid. Andral, Anat. Pathol.

vivid account of the phenomena belonging to such complaints. "The blood of such persons," says he, "when it hath been drawn off, always appears a mere gore as it were, not separating into crassamentum and serum as usual, but remaining in a uniform, half coagulated mass, generally of a livid or darker colour than usual, though sometimes it continues long very florid; but it always putrefies very soon." In another place, when describing a particular case, he says, "I found that neither of the portions of the blood that had been drawn had separated into crassamentum and serum as usual, though it had stood many hours; but continued, as it were, half coagulated, and of a bluish livid colour on the top; it was most easily divided by the slightest touch, and seemed a purulent series rather than blood, with a kind of sooty powder at bottom."

But enough, the lecturer observed, had been said in illustration of the presumed dependence of the phenomena of purpura upon a primitive diseased condition of the blood.

Another circumstance that gives to the disease a high degree of interest, in practice, is the great and peculiar danger which it comprehends. Dr. Watson spoke not merely of the danger arising from the general disturbance of all the functions by the circulation of unhealthy blood, nor of the possible exhaustion of the living powers resulting from an excessive loss of that fluid, but of the hazard that blood may be poured out in some important or vital organ, where even a slight amount of hæmorrhage may be sufficient to extinguish life. Dr. Bateman, adverting to this source of danger, states that he had seen three instances in which persons were carried off, while affected with purpura, by hæmorrhage into the lungs. During the course of one week, in the year 1825, Dr. Watson was present at two dissections, in St. Bartholomew's Hospital, illustrative of the same point in regard to another vital organ. The subjects of examination were both of them women of middle age, who had been brought into the hospital covered with purple spots and bruise-like blotches, and suffering hæmorrhage from the mucous membranes. One of these patients became suddenly hemiplegic a little while before she died: of the manner of dissolution in the other case, he was not aware. In both instances, a considerable quantity of blood was found spread over the surface of the brain between its membranes, and in one of them blood had been effused also into the cerebral substance, which it had extensively lacerated.

It might be worth mentioning, too, that in one of these cases there was evidence either of unusually rapid putrefaction after death, or, what seems more probable, of some degree of decomposition of the blood before life had ended. This woman died in the evening, and the body was examined the next day, twelve or fourteen hours afterwards. A quantity of fœtid gas escaped from the cavity of the abdomen as soon as it was opened, and bubbles of air were seen to ooze from the cellular tissue of various parts of the body. Even when incisions were made into the liver, the air frothed up, as it might do under ordinary circumstances from a section of the lungs. The blood was found fluid in the larger veins, and of a dirty, sanious, dissolved appearance.

There is yet a further consideration which adds to the interest belonging to the disease, viz. the great and somewhat perplexing difference of character under which it declares itself in different cases. Sometimes the state of the patient, and the nature of the injurious influences to which he has been exposed, are so distinctly referable to debility and its causes, that we cannot hesitate about the principles on which the cure is to be attempted. Sometimes, on the other hand, there is so much arterial action, or such a degree of hardness of the pulse, or of fever, as to suggest the propriety of active depletion, and especially of bleeding; and the result of this practice justifies its adoption. Between these well-marked extremes there are many insensible gradations, so that it becomes, in some of those cases which lie midway, an extremely difficult matter to determine upon the proper indications of treatment.

Dr. Watson here gave the particulars of a severe case of purpura, in which the patient was treated with full diet, bark, and a large allowance of fresh lemon-juice. The case may be found briefly reported in our seventh volume, page 128.

The amendment here was so decided and rapid, and began so immediately after the institution of the treatment, that there was scarcely room in this instance for mistaking a recovery for a cure.

Cases of the opposite kind, cured (apparently) by abstinence, venæsection, and purgation, are far from being unfrequent. The late Dr. Parry, of Bath, was one of the first to inculcate this plan of treatment. The lecturer referred to a good example of this kind, which occurred in Dr. Latham's practice, and is detailed in this Journal.—Vide vol. i. p. 544.

Now although these differences in the character of the disease are worthy of all attention, in so far as regards corresponding modifications of the treatment, they do not necessarily impugn the doctrine that the disease resides principally and primarily in the blood.

In the first place, inflammation may casually coincide with the morbid state of the blood which is supposed to give rise to purpura, and mix up, with the symptoms of that disease, its own peculiar features, and

require its appropriate treatment; or the state of the blood may be sufficient, especially when there pre-exists a tendency to local disease—to *cause* inflammation in some part or other. We know that traces of recent acute inflammation have actually been found in the interior of animals that were killed by poisoning their blood. Dr. Watson had seen acute inflammation of the peritoneum accompany purpura, and destroy the patient in spite of large depletion; but whether the peritonitis was accidentally combined with the purpura, or whether it was somehow a consequence of that disease, he did not know.

Besides, the very presence of the blood in its vessels, or in the tissues into which it is effused, when it deviates much from its ordinary and healthy composition, or when the system is naturally plethoric, or naturally irritable, may occasion so much general disturbance and excitement as will require bleeding for its alleviation. And it might not, perhaps, be considered a very wild theory, to suppose that in some cases it may be proper and judicious to combine blood-letting with the use of a full nutritious diet, upon the principle of removing as much of the morbid blood as consists with the patient's safety, and at the same time taking measures to repair the loss by the introduction of more wholesome materials.

Having unavoidably touched upon the treatment of this particular disease, which differs from all the other varieties of hæmorrhage, Dr. Watson thought it right to state that free purgation has been strongly recommended by Dr. Harty, of Dublin, as having proved eminently successful in his practice in the cure of purpura. Dr. Watson remembers to have seen a young man in whom calomel, given in frequent and full doses, produced at once salivation and hypercatharsis; about twenty copious watery dejections were passed during the course of one night; and from that time he ceased to bleed from the nose; and the purple spots with which his body had been profusely covered, began rapidly to vanish. Whether these means sometimes operate in the manner just alluded to, viz. by abstracting from the blood its noxious materials, or whether the specific effect of the mercury was curative here, we have not yet sufficient data for determining. Dr. Whitlock Nichol has spoken in terms of high praise of the oil of turpentine, administered in moderate and repeated doses, as a remedy in this disease. The same medicine has often been found useful in hæmorrhage from the alimentary canal in general.

Upon the whole, however, the practice in purpura should be regulated, in the lecturer's opinion, by the symptoms with which the appearances on the skin, and the other marks of a dissolved state of the blood are combined, rather than by those appearances themselves.

After briefly discussing the question, whether it be proper in all cases of hæmorrhage to attempt to stop the bleeding, Dr. Watson concluded this course of lectures by a very rapid estimate of the means which are found most effectual in restraining the actual efflux of the blood, including the position in which the patient should be placed, blood-letting, the application of cold, and the internal exhibition of various astringent substances.

[In the preceding lecture, p. 467, lines 14, 28, and 45, *for* "splanenic," *read* "splanchnic."]

AN ACCOUNT OF SOME OF THE PREPARATIONS IN THE HUNTERIAN MUSEUM,

Which illustrate the Structure of the Human Placenta.

By John Burns, M.D. F.R.S.
Regius Professor of Surgery in the University of Glasgow.

Bichat considers the placenta as made up chiefly by the ramifications of the fœtal vessels, traversed by white filaments, which he believed to be obliterated vessels. The uterine vessels, he says, rarely pass in considerable size into the placenta, but he thought that they did traverse the decidua, and open in the lobular interstices of the surface of the placenta, where a communication was effected with the fœtal vessels. This opinion was so far different from that of Dr. Hunter, as to deny the universality of cells throughout the placenta, and confine their existence to its uterine surface; for the spots where the small branches of the uterine vessels opened, could be considered in no other light than that of cells. In a paper lately published, by Dr. Lee, in the Philosophical Transactions, it is maintained "that a cellular structure does not exist in the placenta, and that there is no connexion between this organ and the uterus by great arteries and veins;" indeed, that the decidua alone receives vessels from the uterus, and these small; "nor has the appearance of the orifice of a vessel been discovered, even with the help of a magnifier, on the uterine surface of the placenta." These opinions the author supports by an appeal

to preparations made by Dr. Hunter himself, and still preserved in his museum in this university. I will therefore give a short account of some of those which appear to confirm the Hunterian description; I only premise, that I believe the communicating vessels to vary, both in size and firmness, at different periods of gestation. It is also evident, that a prolongation of the uterine vessels into the placenta cannot be accomplished by a continuation of the ordinary texture of the vessel, but by the interposition or intermedium of a circle or portion, not firmer in its fabric than the decidua, otherwise the secundines never could be thrown off.

The preparations are contained in the department marked R.R.

No. 180 is a uterus about the fourth month of pregnancy. The posterior half is cut off, and the fœtus, with the fœtal portion of the placenta, removed. The substance of the uterus is well injected. The arterial mouths which project from the inner surface, all the way from the fundus to the commencement of the cervix, are most distinct, and as large as stocking wires. The veins are much more numerous than the arteries, some of them as large as goose-quills; irregular in some places in their appearance; in others their orifices are well circumscribed, and the coats at the broken termination very thin.

No. 27 is a gravid uterus, well injected and then inverted, so as to shew the vessels opening on the inner surface; the secundines being removed. The circular orifices of both arteries and veins are very numerous, and most distinct.

No. 137 shews the decidua very distinctly injected from the vessels of the uterus.

No. 176 is a section of the uterus and placenta. The cells of the placenta are injected with red wax, from the vessels of the uterus. Almost the whole thickness of the placenta is penetrated, and the wax evidently is not extravasated.

No. 21 is a very important preparation. The uterus at the sixth month is injected and cut open, but the placenta is every where left adhering. No vessels therefore can be seen passing from the uterus into it. But that the red injection has entered freely, and filled the cells, is proved by the colour being finely visible on the fœtal surface of the placenta. The decidua is also red. It is mentioned in the catalogue, that the red injection, thrown in by the hypogastric artery of the mother, returned by her veins. The umbilical cord was afterwards injected with white and green wax, so that the ramification of the fœtal vessels are seen on the surface of the placenta.

No. 124 is described as a small portion of the placenta and uterus, where the cells of the placenta have been filled from the vessels of the uterus. The fœtal portion is not injected. The placenta is detached from the uterus, and hangs down. The cells are filled with red, and amidst them we see cut portions of green. No injected vessel, indeed, is seen passing into the uterine surface of the placenta; but there are several bristles put into unfilled orifices on that surface.

No. 125 is another section of the same. There is an orifice of considerable size on the inner surface of the uterus, and another corresponding to it on the uterine surface of the placenta, with a bristle passing from the one to the other. The decidua is well seen extending on the uterine surface of the placenta.

No. 167, a section of the placenta, with its cells filled with black and red. The decidua covers the uterine surface of the placenta. In some places the wax is irregular, and might therefore be considered as having been extravasated; but in other parts, more especially toward one side, the entrance or passage of vessels through the decidua is very distinctly seen.

No. 158, a portion of the uterus and placenta, with the veins injected green. The placenta is partly raised and turned aside. The orifices of the veins on the inner surface of the uterus are distinct; those on the placenta are more irregular, but still satisfactory.

No. 92 is a section of a uterus which was ruptured in the cervix: the side of the placenta had been attached over the os uteri. The rest of it adhered higher, and part of that is raised up so as distinctly to shew orifices on the surfaces of the uterus and placenta corresponding to each other.

I do not think it necessary to multiply illustrations, and it is not my object here to enter into any physiological inquiry.

ON THE

NERVES OF THE EAR.

By Henry Jones Shrapnell,

Surgeon to the Royal South Gloucester Regiment of Light Infantry Militia, Member of the Royal College of Surgeons, and Fellow of the Geological Society of London.

Reflecting upon the course of the portio dura of the seventh pair of cerebral nerves, through the internal auditory foramen, to join the nervus innominatus, and to pass with that nerve to the canal of Fallopius, and from thence across the tympanum to reach the face; it seems that the mere circumstance of imparting a few minute filaments to the cavity of the tympanum, is scarcely a sufficient reason for this extended distribution. Had the portio dura continued from the internal auditory foramen immediately backwards and downwards in front of the sinus venosus, it might have reached the face through the foramen stylo-mastoideum in one half of the distance which it now takes, and might have been equally near to the tympanum, for the purpose of imparting filaments to that cavity. The mode in which this nerve is distributed is found invariably in the mammalia—at least in all which I have had an opportunity of examining; from whence it is evident that some particular purpose is effected by it, independent of merely imparting filaments to the tympanum. It is not, therefore, unreasonable to expect some design to be answered within the meatus itself, previous to the entrance of the portio dura to the canal of Fallopius.

On removing the upper part of the internal meatus auditorius, and continuing the section from the bottom of the meatus to the commencement of the canal of Fallopius, so as to expose the passage for the portio dura, the following appearances may be observed. First, the entrance through which the portio dura passes, from the bottom of the external meatus: this opening is so small as scarcely to admit a middle sized pin: from which circumstance I conclude it to be impossible for every filament, from the extended and multitudinous distribution of the portio dura, to find its way through this minute opening; for if they did, we might expect to find the nerve at this point hard, as its name implies, and much condensed; whereas, it is as soft as any of its numerous branches on the face. Again, if it were possible to imagine distinct threads, or filaments, so infinitely minute as to pass through a foramen which scarcely admits a common-sized pin, from the nose, forehead, chin, face, ear, head, &c.; in short, from all the branches constituting the distribution named the pes anserinus, we should expect that they would not exhibit their present size on the face, which is not attributable to mere neurilema, but to abundant filaments of medullary matter. How is this fact to be reconciled with the theory, that distinct filaments or threads invariably pass to the seats of impression or perception from the extremities of sentient nerves; and, *vice versâ*, in the case of voluntary motor nerves? With all due deference to the high authorities from whence we have received this theory, I would ask, if it does not seem that the nervous influence, whatever it may be, must, in the instance of the portio dura at least, obey a law which is similar to the circulation of fluids, in which currents from the most extended distributions are capable of passing through one and the same main vessel? As our present knowledge of the nature of nervous influence allows us only the bare comparison of its nature to that of the transmission of electric fluids, it may be observed, that it agrees with our experience of the electric phenomena, that one minute rod may be made the common channel from many conductors; indeed, from an infinite number.

The next appearance of the bony canal for the portio dura is a considerable enlargement between its minute entrance, and its union with the canal for the nervus innominatus. In some temporal bones this enlargement equals the size of the largest of the ampullæ of the semicircular canal; and in others the foramen innominatum is seen to open at once into it, instead of forming a previous bony canal; in others, again, it is more elongated, and occupies the posterior side of the canal, next to the upper part of the vestibule, more than the anterior side, which is over the central entrance of the modiolus of the cochlea. Hence this situation of the

enlargement more on one side of the canal than on the other, produces an appearance as if the portio dura took its course forwards into the channel to the foramen innominatum, instead of backwards to the Fallopian canal. Another circumstance not unworthy of remark appears from the subjoined section of the internal meatus, which is precisely a similar enlargement, resembling an ampulla at the entrance of the great twig of the portio mollis to the vestibule. This enlargement, still on the upper portion, but at the bottom of the meatus, is posterior, and rather inferior to the minute orifice for the portio dura; it gives an appearance in the bone as if the portio dura itself continued directly to the vestibule. These openings for the dura and large twig of the mollis, are divided from the lower portion of the meatus by a ridge, or septum of bone, which is much more produced in some subjects than in others, and will even be extended some distance up the anterior part of the meatus, where it marks the passage of the portio dura. These appearances, so readily perceived in the dried temporal bone, we may reasonably expect, are formed for the reception of corresponding enlarged portions of the soft parts they are destined to enclose. Upon a careful dissection, I find these structures to be ganglia, one on the large twig of the portio mollis, communicating with the nervus innominatus, and one on the portio dura, communicating also with the nervus innominatus. That as the great twig of the mollis is situated in the ampulla before its entrance to the cribriform plate of the upper and anterior part of the vestibule. This twig is here distributed in two portions; the first passes through the pyramid, and then spreads upon an elastic tense membrane, stretched across the vestibule, anterior to the foramen ovale and scala of the vestibule: upon this membrane three filaments of nerve may be seen extending upwards, backwards, and downwards: the other portion of the great twig passes beyond the pyramidal plate, entering a cribriform plate directly posterior to the pyramid, at the superior part of the vestibule, and from thence passes to the ampullæ of the superior vertical, and inferior semicircular canals.

Of this elastic plate, or membrane, I must here, by the way, remark that I constantly find it without difficulty in the human subject, and in other mammalia; it has prolongations into the semicircular canals, upon which the nerves of the ampullæ seem to be finely distributed; it appears to be elastic, capable of tension, and has various fixed points in the vestibule. This membrane is substituted by the chalky and bony concretions met with in fishes and reptiles. I have not, however, yet completed a series of preparations which will enable me to make drawings, and to ascertain the nature of this structure beyond a doubt. I would, indeed, be almost tempted to speak positively now, but confess that I feel a deference to the opinions of so many eminent anatomists, who have all described the structures of the vestibule, according to the elaborate plates first published by Scarpa, and which have been handed down in a multitude of copies; but as I have never seen the sacculus, alveolus communis, &c. figured by Scarpa, I must say that I am sceptical in my belief of their existence in mammalia.

I have made several attempts to obtain a view of Scarpa's representations of the parts within the labyrinth, but have never been able to succeed in the happy manner in which he appears to have done; and I must own, however presumptuous it may appear, that if, in the place of the common sac (alveolus communis), we substitute the bag of chalky matter met with in reptiles, the whole of Scarpa's plates, excepting the distributions of the nerves of the gyrus cochleæ, appears more like what is readily met with in the vestibule of the turtle, for instance, than what I have been able to detect in the human subject. Upon putting the question to a gentleman, who has made several beautiful preparations of the minute anatomy of the eye and ear, he answered, "I confess I have never seen the parts described by Scarpa." To prove the reality of these delicate structures, a good microscopic eye is required, repeated preparations must be made, and in different orders of animals; and these must be elucidated by drawings and diagrams. Of these, I trust, I shall soon be able to accomplish a series. For the information of those who may like to promote this inquiry, I may here mention, that instead of a membrana fenestræ ovalis filling the foramen ovale,

I find no such membrane exists; but from the anterior centre of the oval base of the stapis, a filament or ligament passes across to the centre of the elastic membrane above-named; which filament, by the lever-like elevation and depression of that bone, stretches or relaxes the whole of the elastic membranes of the labyrinth. There appear also to be connexions with the ampulla of the scala vestibuli, and with the membranous state of the gyrus cochleæ; which plate commences close to the edge of the membrana fenestræ rotunda. Two or three small membranes also pass across from the round membrane to the gyrus, and probably are means of producing simultaneous actions with the rest of the labyrinth.

The perception of sounds, however, also depends upon an elaborate distribution of nerves within the tympanum. One of these nerves passes to the posterior arm of the stapes, and forms a minor chorda tympani; another passes through a membrane which fills the arms of the stapes, and, accompanied by an artery, passes to the promontory, to inosculate with the numerous nerves and vessels on that eminence. All the ossicula are capable of vibration, for which they are held together by elastic ligaments; and the chorda tympani on the posterior side of the neck of the malleus, passes upon a similar tense elastic ligament, very curiously constructed in some animals. In the rat, the nerve and artery which passes through the arms of the stapes is supported by a minute channel of bone, and the stapes cannot be removed with the other ossicula in that animal until this bridge of bone is broken away from its buttresses on the promontory and canal of Fallopius. This is the only instance I know of an isolated bony structure surrounded by another, and a moveable bone. I was led to look for this artery and nerve in the human ear, after seeing it in the rat, by a remark made to me by Sir Astley Cooper, (for whose kindness I am under many obligations)—namely, that "when you meet with new structures in animals, it is seldom that you do not find similar or equivalent structures in the human subject."

This brings me back to the nerves within the internal meatus. The second enlargement—namely, at the junction of the portio dura and nervus innominatus—is filled with a grey, very soft, vascular, pulpy ganglion. This I found in the calf; and the accompanying drawing is from my first dissection of it in the human ear. Some apology may appear necessary for remarks so desultory as the above, but I consider the great end of surgical and medical knowledge—the honey of which our profession is the hive—to be the cure or amelioration of the miseries consequent to disease; and the flowers from which this honey must be gathered, are the facts obtained from anatomical investigations. It is our duty, therefore, at once to impart any new facts, or even the suspicion of the existence of a flower from whence any may be gathered; for it is not one working bee that can bring all the honey to the hive, for it must be stored from the labours of the many. By these remarks, therefore, I would rather invite the attention of the profession to the anatomy and physiology of the ear, as a most ample field of research; and surely the late works of Sir Astley Cooper on the Testes and the Thymus Gland, ought to lead every young member of the profession to emulate his surprising industry.

How greatly the profession is indebted also to the brilliant discoveries of Sir Charles Bell, relative to the functions of the nerves; particularly that of the separate influences of voluntary and sentient nerves, and the important connexions in the associations of the nerves distributed to the organs of respiration. I am disposed, however, to think that many of the arguments used to establish the portio dura of the seventh, the third, and the sixth pair of nerves, as respiratory, are equally applicable to these nerves as connected with the organs of hearing. They necessarily and undoubtedly associate with respiration, so far as respiration is concerned in the production of the voice; but it is the ear which regulates all the modulations of the voice—which communicates the perception of sounds produced in singing—and to which articulate language is indebted as an essential medium by which ideas are communicated to the sensorium commune. In each of these instances the ear is primary, the respiration only secondary; speech is consequent to hearing. So also the

sympathies between the eye and the ear have a more intimate connexion with each other than either sense has with the organs of respiration. We instinctively or habitually turn the eye to the direction from whence sounds proceed; and it is unnecessary to enumerate here the variety of emotions perceived and felt from music, language, and an infinite multitude of sounds; all constituting the hearing as a primary function of relation between the external world and the centre of vital perception.

Let us now examine the connexion of the seventh and the fifth pairs of nerves to the sense of hearing, through the medium of the great sympathetic nerve. The seventh pair of nerves is very inappropriately divided into portio mollis and portio dura, which terms really mislead, as the part named dura is but very slightly harder than the portio mollis; while one part of the mollis, namely, that to the cochlea, is harder than the portio dura. The portio mollis, we may presume, is intended to receive the peculiar impressions of sounds, as the retina does those of light; like it, also, as M. Magendie states, it is insensible to pain on being wounded. The portio mollis is a compound nerve, having two distinct characters of filaments, one white and compact, its filaments being but slightly marked,—this passes solely to the cochlea; the other is reticulated, having loose filaments darker in colour and softer than the nerve to the cochlea,—this branch divides into three twigs, which enter the vestibule at separate cribriform plates.

The largest of these has upon it a gangliform enlargement, and supplies two apparently separate parts, namely, two ampullæ of the semicircular canals, and the elastic plate of the vestibule; the remaining two twigs are similarly disposed, the one to the vestibule, the other to two ampullæ;—one, the ampulla of the scala vestibuli, the other the ampulla of the posterior vertical semicircular canal. There is no doubt a specific purpose answered by these separate distributions and characters of nerve.

The large twig of the portio mollis vestibuli sends off a considerable filament, about midway in the internal meatus, which unites with the portio dura, just before its entrance to the minute bony canal: this filament spreads upwards and downwards, in a plexiform manner, but is traceable also to the ganglion at the union of the dura and nervus innominatus. Minute filaments also appear to pass from the dura to the gangliform enlargement of the large twig of the mollis, and one small twig appears to pass upwards from the innominatus to the same ganglion: see engraving. Thus, then, we find three distinct characters of nerve going to the vestibule and semicircular canals, and one to the cochlea.

The portio mollis, we presume, is for the direct impressions of sound. The portio dura—a nerve of motion, and according to Sir Charles Bell's theory, of respiration—is also evidently associated with that peculiar kind of motion of sounds named vibration, by the distinct nerve it receives from the portio mollis vestibuli, and which, from its very commencement, thus constitutes the portio dura a direct auditory nerve. The third character of nerve is derived from the union of the associated portio dura and portio mollis with the nervus innominatus, at the ganglion formed at the commencement of the canal of Fallopius above described. From this same ganglion a filament also passes to the gangliform enlargement of the great twig of the mollis, furnishing a similar triple character to the nerves in the vestibule. The nervus innominatus is a branch from the ganglion of Meckel, and associates the second of the trigeminus, a nerve of direct sensation and feeling. Its great influence in the perfection of hearing may be observed from various diseases, where this nerve being affected, the hearing becomes disordered. Also to this nerve, with the portio dura, we must attribute the exquisite sense of feeling of deaf and dumb persons, which, in conjunction with the eye, is a substitute for all but the actual sense of sound in hearing. Innumerable instances of the distributions of the ganglionic portions of the fifth pair of nerves, to the organs of hearing, seeing, &c., in man and various orders of animals, might be here adduced to prove the great importance, and close connexion, of the sympathetic actions of the senses.

19, Russell Street, Brixton-Road.

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Fig. I.

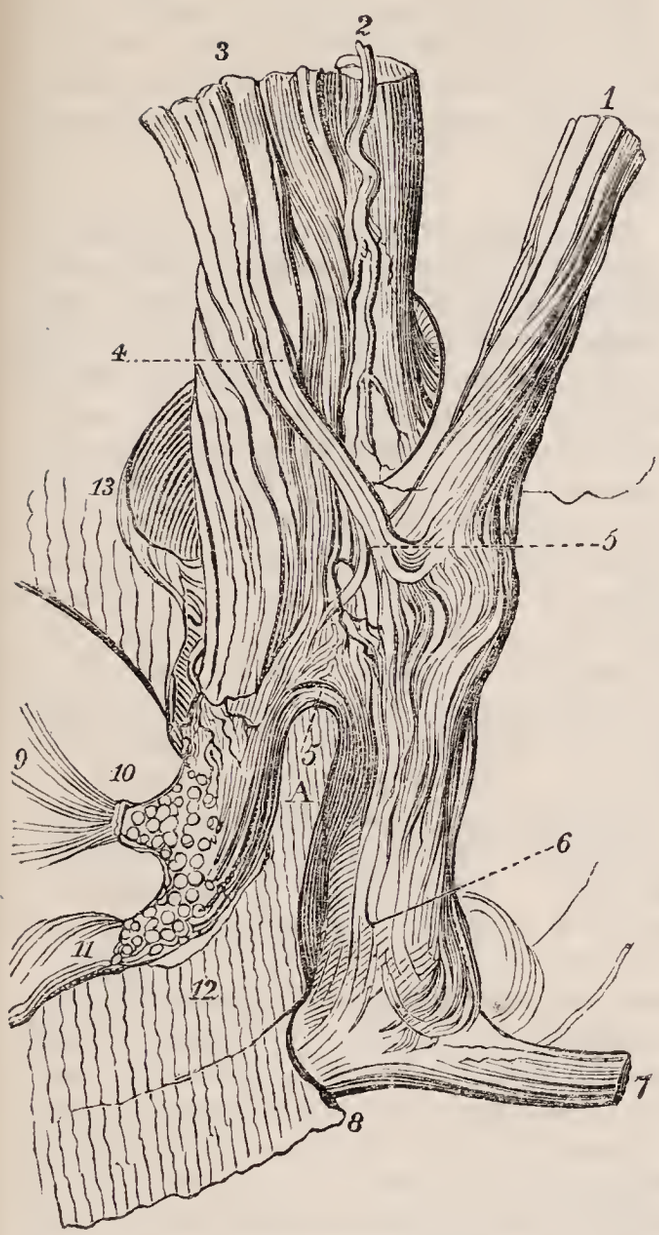

Fig. II.

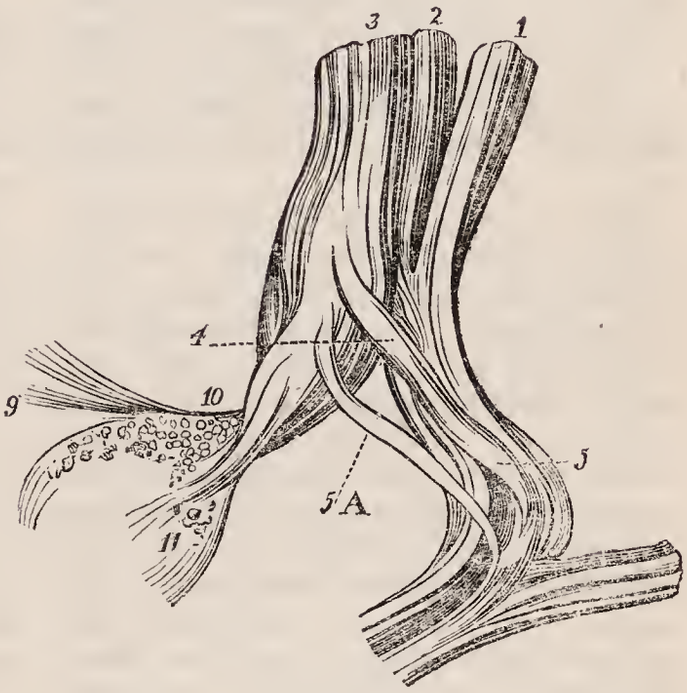

Fig. I.

1. Portio dura separated from the portio mollis.
2. Unfilamentous branch of the portio mollis to the cochlea, with the artery passing between it to the cochlea, &c.
3. Filamentous branch to the vestibule, of which the part forming the great twig only is seen in this view.
4. The filament given off to the portio dura.
5. Its mode of union with the portio dura, at a slight enlargement previous to the entrance of the dura to the bony canal.

5. A. Minute filament passing to the innominatus, more distinctly seen in fig. II.

6. Ganglion at the union of the portio dura and nervus innominatus; a great part of it has been washed away; it is more distinct in fig. II., and its centre is of a dark grey colour.
7. Nervus innominatus.
8. Entrance to the canal of Fallopius.
9. Nerves from the great twig of the portio mollis spreading on the elastic membrane of the vestibule, from the pyramid 10.
11. Cribriform plate through which the nerves pass to the approximated ampullæ of the superior vertical, and inferior semicircular canals.
12. The section of the bone between the vestibule and canal of Fallopius.
13. The bottom of the internal meatus.

Fig. II.

The same parts, as formed in the calf. The references the same as in fig. I., but magnified only half as much.

CANTHARIS IN HYDROPHOBIA—SALINES IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

ALLOW me to notice the following errata which occur in my remarks on salines in cholera, inserted in your No. for June 23. At page 379, line 17, *for* "life," *read* "health;" third line from the bottom, *after* "cantharides," *insert* " in hydrophobia, and."

This latter omission I am the more desirous of seeing supplied, as this remedy, when actively employed, even to hæmaturia, has been strongly recommended in that disease, our success in treating which is still far from satisfactory. Diuretics in general have been considered as alexipharmics, but apparently shared the fate of humeral pathology in general, and been lost sight of. The lithic acid containing azote, carbon, oxygen, and hydrogen, elements which, variously combined, form narcotin, the acrid principle of colchicum, digitalis, &c. alcohol, oxalic and prussic acid, and have, I believe, been found to constitute the base of animal poisons; it would not, *à priori*, be unreasonable to expect benefit in hydrophobia, and other diseases, where the blood seems contaminated, from remedies which produce an increased secretion of urine, and thereby remove those elements which have formed, or might form, deleterious compounds. The reputation some remedies have from time to time enjoyed in this disease (such as lichen cinereus, scutellaria, genista, &c.) may have been owing to their diuretic qualities; their failures, to the effects of diuretics being so uncertain. But whether the efficacy of cantharides be owing to their ejecting the poison—to simple counter-irritation—to their homœopathic virtue, since Paris, vol. ii. enumerates among the symptoms they produce, " sometimes universal convulsions, attended with a horror of liquids, resembling that which occurs in hydrophobia, furious delirium, &c." —or to a combination of these effects, I certainly think they deserve further trial, either by mouth, or injected into a vein with a considerable quantity of liquid, alone, or in conjunction with strychnine, mercurial inhalation, and the cold dash.

In the concluding paragraph of that paper I might also have instanced the use of iodine, pyrola umbellata, and alkalies, in scrofula, and the occasional benefit derived from tar water in various disorders.

As to saline injections in cholera, I should in preference employ, in more moderate quantities of menstruum, the nitrate, chlorate, and carbonate of potass, and carbonate of soda, as more apt to act on the kidneys than the muriate of soda. But why not at once administer powerful diuretics, such as tinct. ferr. mur. or cantharides (*not* digitalis), by mouth, aided by plentiful dilution? That these injections do not produce a change in the blood, or if they do so that it is to no purpose, becomes more and more evident from their failures, which will tend to render the profession less sanguine with respect to the practical improvements expected to result from Dr. Stevens's interesting investigations, which I confess have only appeared to me—by chemically, instead of functionally, accounting for the efficacy of non-purgative salts—to threaten the partial revival of the doctrines of the chemical physicians. Let us ever bear in mind, that the efforts of the living organism are continually directed towards resisting chemical agency from without; and that the chief aim of the physician, in treating diseases, ought to be either to produce a fresh supply of vital power, by tonics, &c., or to assist nature, both by provoking those changes of action she herself occasionally employs to get rid of disease, and by causing new, or similar affections, in the same or a distant part. Thus in cholera, instead of attributing the success of those salts in solution to their supplying the saline particles of the blood, which, after all, are chiefly destined for final removal by excretion, and which in this disease, instead of being directed to the skin and kidneys, are probably, by a remedial effort of nature, though in morbidly increased quantities, seeking a vicarious outlet by the alimentary canal—they rather appear to me to rouse the energy of the most important organs of secretion, the kidneys and skin, but especially the former, both whose inherent function it is to eliminate substances of that nature, and along with them, probably, other noxious elements,

whether the cause or effect of disease; the secretion of the liver appearing only of minor importance, since in milder cases of cholera it is not suppressed, but rather vicariously increased (owing to the sympathy, or rather synergy, of the liver with the skin and kidneys, in being equally destined to remove noxious matter from the circulation); and, in general, its suppression proves less suddenly injurious to the economy than that of urine or perspiration.

Requesting an early insertion of the above, I remain, sir,

Your obedient servant,

E. Stanley, M.D.

July 11, 1832.

SALINE INJECTION IN CHOLERA.

To the Editor of the London Medical Gazette.

North Shields, July 10, 1832.

Sir,

I have read with much interest the cases of cholera, recorded in the Gazette, which have been treated by saline injections into the veins, and I consider the result as highly important, inasmuch as the *vis vitæ* being temporarily renewed, for the time snatches the patient from impending death, and thus places him in such a state as to be within the influence of other remedies; and certainly this is a most valuable desideratum.

But, on the other hand, I do not think the saline injections can possess any specific power over the disease; it is only a renewing of that portion of the blood which is so rapidly thrown out by the stomach and bowels, and thus supporting the system a little longer, by furnishing an additional supply. It is true, some have recovered under this plan of treatment, where nothing else was resorted to; and, no doubt, by protracting the life of the individual, nature is afforded another chance of throwing off the disease; but this is at best but a very precarious chance, and I think we must still look to other means to remove the disease.

Very soon after cholera made its appearance in this town, circumstances led me to the conclusion that it was of an inflammatory nature; and I then addressed a letter to the Board of Health for this place, advocating bleeding and large doses of calomel, combined with small ones of opium; this, I believe, was the first attempt at laying down a plan of treatment, after the disease appeared in this country. Subsequent experience has taught me, that certain modifications are necessary, more particularly as regards bleeding; but it has also convinced me of the disease being of an inflammatory character, and I have invariably found stimulants to prove injurious.

What I would venture, then, to suggest, in conjunction with saline injections, would be, to administer frequent doses of calomel and opium, washing it down with a saturated solution of the muriate of soda (warm), combined with sp. æther. nitrici; and when the pulse is restored to some degree of firmness by injecting into the vein of one arm, cautiously to abstract blood from a vein in the other, at the same time continuing to inject.

I observe by the Gazette for the 7th July, Dr. Laurie, of Glasgow, has practised bleeding in the way I propose, and the case terminated unsuccessfully; but I do not by any means consider that one case as sufficient to deter from giving it a further trial, particularly after the contradictory results I have seen to arise from bleeding in cholera. I have very often seen it produce re-action in the worst cases; and it is but right to acknowledge, that I have seen a feeble pulse sink irrecoverably after a very small bleeding.

I have the honour to remain, sir,

Your obedient servant,

Edward Greenhow.

ON THE SALINE TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

If your patience is not already exhausted with communications respecting malignant cholera, perhaps you will have no objections to give insertion to two cases; in one of which the saline treatment was employed without the slightest benefit; in the other opium

was given, combined with salines, and with the best effects. When I speak of the saline treatment, I do not mean saline injections, for no injections were used in either case; but I mean the exhibition of saline medicine by the mouth, as practised at Cold-Bath-Fields Prison, in many cases in which the injections were not employed, and in many of those cases also in which the injections were employed.

Case I.—I was called to a lady who resides on Ludgate Hill, at six o'clock in the morning, on Monday the 9th July; and from her relatives I received the following statement. She was attacked with diarrhœa on the Saturday night previous, which continued all Sunday, and until two o'clock on Monday morning, unaccompanied with any other symptoms of disease. At two o'clock that morning vomiting came on; at four o'clock cramp in the lower extremities; at six o'clock the patient got out of bed, and was so exhausted that her daughters were obliged to lift her in again. Up to this period nothing had been done for her relief, the patient having most unfortunately concluded that it was merely a bilious attack, which would go off spontaneously. If I had been called in sooner, I have no doubt but the disease might have been cured without the slightest difficulty; but when I attended the evacuations were like rice-water, and the bowels extremely irritable; pulse feeble; countenance anxious; the eyes sunk in the orbits; thirst; vomiting of a bilious fluid; and cramp of the lower extremities. I immediately prescribed saline draughts with opium, which only remained a short time on the stomach. At ten o'clock the symptoms were not mitigated in the least. It was now proposed to send for the medical attendant of the Cold-Bath-Fields Prison; to which I assented, as I was desirous that the saline treatment might be strictly carried into effect, according to the plan adopted in the prison. Mr. Wakefield attended immediately, and about this period the patient's hands turned blue, cold, and shrivelled, in addition to the former symptoms. Mr. W. stated, that at the prison no opium was administered in this disease; no calomel; no stimulants. They trusted to salines, and to salines alone. He recommended a powder, containing oxymuriate of potass, carbonate of soda, and muriate of soda, every hour; and in every intermediate half hour the third of a Seidlitz powder. Mustard poultices to the stomach and lower extremities. When the patient was thirsty, the only fluid which she was allowed to drink was a solution of carbonate of soda; and as soon as it was rejected by vomiting, it was repeated. This plan was strictly enforced, with very little variation; and under it the patient became gradually worse, and died at eight o'clock the same evening.

I have sent you the history of this case, in order that the plan adopted in Cold-Bath-Fields may become generally known. I am not an uncompromising advocate for any exclusive plan of treatment; and I must say the salines proved utterly useless in the present instance. The patient was not an unfortunate individual, who had been destitute of the ordinary comforts and conveniences of life; on the contrary, she was a stout healthy woman, and a very favourable subject. This mode of treatment also was commenced soon after the disease exhibited unequivocal symptoms of malignancy. Of course this case proves nothing either for or against saline injections.

Case II.—This patient is a young man, æt. 28, living in Broad-Street, Holborn. He was attacked precisely in the same manner as in the first case which I have related; and I was sent for to his assistance when the disease had made an equal progress. I gave him salines every half hour, with the addition of tincture of opium—a combination, as far as my limited experience has gone, much more effectual than salines alone. The patient took, in divided doses, *six drachms of tinctura opii in twenty-four hours*, with very great advantage. He is now convalescent.

I have lately been informed of a case by an eminent physician, in which a saline solution was injected into the veins. The immediate effect was astonishing. From a state of complete collapse the patient raised himself up, and declared he was well again; but in three hours he died of apoplexy. Saline injections* appear to have been

* We apprehend that our correspondent is in error in supposing that saline *injections* have been extensively used in the prison in question; at least we have understood that saline medicines by the mouth had been almost exclusively adopted.—E.G.

more successful in Cold-Bath-Fields Prison than any where else.

I am, sir,
Your most obedient humble servant,
R. S. RICHARDSON,
Member of the Royal College of Surgeons, &c.

1, Bartlett's Buildings, Holborn,
17th July, 1832.

NARCOTIC CLYSTERS IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

HAVING in my own person experienced the most beneficial results from narcotic clysters, at periods of extreme pain from intestinal spasmodic contraction, and also when suffering under very severe nervous irritability, after an operation on the hand, I would call the attention of the profession to this neglected means in the treatment of cholera. By the term neglected, I would infer its too unfrequent use by some men in ordinary diseases. Being justified in practising any treatment in this disease which may promise but a shadow of success, we should fully satisfy ourselves how far the judicious and well-timed introduction of stimulating or soothing proportions of tinct. opii—the diffusive stimulus of ammonia in cautious quantities—mercurial fumigation, &c. &c. would answer by the means proposed. The remedy applied in my own case has been, gtt. x. to xx. of tinct. opii in ʒij. ʒiij. or ℥ss. of water; or from one to two grains of crude opium in watery solution, by means of a small brass syringe with an ivory pipe. In Indian cholera, of course, we dare not employ this drug but as a stimulant. Opium administered after the above plan is readily absorbed, perhaps in a large proportion by the numerous veins supplying the parts taking it at once into the circulation. It produces its effects in small quantities, and does not disturb the sensorium. These are valuable properties in a general point of view.

There are cases of traumatic delirium (where this remedy alone was most successfully practised in a Parisian hospital), recorded, I think, in one of the Medico-Chirurgical Reviews about 12 months since.

Should you deem this communication worthy of publicity through your Gazette, you will oblige

Your obedient servant,
WM. HALLETT.
Lic. Ap. Comp.

Devonport, July 14, 1832.

REPORTED CHOLERA IN COLD-BATH-FIELDS PRISON.

THE following additional document has been sent to us by Sir D. Barry, with this observation, "that being the result of a non-official inspection with Dr. Stevens, on the 27th ultimo, it was not intended for publication; but that *now* it cannot be withheld, seeing that neither the Governor, nor the visiting magistrates, at their meeting on the 12th instant, had the candour to state, that the inspection on the 28th ult. was the *second* which Sir David had made within twenty-six hours, though they had all seen him on both occasions. This paper fully accounts for the incredulity, as to the actual existence of cholera in the prison on the 28th June, manifested by the official medical inspectors on that day, and commented upon by Mr. Rotch at the Justice Meeting on the 12th instant."

June 27, 1832.

Visited the Cold-Bath-Fields Prison this day at two o'clock, accompanied by Dr. O'Shaughnessy. Learned from the Governor, in his office, that the population of the prison at this moment is about eleven hundred of both sexes, including twenty-one children; that the daily discharges and admissions are each about thirty-five on an average; the whole composed of malefactors, vagrants, and paupers. That the daily allowance of food to an adult is (since the introduction of cholera) 1¼ lb. of bread, 1 pint of gruel, 1 pint of oxhead soup; 6 ounces of boiled meat four times a week, free of bone, and weighed after having been boiled. No soup on meat days. That the deaths during the spring, summer, and autumn months last year were only two; during the

whole of last year fifteen. That since the 3d June this year there have been twelve deaths, all from cholera. That the present state of cholera cases in the prison is, as reported to him (the Governor):—Men, 55 cases; women, 15 ditto. That he had not lately seen them—he could be of no service, and was recommended not to go into the cholera wards*.

Walked with the Governor, Dr. Stevens, and Dr. O'Shaughnessy, round the gardens and other open spaces within the walls, and recommended the prisoners' rooms to be thinned, by encamping a part of the inmates in the gardens.

Dr. Stevens, with his two assistants, then conducted Dr. O'Shaughnessy and myself round his cholera wards, in which there might have been thirty or forty patients, but not one in the collapsed stage, though thirteen cholera cases are stated to have been admitted this morning. There certainly was not a single case which, from any symptoms witnessed by me, I could point out as a case of cholera†. I repeatedly begged Dr. Stevens to shew a recent case—a case of confirmed cholera—a case of collapse. He replied, that it was unlucky I had come on that day‡; that there was no case of collapse then in the house; and remarked, that by his treatment he prevents them ever coming to the collapsed stage. Yet twelve have died since the 3d,—two of whom the day before yesterday; and he (Dr. Stevens) allows, that several became collapsed after having been admitted into hospital in the prison, consequently whilst under the saline treatment.

One boy admitted this morning; in bed; has had neither vomiting nor purging, and presents a good steady pulse.

Two persons look pale, sallow, and with sunken eyes, as if they had suffered some severe evacuations, but the pulse in both is good. Seltzer water is given as common drink.

Recommended grated openings to be constructed in the walls of the small yards and rooms, on a level with the floors: the air in these places being more or less stagnant to a height of eight feet at least.

Dr. Stevens, during our visit to the prison, and afterwards, on the way to the Regent's Park, repeatedly asserted that two-thirds of the patients which he shewed us *would* have fallen into collapse, and *would* have most certainly died, if subjected to the ordinary plans of treatment by calomel, opium, stimulants, &c. In short, that the present usual mode of treating this disease is absolute poisoning. When asked if he had ever seen even one case of blue collapsed cholera saved, that had been treated by the twenty-grain doses of culinary salt, &c. and by nothing else, he candidly admitted that he never had; for that he recommended, in addition to the salt, large mustard poultices, hot saline, and opiate enemata, and hot salt and water baths, frictions, &c.

The substance of the above notes was written in the prison, or immediately after returning from it, and in their present state were read by Dr. O'Shaughnessy previously to the publication of Dr. Stevens's work on the Blood.

D. Barry.

I have read the above notes, which I saw several days since, and have to state that they are accurate in every respect.

W. B. O'Shaughnessy, M.D.

July 16, 1832.

On *the* 14*th instant*, a return was received at the Central Board of one man, stated to have been attacked in the prison on the 12*th instant*. He is noticed under the head "diarrhœa," and also under that of "collapse without pulse."

A woman was also reported, some days ago, as having been attacked, on the 5*th July*, with confirmed cholera. This last return was overlooked by me in my last communication.

D. B.

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* The Governor accompanied me round the cholera wards on the 28th June, but *not on 27th*. —D.B.

† I afterwards made the same observations to Dr. Stevens in presence of the Governor and Dr. O'Shaughnessy.

‡ I had come by appointment with the Doctor himself, made on the 25th at the College of Physicians.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

ON CALCULI IN THE VEINS OF THE SPERMATIC CORD, &c*.

By F. Tiedemann.

In the fourth volume of Meckel's *Archiv. für Physiologie,* Tiedemann detailed some observations on calculi found in the veins. Since then, farther information has been communicated by Otto†, Bouialsky‡, and Lobstein§. Otto found them most frequently in the venous plexuses of the womb and vagina, and occasionally also in that of the bladder. For the most part, the persons in whom they were found were more than 50 years old. He once discovered them in the veins of the prostate in an old man. In all the cases the veins were varicose, and contained coagulated blood, in which the calculi were deposited. In two instances, gouty concretions existed at the same time in the joints; and such was the case in the man in whom the calculi were found in the veins of the prostate.

The concretions were either whitish or yellow, and of a pearly lustre, and they varied in magnitude, from the size of a millet-seed to that of a pea. They were round or oval, sometimes uneven on the surface, and consisted of concentric laminæ. Bouialsky found five calculi in the veins of the spermatic cord of a man 57 years of age; four of these lay detached in the blood, and one was connected with the internal coat of the vein. Lobstein has found them in the veins of the testicle, of the womb, of the bladder and rectum, and once in the veins of the spleen. Dupuytren discovered them in the anterior and posterior tibial veins; and Tilorier in the varicose subcutaneous veins of the leg. Bouillaud has repeatedly seen them in old varices of the lower extremities.

Since Tiedemann's former publication on the subject, he has several times observed calculi in the veins of the bladder, rectum, and womb. The most remarkable instance was that of a man, 51 years old, in whom numerous concretions were formed in the varicose veins of both spermatic cords. In the veins of the right, he counted fifteen of these calculi; in that of the left, twenty-one. The smallest were about the size of a mustard-seed, the largest exceeded a line in diameter; they were either round or oval, and of a yellowish-white colour. In general they lay disengaged in the coagulated blood, but some were attached to the inner coat of the vein by means of a very thin transparent membranous covering. In some parts of the varicose dilatations, soft fibrous masses adhered to the internal tunic of the vein, which Tiedemann supposed to be the nuclei of similar concretions. Cruveilhier has seen calculi adherent to the inner coat of the vein.

Gmelin subjected some to analysis, and found them to consist of—

Animal matter.....	27.5
Phosphate of lime	53.5
Carbonate of lime	15.5
Magnesia and loss	3.5
	100.

Two explanations have been offered as to the origin of these concretions. Tiedemann, Otto, and Lobstein, conceive that they are formed in the varicose dilatations of the veins, from the deposition and aggregation of the earthy matters of the blood. Others have imagined that they originate in the coats of the veins, in a similar manner to the calcareous concretions in the coats of arteries, and are subsequently detached. Their rounded form, and uniformly smooth surface, are opposed to the latter explanation. When they are attached to the inner coat of the vein, Tiedemann suggests that the connexion arises from inflammation excited by their presence, and the consequent effusion of coagulable lymph.

SULPHATE OF QUINIA—CAUSES OF ITS HIGH PRICE.

The quantity of Peruvian bark which is imported into Europe is very considerable; but chemistry has recently proved that a large portion of the bark itself is useless. The alkali quinia which has been extracted from it, possesses all the properties for which the bark is valuable, and only 40 ounces of this substance, when in combination with sulphuric acid, can be extracted from 100 ℔. of the bark. In this instance, then, with

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* Translated and abridged from Tiedemann's Zeitschift für Physiologie, iv. b. 1 heft.

† Neue seltene Beobachtungen zur Anatomie, u. s. w. Berlin. 1822.

‡ Voienno-Meditsinski Journal. Petersburgh. 1827.

§ Traité d'Anatomie Pathologique. Paris. 1829. T. i. 504.

every ton of useful matter, 39 tons of rubbish are transported across the Atlantic. At the present time, the greatest part of the sulphate of quinia used in this country is imported from France, where the low price of the alcohol, by which it is extracted from the bark, renders the process cheap; but it cannot be doubted, that when more settled forms of government shall have given security to capital, and when advancing civilization shall have spread over the states of South America, the alkaline medicine will be extracted from the woody fibres by which its efficacy is almost lost, and that it will be exported in its most condensed form.—*Babbage on the Economy of Machinery.*

BITE OF A RATTLE-SNAKE CURED BY ARSENIC.

On the 25th of July, 1830, at 8 o'clock, A.M. I was called to visit Miss Happy Briggs, who had been bitten twenty-six hours before by a rattlesnake; she had received two wounds, one on the instep, and the other near the great toe of the left foot. Her body was considerably swollen; her eyes almost closed; tongue tumid; deglutition impeded to some degree, and articulation indistinct. Her left leg, as high as the hip, was enormously distended, and threatened mortification, the skin having a shining appearance, with discolouration, being black on the outside and mottled on the inside with black and yellow spots, so that one might have fancied it resembled the skin of the snake. The bitten part pained her severely, and the inguinal glands on that side were much enlarged; pulse low, and about sixty strokes in a minute, and surface cold. She felt extreme nausea, and vomited on making the least exertion. Great thirst was an attendant symptom from the first, so much so, that previous to my arrival she had allayed it with an immoderate quantity of water. Her bowels were rather constipated. Her mind appeared not in the least affected.

I commenced my treatment by making longitudinal incisions from the left knee downwards to the foot, and scarifying the wounds, and then blistered the limb extensively. She took the volatile alkali in f. ʒij. doses at intervals for two hours without any good effect. Finding the circumstances thus, I determined to have resort to arsenic, and Fowler's solution not being procurable at the moment, I employed the oxyde, commencing in doses of a quarter of a grain every fifteen minutes, for about two hours, when I perceived a material alteration in my patient; the swelling diminished somewhat, the nausea ceased, and she was able to articulate distinctly. By continuing the treatment with the arsenic for two hours more, all dangerous symptoms disappeared. All her body except the left leg, (which, however, was lessened), was completely unswollen. Towards night she complained of headache, and her pulse rose to about 130, strong, full, and hard, when I judged venesection expedient, and extracted ten ounces of blood, which relieved her. I prescribed then the oleum ricini, to obviate constipation, and effected the intention. After leaving the blisters twenty-two hours on the limb, they were removed, and the sacs formed by the cuticle contained from a pint and a half to a quart of liquid of a dark greenish colour. The blistered surfaces were dressed in the usual way, and in three weeks my patient, by taking a gentle purgative every third day, was restored to her former health and vigour.—*Dr. H. B. Philips, of North Carolina, in American Journal of the Medical Sciences.*

MEDICAL GAZETTE.

Saturday, July 21, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

CASE OF MISS BAGSTER.

THAT these lunacy commissions have become, as Sir Edward Sugden remarked the other day, "a scandal to the justice of the country"—a remark, by the way, in the propriety of which the Lord Chancellor expressed his decided concurrence—it scarcely needed the late proceedings in Miss Bagster's case to prove; but without that case the doings of inquests in lunacy matters would have been incompletely understood,—with it they are shewn up in due perfection, and they expire in a blaze of renown. We speak of their expiring, because we suppose there can be no doubt that the bill for the abolition of lunacy inquests, in their present dangerous form, will ere long be passed into a law*.

The abuses which have crept into the

* The preamble of this Bill (as amended by the Committee) purports that it is "to diminish the inconvenience and expense of commissions of lunacy:" and the first clause empowers the Lord Chancellor or the Lord Keeper "to address writs *de Lunatico* to any *one* or *more* persons who shall have sufficient powers to make inquisitions thereon."

constitution of this tribunal are very remarkable, and may warrant a moment's notice. Originally destined for inquiring into the state of mind of alleged lunatics and idiots, in order to determine whether their property should be left at their own disposal or that of others, the latter object soon took precedence of the former, and the disposal of the property became the thing of primary importance. Under the semblance of due deliberation on the mental state of the unlucky persons brought before them—generally persons entitled to large possessions—the most enormous expenses have been incurred, and much time has been apparently bestowed in coming to a conclusion which, if there were no property concerned—if it were a mere matter of life and death—there can be little doubt would be arrived at with singularly quickened speed. In criminal cases, it is well known, that a jury will come to a verdict determining the sanity or insanity of the accused without any considerable delay, and in nine cases out of ten nobody sees reason to question its correctness: but it is equally well known that there is generally no property to be settled in these cases—nothing in the shape of money to be spent—or if there be, that the question of sanity or insanity must be first determined, and the arrangement of the other point be suffered to follow as matter of course. The lunacy inquirers, on the other hand, seem unable to keep their eyes off the money—*pecunia primum* is their maxim—instead of considering independently the intellectual capacity of the individual submitted to their judgment, the money is their test: in order to determine whether the individual has capacity enough to take care of his money, they inquire chiefly how he is as to his "figures," and how he *has* managed his pecuniary concerns: money is their "all in all," and its right management the perfection of intellect.

Some of our legislators, a few years ago, were suggesting that there ought to be inquests on spendthrifts as well as on lunatics: it was quite an unnecessary suggestion, but we believe it was not then generally understood that the tribunal of which we speak had made itself competent to the settling of all questions of that nature: in short, their only limitation seems to be this,—whoever cannot take care of his money clearly comes within the range and cognizance of their jurisdiction,—whoever has none to take care of, may go about his business—he shall have none of their regard.

Miss Bagster's case throws light on some new powers with which it seems our inquisitors are invested: it would now appear that they can take cognizance of all mental defects, natural and acquired, connected with inability to *keep accounts:* and "unsoundness of mind"—that capacious verdict—must be understood to include henceforth—or at least while the tribunal lasts—more than it ever did before.

Miss Bagster was found to be "of unsound mind, so as to be unable to take proper care of her goods and chattels, tenements, and so forth." We are half inclined to believe that there has been some mistake here, and that the jury found, according to the evidence, that the lady was "unable to manage her affairs so as to be of unsound mind, and to come under the jurisdiction of the court:" but leaving the verdict as it stands, we shall take leave to examine the grounds on which it was returned, and to shew what we take to be sufficient reason for questioning its correctness.

That Miss Bagster is unable to "manage her own affairs" in the sense in which that phrase seems to have been

understood by the jury, and employed by several of the witnesses, namely, that she cannot reckon, nor buy and sell, nor keep accounts, we are quite ready to admit; but how the *unsoundness* of her mind has been made out, after the most deliberate perusal of the eleven days proceedings, we cannot see. There was a favourite paradox among the sages in times of yore, that all but the wise man were fools and insane: in this paradoxical sense, the term *unsoundness of mind* might perhaps be taken to comprehend Miss Bagster's disabilities; but we cannot persuade ourselves that sophistry of this sort would be employed in so serious an investigation: we must, therefore, try again whether we can pretend to explain the mystery.

Unsoundness of mind—the *non compos mentis* of the old lawyers — has been, we suspect, applied to Miss Bagster in a new and most questionable sense. Her conduct on certain occasions has been very *childish*, (indeed, so much was fully made out in evidence), and she is very *deficient in arithmetic*. As to the former, is there any one so mere a tyro in physiology as not to know that the mind is not invariably developed with the number of years; and that such development greatly depends upon the circumstances in which the individual is placed? Now what were the circumstances of Miss Bagster from her infancy upwards?—complete dependence upon others; all her wants supplied; all her wishes gratified, without the slightest exertion of mind on her part; indulgences even beyond those allowed to the generality of children, extended to her; never enjoying the society of her equals in age; brought up by a doatingly-fond grandfather, and an ill-tempered mother; and attended by servants who always treated her as the favourite of "grandpapa." She should, indeed, have an unusually strong mind, if, under such circumstances, she could arrive at "years of discretion" with more than the discretion of a child. Then, as to her arithmetic, she was taught nothing that would give her the least trouble and annoyance in the learning. For "figures" and arithmetic she had a dislike, and was accordingly never instructed in them; yet we find, in the evidence, the principal stress laid on this: some of the medical witnesses thought it of vital importance, and the jury, who no doubt were (like a true London jury) pounds-shillings-and-pence men, most probably founded upon it, naturally enough, their inference as to her mental unsoundness.

She was ignorant of arithmetic, which she *had never been taught*—for it does not appear that any of the *nine* schoolmistresses who gave evidence against her ever systematically set about teaching her that branch of education; she could not read off a series of figures; she could add but imperfectly; came to a full stop in her multiplication table, at "twice ten is twenty;" and (monstrous to relate!) could not solve a problem for Dr. Roots, relative to her income from 10,000l. stock in the 4 per cents!!! Nay more, she could not tell the probable annual expenditure of a carriage and horses; and worst of all, as it shewed so perfect an inability for the management of a household, she valued a quartern loaf at two shillings! Once for all we ask seriously, what does this prove? The girl had no motive—and no small dislike—to "do her tables" when she attended school (though now she laments her neglect so feelingly); say even that she was deficient in mental power to comprehend the properties of numbers;—and shall these be dwelt upon as facts of primary importance in deciding her unsoundness of mind, or her incompetency for the management of her own affairs? True it is that the simplest

intellect is deemed capable of attaining the elements of arithmetic; but there is another ingredient or two requisite, in aid of whatever portion of intellect the learner may bring to his task—patience and attention; two attributes of mind of which Miss Bagster, in common with many possessed of undoubted talent, was destitute: and if she really could not acquire this sort of knowledge by whatever dint of teaching, —what is more usual than to meet persons who have a most invincible repugnance to directing their attention to some particular subject? We must confess, then, that it was with feelings of unmingled surprise we perused the evidence of the majority of the medical witnesses, who related how they returned again and again to "figures," in examining Miss Bagster professionally. Why it were enough to confound a moderately nervous *mathematician*, to be catechized as the unhappy young lady was, by a succession of grave personages, whom she, ere long, discovered to be "mad-doctors!" And then such a test!

"It is quite ludicrous," says our contemporary of the *True Sun*, "to see the stress that has been laid on this trial, upon a knowledge of the elements of arithmetic: it is, we allow, one of great importance in life, but which any fool may learn. Miss Bagster, who is no greater fool than thousands who walk abroad, and dress, and give dinners, without a keeper, might evidently have acquired it as well as any body. What little she does know, shews that she might have known all the rest. And then to asks her about the '*Four per cent. Consols!*' What wretched pedantry! We should like to know how many of the intelligent and accomplished young ladies of the west end of the town,—aye, or of the young gentlemen either, who are not commercial,—know of the art and mystery of money-getting, and of its learned language, and most Greek contractions! As little as they of the Stock Exchange know or care about the Consuls Hortensius or Pansa."

We have confined ourselves in the present article merely to one or two points connected with this remarkable Commission — indeed, chiefly to the value of the arithmetical test, so prominently introduced in it as a mode of coming to an estimate of Miss Bagster's powers of mind; but there are several other considerations no less deserving of notice,—such, for example, as the moral and external circumstances which led to some of the most apparently unaccountable incidents in the lady's conduct. The discussion of these we are obliged to forego, in consequence of the unusually large portion of our space devoted to our condensed report of the proceedings; and we do so the less unwillingly, as we find the subject is still in the hands of an able writer, who has already favoured the public with an admirable series of papers upon it*.

ANATOMY BILL.

This Bill was read a third time and passed, last night (Thursday 19), in the House of Lords. The numbers were—29 for, and 9 against it—Majority, 20.

COMMISSION OF LUNACY.

COURT OF KING'S BENCH.

(Before Commissioners Coulton, Winston, and Whitmarsh, and a Special Jury.)

(*Specially reported for this Journal.*)

First Day, *Tuesday, July* 3, 1832.

BAGSTER *v.* NEWTON.

This was an inquisition to try the state of mind of Mrs. Newton, otherwise Bagster.

Mr. Pollock stated the case for the petitioner, Mrs. Bagster, Mrs. Newton's mother; and, in the course of his statement, gave a summary of the evidence in behalf of

* See the *True Sun*, evening paper, since Monday (16th) inclusive.

his client. The learned gentleman mentioned that the fortune of the alleged lunatic was 5000*l.* by the death of her uncle, and 4000*l.* *per annum* from other relations.

The first witnesses were three school-mistresses, under whose care Miss Bagster had successively been. They all proved that she was an exceedingly slow and stupid scholar—particularly deficient in capability to learn arithmetic; that her temper was occasionally very violent; and that, in short, she was a very giddy and bold girl.

Two other witnesses, who were governesses, and entrusted with the domestic management of Miss B. at Alderman Crowder's, proved that she was guilty of very unruly conduct on several occasions, tearing off their caps and bonnets, and sometimes kicking, and fighting, and spitting at them. She was totally ignorant of the value of money, and evidently weak minded, and not fit to manage her own affairs.

Dr. Macmichael examined.—Visited Miss Bagster by appointment on the 5th of May last, in company with Dr. Munroe. My object was to ascertain the state of her mind. She did not appear to be a person of sound intellect, or to possess the ordinary moral perception of a person of her age. This was subsequent to her marriage with Mr. Newton. I asked her to tell us the story of her elopement with Mr. N., which she did very clearly. She said she had joined a party to go to the Zoological Gardens: while walking there, Mr. N. asked her to leave the party, and get into his cabriolet. At first he promised to take her to see a lady in the neighbourhood of the Regent's Park. After some time he told her he should not go there, and she then found herself at Camden-Town, where she saw a carriage with four horses, and she was ultimately persuaded to get into the carriage. I then asked her if he told her where she was going to, and she said that they were half way off before she was informed that they were going to Gretna Green. I inquired why she ran away with Mr. Newton? when she said, "I don't know; but I love somebody else better." She then said that she had read a great number of novels and stories of elopements, and thought she should like to run away. I asked her whether she considered herself the wife of Mr. Newton: she said,—no; because the marriage was not performed in a church, and by books. She also said, that she did not know whether she could consider it as a marriage or a farce. I asked her whether she considered herself Miss Bagster or Mrs. Newton? She said she did not know whether it was a marriage. At another visit I asked her if she slept with Mr. Newton? She did not show any appearances of modesty, and said,—yes; she slept with him two nights. She said so without hesitation or embarrassment. I was satisfied from her answers that the marriage was consummated. She mentioned other persons whom she would wish to marry—among them, Alderman Kelly; but, she added, that she thought he was too old for her. I examined her respecting figures; but she seemed perfectly ignorant of how many pence there were in a shilling. I think she has no reasoning faculty whatever. She said there were six weeks in the year, and six days in the week, and as for Sunday—that was kept holy. I wrote down a common sum in addition; but she could not manage it, and said she was very stupid. She could not add shillings and pence together. I do not think she is competent to manage her own affairs, nor to be trusted with any money. In my opinion she is of unsound mind.

Second Day, *Wednesday, July* 4.

Miss Clayton, Miss Bagster's governess from the year 1829 until the affair of the elopement, was next examined.—I could never teach Miss B. the value of money: on one occasion she gave away a sovereign to a beggar for a sixpence. I followed her one day into the drawing-room, and observed her drinking wine out of a decanter. I told her I thought she had often done so,—on which she took up the decanter to throw it at me. She did not do it, for I ran behind a screen; she, however, took up some pears and threw them. She scratched the face of her mother, so as to render her unfit to be seen for some days. On the day of Alderman Crowder's funeral she wore a black crape dress; before the funeral left the house, she tore the sleeves from her frock, and also the frill from her mother's dress. She always exhibited great partiality for gentlemen. Shortly after the funeral, she expressed a wish to write to Mr. Jupp (who had been proposed for her in marriage;) I told her I could not allow her to write,—upon which she took up a dessert-knife, and threw it at me. It lodged in the sleeve of my gown. Her conduct was so violent, that I was obliged to quit the room, and lock myself up in another apartment. (The witness then went on to detail a variety of adventures and eccentricities of her pupil, which we are obliged to leave out for want of room.) At the Zoological Gardens, on missing her, I was much alarmed. The next time I saw her was at Keswick, where she was in company with Mr. Newton and Mr. Newberry, (or Wilson.) On seeing us she kissed us all, and her mother took her away in a carriage: after riding for about an hour, she became very violent, and scratched her mother's face. We were under the necessity of tying her arms. She then said, "why don't you let me go to my husband?" When she arrived in London, she spoke of the elopement as a lark, and said she should like to run away again. She asked me whether I thought she was *enceinte*. She has not

the least idea of matrimony: on one occasion she said she should get married for the sake of dressing smart, and walking the saloons. I told her the description of persons who frequented such places, but she did not understand me. She frequently spoke on this subject before company. Her mother's conduct has been too kind to her.

Dr. Sutherland said he attended Miss Bagster. I said to her, that having understood she had been on a journey, I wished to ask her where she had been, and she said to Gretna-Green for a bit of fun; and she wished to have gone on to Edinburgh, fearing she might be caught. I asked her if she had made any promise at Gretna-Green. She said she laughed immoderately the whole time, and thought it a good bit of fun. She remarked that she was indifferent as to the person she should marry, and mentioned the names of six persons, but Mr. Newton's name was not amongst them. I asked her if she had been with Mr. Newton: she said she had been with him two nights. I set her some simple sums in addition, but she could not manage them. She maintained that her name was Bagster, and no other. On asking the reason why she had such a dislike to her mother, she said they never could agree. She stated her age to be twenty-one, (she should have said twenty-three.) On the whole, I conclude her to be of unsound mind, and not fit to take care of her person and property.

Dr. Gordon, from similar conversations with her about the Gretna Green "bit of fun," &c. formed the opinion that she had a very imbecile mind. He asked if she was not aware that marriage was a very serious obligation, and she said, no, she was not. He asked her what she understood by marriage, and she said, "cake, favours, and church!" She mentioned then two or three gentlemen whom she would wish to marry. Dr. Gordon was now questioned as to the remarks which Miss B. had made on the marriage state,—they were of so *delicate* a nature that the female auditors were ordered to withdraw. Dr. Gordon then related the conversation on the subject. He said Miss B. gave her answers without any reluctance or a blush.

THIRD DAY, *Thursday, July 5.*

Miss Catharine Kett gave evidence to a piece of unruly conduct on the part of Miss Bagster, which occurred four years ago—when the latter lady beat her governess and tore her mother's bonnet. But Miss Kett's evidence was overruled, as it appeared that in the event of the death of Miss Bagster without issue, she was to succeed to a part of the property.

Rev. Samuel Smith, officiating chaplain at the Mansion-House, examined—In the year 1829 I used to see Miss Bagster repeatedly every day: her manners were those of a child of eight or nine: her behaviour in company was calculated to expose her. She would turn round in her chair, and look strangers in the face. I endeavoured to instruct her mind in religion, but I could not make her comprehend her moral and religious duties. I attributed it to incapacity. She did not seem to have a proper sense of delicacy and modesty before gentlemen; nor was she seemingly aware that telling a falsehood was criminal. Miss Bagster expressed a great dislike to her mother, who was certainly hasty and vehement in her manner. The Alderman was doatingly fond of Miss B.; and when the mother corrected her failings, the grandfather would pass them over.

Mr. Windus examined.—At a dinner in the Mansion-House, I handed Miss Bagster from the drawing-room to the dining-room: she was then a total stranger to me. After my asking her to take a glass of wine, she turned round and looked at me very earnestly in the face; she trembled very much, and said that she was very unused to company, and did not know how to behave. Presently she said she was in love, and asked me if I knew Mr. Jupp. I asked her who she was, and she said, "I am the grand-daughter of Alderman Crowder, and the Lady Mayoress is my mother." I then inquired if her father was living, and she said he was, but that she had not seen him—he was a very unfortunate man. She then went on to talk about Mr. Jupp, but I commented on the impropriety of her conduct, which she received with considerable apathy. I thought her to be in age about sixteen, but in knowledge about eight or nine.

Mr. Samuel Wiggins considered Miss B. to be very childish. In the year 1827, he witnessed some wild symptoms of temper in her, when she threatened to tear her governess's bonnet to pieces, and would no doubt have done so, but that witness seized her hands, and the governess tied them behind her back with a riband, and then sent her to bed.

Frances Holland examined.—I was nurse to Miss B. for the first eleven years of her life, and have been again for the last seven or eight years. She was always passionate; would throw knives, plates, and forks, at people; her plate, with her dinner on it, she has thrown at me. Some of these violent things she would do without the least provocation, and then perhaps ask, "what harm she had done?" She has not any consciousness of the propriety belonging to her sex. She has said that she thought there was no harm in living with different gentlemen. I have reason to believe that she is not in the family way. (Witness then described Dr. Haslam's visit—for which we refer to the Doctor's own evidence.) I formerly had twelve guineas a year wages, I have now thirty; it was raised by Miss Bagster on her coming of age, or soon after; it

was obtained for me by her from her executors, in consideration of my having been so long in her grandfather's family.

Sir George Tuthill examined.—I visited Miss Bagster three times. I told her that my object was to ascertain the state of her mind. I was first struck with her very childish appearance, which is not that of a person of more than fourteen or fifteen years of age. I began by asking her a few questions, but she did not answer me at first; she wished Miss Clayton to tell her what to say. Sir George having presently found her more communicative, obtained from her a detailed account of her visit to the Zoological Gardens, and the transactions connected with the elopement. She got into Mr. Newton's cab, because it was suggested to her by Newton's sister that it would be a capital joke: she had no thought of marriage at the time. When she was anxious to return, after having gone some distance, she was told that she would be laughed at by every body. She said that they arrived at Gretna Green about one in the morning, when the parson was called up to perform the ceremony; and when she objected, the parson said she *must* go through it since he was called up, (laughter.) She said that if she had not been treated violently, she would have had no objection to return home and quit Mr. Newton, for she liked another (Mr. Perfect) better. The witness then detailed his examination of Miss Bagster, as to her knowledge of the value of the simplest articles of dress—as to the number of shillings in a guinea and a sovereign—and of sixpences in a half-crown—of all which she was ignorant. He examined her in arithmetic, and set her a simple sum in common addition, which she was unable to complete. When I inquired what Mr. Perfect would think of her having slept two nights with Mr. Newton, she replied that she hoped Mr. P. would never hear of it, but added that she considered her trip to Gretna to be a mere joke. On inquiring what she thought a good age for a young lady to marry, she said twenty-four, and that she would marry at that age. The witness detailed at some length the accounts he had heard from Miss Bagster with reference to her attachment for other gentlemen, as given by other witnesses, and concluded by stating that he had no doubt of her being of unsound mind, and incompetent to take care of herself and her property. The main ingredients, he said, on which he formed his opinion, were the want of knowledge of figures, and the total absence of sensibility as to her marriage. She was not a lunatic, and certainly not what might be called a complete idiot.

Mr. Bowling examined.—I am a surgeon, residing at Hammersmith, and have been in professional attendance on Alderman Crowder's family since 1819. Miss Bagster has been always treated by all who knew her as a person in an imbecile state of mind. She is violent in temper, and is subject to fits of passion: those fits have become more violent of late, (in 1827 and 1828.) The witness now detailed a conversation which he had with Miss B. relative to the journey to Gretna-Green, in the course of which it appeared that the same familiarities which had taken place after the marriage at Carlisle and Keswick, had occurred in the carriage on the way to Gretna-Green. He considered Miss B. to be of unsound mind.

FOURTH DAY, *Friday, July 6.*

Miss Laforest examined.—I have been intimately acquainted with Miss Bagster since 1824. She has always been treated as a child both by her mother and Alderman Crowder. Her governesses and servants treated her in the same way. Her conversation was quite irrational: she would sometimes attempt a conversation upon books, but she could never follow it up. When the Lord's prayer has been repeated, she always laughed, and treated it as a jest; I am certain she did not understand the meaning of it. Mrs. Bagster always read prayers morning and evening at the house of Alderman Crowder. When last I spoke to Miss B. (now three weeks ago) about her being married, she said she did not understand nor care about it, and that while the ceremony was being performed she said, "I wonder what will Miss Laforest think of this fun." From my repeatedly seeing her, I have not the least doubt of her being of unsound mind.

Two chambermaids, one from Carlisle, the other from Keswick, were now called, and proved the conversations which they had had with Miss Bagster at their respective inns. They thought her very childish and silly.

Captain Kelly examined, (the ladies in court being previously requested to withdraw.)—He stated that on Thursday last he dined with Alderman Kelly; and after dinner, being left alone with Miss Bagster, she related to him the particulars of the familiarities that had passed between her and Mr. Newton, and that in the plainest and grossest terms. He considered that she had little or no intellectual faculties, and was unable to take care of herself.

Mr. Thomas Richards said, he knew Miss B. since she was twelve years old. Her conversation was always that of a child, and she was treated so by the family: it did not become a lady of her age. Since her return from Gretna, she has said that she should like to marry Alderman Kelly, because he had bags of money. This was said, not as a joke, but in the childish manner in which she was in the habit of talking. She also said that she expected to be Lady Mayoress. After Dr. Haslam visited her, she said several strange gentlemen had been there, and

she understood they were mad-doctors, and intended to confine her: she is in the habit of looking in the glass, and inquiring how she looked. His opinion was that she was of unsound mind.

The remainder of this day was spent in hearing several other witnesses, who gave evidence much like the preceding.

FIFTH DAY, *Saturday, July 7.*

Mr. Kingdon, Surgeon, examined.—I have been acquainted with the family two years. I visited at the Mansion-House; when one day Miss B. said she wanted to speak to me in private: this was to request me to ask the Lord Mayor to let her marry Mr. Jupp. I thought her excessively imbecile. Since her return from Gretna Green I have paid five visits: the first was about half an hour after her arrival. I found her in a violent paroxysm of rage. She was spitting and kicking at the persons in the room. A footman was holding her in the chair. The footman was ordered to let her go, and to quit the room; when she became pacified. She then talked about her journey, and said she had enjoyed it very much. She did not consider herself married, for a proper marriage was when they went to church with carriages, and had cake and wine, with favours and bridemaids. I should think it an insult to compare a child of five years of age to her, in point of intellect.

Mr. Shadbolt and *Mr. Cope*, two of the executors of the late Alderman Crowder, were examined, and gave their opinion of the trifling nature of Miss B.'s conversations: neither thought her fit to be trusted with money. The property left her by her uncle was without restriction. One Good Friday, Miss Bagster came into Mr. Shadbolt's pew, at Kensington church, and was observed to look for the prayers with the wrong side of the prayer-book upwards. Mr. S. thought her manners those of an insane person.

Dr. Munroe, sen.—I have been physician to Bethlam Hospital for 16 years; I saw Miss Bagster on the 5th of May last, at the house of Alderman Kelly. Dr. Macmichael was with me. I was struck with her appearance and manner, which were quite childish; yet, from her physical development, I would take her to be sixteen or seventeen years of age. We were introduced as friends of Alderman Kelly. She told us all about her journey. I asked her whether she was married? She said she did not know whether it was a marriage or a farce. She had no particular affection for Mr. Newton; she said she preferred Mr. Perfect, and must marry him, to fulfil an old promise. She did not wish to return to Mr. Newton. She seemed wholly unconscious of matrimonial duties. She spoke of balls and the theatre as her principal amusements. At another visit I put to her some arithmetical questions. She knew that two and two made four, and four and two made six; but six and four, she said, made eleven, but corrected herself afterwards, and said ten. She could not tell how many shillings were in a crown or a sovereign, nor how many tens in 100. She said there were six weeks in a year; and, when I expressed my dissatisfaction, asked me, "Is not that right, sir?" She said it was not anger that made her violent to her mother, but something she could not overcome. She said that her teachers had never been able to teach her arithmetic. She asked what she should do when Lady Mayoress, as she expected an offer from Alderman Kelly: "Should she wear feathers or not?"—"Would they make her taller, or shorter?" Her fortune, she said, added to the Alderman's, would be immense: her's was 700l. a year, his 100l. I asked, "No more?" She then said 150l. She knew not how many months in the year. At my last visit (30th June), she said she had been told the object of the visits of the several physicians who came, and that she was not such a fool as not to understand them. She pressed me for my opinion of the propriety of her marrying Alderman Kelly. She is decidedly of unsound mind, and not in any degree fit to take care of herself or of her property. I should not call her idiotic or lunatic; but I should describe the state of her mind as that of extreme imbecility. Many insane persons have excellent memories, but I have not known imbecile patients have good memories. The power of learning language to the extent that Miss Bagster knew French, is consistent with what is called imbecility. The utter want of all judgment as to decency of demeanor, is a strong indication of imbecility. I have not the slightest doubt that she is imbecile.

The Commissioners and the Jury now retired to the Judges' room, for the purpose of seeing and examining Miss Bagster, who was in attendance, accompanied by her mother and Alderman Kelly. The principal questions put to her were the following:—Have you come from Mr. Alderman Kelly's? Yes.—I believe you are living with Mr. Alderman Kelly? I am a little deaf.—How long have you been living with Alderman Kelly? I think three months.—How long is it since you lived with your grandfather? I think three years ago.—Did you live with him a long while? Yes; as long as I can remember any thing.—How long did you stay at Miss Price's school? About a year; and that lady said, that if I did not continue at the school I should not succeed.—You left the house because you could not be constantly under the roof? My grandfather was terrified lest I should be ill, and he would not allow me to go. Miss Bagster then gave an account of her other schools, and of her governesses. How long has Miss Clayton been with you? Four or five years.—Are you fond of Miss Clayton? Yes, I

am; she is very kind to me.—She is with you still? Yes.—Do you recollect going a long journey? Yes, sir, I do.—How long ago is it, do you recollect? No.—Where did you go to on that journey?—(A pause.)—You do not recollect? Yes, I do; the name of the place is Springfield.—Where did you go from? The Zoological Gardens. Upon being asked whom she went with, she hesitated, and then said she went with Miss Clayton and the two Miss Newtons. The Commissioner, seeing her agitated, asked her if she liked the gardens? She replied, "Yes, but I like other places better; I like the theatres." She was then asked why she liked to go to the theatres? when she said, "merely for the sake of fashion." What piece do you like best? I think the last one; and that, gentlemen, was the *Hunchback*. I do not know why I like it, but there is something about it very much to be liked; there was Miss Kemble—she is a lady—I do not know the exact part—but she comes to town to visit the public places, and to visit her friends, but I forget their names; I did not remain to see the whole of it.—What was it that amused you most? I do not remember at this moment.—At what house was it? Covent Garden.—Which theatre do you like best (having mentioned that she had been at Sadler's Wells, and some of the minors)? Covent Garden; because it is the most genteel and fashionable. I like to go to Covent Garden, for all the fashions are there—in the winter season, I mean.

Did Mr. Newton say any thing to you at the Gardens? No; but his sister did.—What was it?—(A pause.)—I think she asked me to go and take a ride in her brother's cab, to go and pay a morning visit.—What did you say? I have no objection; I don't recollect what I said exactly.—Did you pay the visit? No, sir, I altered my mind afterwards; I said, no, I would not go.—Then where did you go, when you changed your mind? You know I went to this place (meaning Springfield).—Did you go all the way in the cab? No; I think I went to Camden Town, and then got into another carriage.—Did Mr. Newton ask you to go? I said to him, that if he would go to Springfield as fast as he could, and then return to Warwick-Square as fast as he could, I would have no objection.—When was it you said that?—(A pause.)—It was before I got into the carriage—it was in the cab.—Did you ask how far it was? No; I should think it was 800 miles, or perhaps more. Before you went to the gardens, had you consented to go off with Mr. Newton? I have been in the habit of going in to coffee, and I have heard whispers of it. I must say.—Whom have you heard whisper? Miss Emma Newton; she asked me if I should like to go; I said if my mamma heard about it she would not like it. When I went to the gardens I made up my mind not to go back to Warwick-Square. My mamma and I never agreed, I am sorry to say. When at Springfield somebody was called up, and came to the carriage door, and asked if we came to be married? I said, "do it as quick as you can, for I am in a hurry." I did not laugh at the ceremony, for I was too fatigued.—Did you write any thing? Yes; the blacksmith told me to write my name, and I wrote it with a *g*, although my mamma uses the letter *x*. Questions were then put to her relative to what took place afterwards; these she was very reluctant to answer. Who slept in the room with you?—(A pause.)—Mr. Newton.—Why did he sleep with you? I don't know.—Was he your husband? I don't know; they say not, because it was done at Springfield. I was told so when I came home to Alderman Kelly's. My mamma says it was not a right marriage. I am much perplexed about it.

Miss Bagster was then questioned at considerable length, about her feelings towards Alderman Kelly, concluding with this question: supposing you were married to Mr. Newton, and Mr. Alderman Kelly offered to marry you, would you think it right to consent? If I had ever thought of Mr. Alderman Kelly, I should give up all thought of him if I knew I was married to Mr. Newton.—She was also questioned on religious matters, and returned simple and apparently rational answers. Questions were put to her about numbers, but she shewed great reluctance to commit herself by replying. How many hundreds are there in a thousand? I am so afraid of making mistakes that I cannot say. I don't know. I have seen about two dozen doctors, and I cannot reply.—Do you know Dr. Haslam? Yes, and I do not like him; he frightened me so about a madhouse, that my face became as white as a sheet. A watch was now put into her hand, and she told the hour correctly—a quarter past five. Have you read your catechism? I have not looked at it lately. I have not set foot in a church these two months. She was again questioned about figures. Unfortunately (said she) I was never obliged to do my tables; and when I felt unwilling, my grandfather sent his compliments to the governess that I might be excused. Suppose you were to live by yourself, and keep a carriage and servants, how much would it cost you a year? I do not know; I thought of going to live with an old lady in the country. She, after this, explained to the Commissioners various French phrases, and turned some English phrases into French. She said she told Mr. Bowling some secrets, as an old friend, which she would not repeat before the Jury. A Testament was put into her hand, and she read some verses clearly and emphatically. Are you in the family way (asked one of the Commissioners)? Are you a doctor? was the reply. As you are

not a doctor (she continued), it is no use to tell you.—Should you like to take another trip to Gretna-Green? No, sir; I think one journey was quite enough.—How old are you? I am twenty-one; I was twenty-one last November.

The Commissioners now returned to Court, and adjourned the inquest.

SIXTH DAY, *Monday, July* 9.

The will of the late Alderman Crowder was read, by which the principal portion of the personal property is left to Miss Bagster, as well as his other estates, upon her marrying with the consent of her guardians; and even with that consent her husband must have no control over her fortune. The sum of 20,000l. left to Mrs. Bagster, is also to descend to Miss B. on her mother's death. The whole property bequeathed by the will was sworn under 80,000l.

Mr. Kelly now summed up the evidence for the Commission.

Mr. Law addressed the Court in reply; and at great length commented on the evidence.

SEVENTH DAY, *Tuesday, July* 10.

Mr. Law resumed his address, and went through the entire of the examination of Miss Bagster taken before the Commissioners on Saturday; and shewed it to be sufficient and highly satisfactory for his client's cause in opposing the petition.

EIGHTH DAY, *Wednesday, July* 11.

After some deliberation on the part of the Court and Jury, whether there should be further evidence called, Mr Law having declined calling any witnesses, the Commissioners decided that additional evidence was necessary.

Dr. Morison examined by the Court.—I saw Miss Bagster four times during the last month. On my first visit (June 24th), she received me politely; said she was in good health, and that she was a little deaf, as she thought, from nervousness. I proceeded to ask her some questions. She told me she had neglected her arithmetic, and was not fond of it. She talked about the Opera, Paganini, Sir W. Scott, and the exhibition; of a journey she had once taken to Wales. She asked me what was a mad-doctor, and if I was one? I told her no; that I cured all diseases if I could. I then asked her if she had any money in her purse? She said, no; I have no occasion for any, for every thing is bought for me. On my second visit she shook hands with me, and asked after the health of my daughters. She complained of being very nervous, from seeing so many persons and having to answer so many questions as to her state of mind. She said that Dr. Haslam had frightened her very much, by telling her he was a mad-doctor. On asking her how she became so defective in her arithmetic, she said that her grandfather was very fond of her, and indulged her too much, and would never allow her to be teased about it. She said that she could read French, and "Telemachus," fluently. Talking of her trip to Gretna-Green, it was a foolish thing, she said, but you know one reads such things in novels; but I am sure I would not have done so if I thought there would have been all this trouble about it. After some further conversation, she asked me, What do you think of my capacity? I told her that I considered her to be very deficient in her arithmetic, but that I did not think her possessed of less sense than many young ladies of her own age. She asked me if I thought she had as much capacity as was necessary? I said I thought she had. Speaking of her fortune, she said her mother never let her have any money—not even a penny to give to a beggar; and that she had had no opportunity of knowing the value of money. After this, she asked a great many questions about the law proceedings which were about to take place; and, finally, without any question, said, "I have been very violent, and passionate, and undutiful to my mother, for I have sometimes pulled and slapt her, and I am very sensible of the impropriety of my conduct." My third visit was a very short one. I examined her again upon figures: she put down the first four figures under each other, and in a little time made out ten. She complained of being unwell, from the effect of seeing so many doctors. At my last visit I said, now you are married, how do you think you ought to sign your name? She said I think it ought to be Newton; but those about me say it was not a marriage, as it was not done in church. I am told by those about me, particularly by Miss Clayton, that Alderman Kelly is in love with me, and will marry me in three months, and I may be Lady Mayoress next year. I said, well, do you think that your marriage with Mr. Newton is not a good one, that you may marry Alderman Kelly? She said, no. I asked her what would her marrying be? She replied, adultery. I have directed much of my attention to insanity of late years, and have written two works on the subject. I am physician to the Surrey Lunatic House, and consulting physician to the Middlesex Lunatic Asylum. Miss Bagster is a little deaf, and in that respect only is different from other young ladies. I did not observe the slightest disposition to laugh without cause; she had not the unmeaning laugh and titter of those who are weak in mind. The statements she made I believe to be true, believing her to be a reasonable creature, and that she would not deceive me. I do not believe that her governess had fallen upon the proper mode of instructing her, and I would undertake in six months to teach her arithmetic and the use of money. She has begun to think now, and her mind is more opened than it was a

year ago, and I think if pains were taken with her, she might be instructed. Such a communication as she is represented to have made to Captain Kelly was very indecent and strange, but in my opinion not inconsistent with sanity. I think that late occurrences, coupled with the repeated conversations with the medical men on these indelicate subjects, have so lessened her sense of modesty as to account for it. A deficiency of education will account for all the appearances observed in Miss Bagster. Her incompetency to manage her affairs arises, not from unsoundness of mind, but from ignorance. She is capable of instruction, so as to be able to manage her affairs. The indulgence of her grandfather, the conduct of her mother towards her, and the frequent change of her teachers, were calculated to produce the results which we see. I hold her to be a responsible agent. She has this proof of soundness of mind, that she is sensible of the deficiencies under which she labours; one test of non-insanity is a consciousness of deficiency.

Dr. Roots examined.—I visited Miss Bagster twice. At my first visit she behaved with great courtesy, and gave very rational answers about Scott's novels. I put some questions to her about property, which I endeavoured to explain in a short way. I asked her, if she had 10,000*l.* in the four per cents. what ought to be her income? She replied, 100*l.* per annum. I asked her how she should live on 100*l.* She said she should take a great house at the West-End, and keep a number of servants; she would pay her lady's maid 30*l.* a year, and six other servants 20*l.* a year each—all out of the hundred. But she doubted if she could keep a carriage at the same time. She knew not how many shillings were in a sovereign, nor how many pence in a shilling, nor how many halfpence or farthings there were in a penny. To repeated questions of this sort she said, I fear I am very stupid, and never can do any thing of this sort.

NINTH DAY.—*Thursday, July 12.*

Dr. Roots's Examination continued.—I put down a number of figures on paper, but she was unable to read them off, though she could tell their names separately. I inquired if any medical gentlemen were to call next day, and was informed that Dr. Haslam was expected. As soon as Miss Bagster heard this, she turned to her attendant, and said, Oh! do not let him come; say I have gone out in the carriage. I said that would be a story, and would it not be wrong? She said yes, and knew that the Bible said it was wrong; but when a person gets into a scrape, it is the only way of getting out of it, and then there was no harm in it. She appeared to me to be deaf. I said to her, "having gone off willingly, and promised to love, honour, and obey Mr. Newton, and admitted you liked him, did you not consider you were bound to him, both my moral and religious obligations?" She said, "no, for I was not married in a church." Among other questions I asked her what would she give for a quartern loaf? She said 2*s.* She was aware that 20*s.* made a sovereign, and 20*d.* 1*s.* 8*d.*; but she said that fifty sovereigns were twice as much as a 50*l.* note. Some simple questions were put to her about the New Testament and English history, which she answered. She complained of her mother, for not being kind to her. I asked her what would be the expense of a carriage and pair of horses? She said, I should think from 20*l.* to 30*l.* a year. From the feeble state of her reasoning power, united with her total ignorance of the value of property, I consider Miss B. not capable of taking care of her person and property, and that she is of unsound mind.

Dr. Haslam examined.—I saw Miss Bagster three times. On the first visit, I saw Mrs. Bagster, who said that if Nelson Newton, (the brother) or any other respectable young man, asked to marry her daughter, he should have her consent; but Raymond was her detestation, and he should have her heart and life's blood before he should have her daughter. I was introduced to Miss Bagster as a friend of Alderman Kelly: I looked at her very attentively, and her countenance bore no mark of imbecility whatever, nor did she laugh nor smile without occasion; her eye was fixed when engaged, and her articulation was correct. I found her, however, deaf to a certain degree. I asked her if she had been religiously brought up. She said I have been a frequenter of the church, and an observer of its forms. I asked her if she had learned music; she said "I have, but have made little progress in it." She then said, I have been very neglectful of arithmetic. This led to some simple questions about money matters. She talked about the theatres and books, for she said she was a novel reader, and the last novel she had read was *Eugene Aram.* At my next visit, (when she recollected me immediately) I said to her, "when your grandfather was Lord Mayor, were you neglected and put out of the way, or were you introduced to the company that visited him? Her answer was, "I was never put out of the way, but always introduced." Were you at the feast at Guildhall on Lord Mayor's day? Yes, and introduced to Lord Wellington. Did you dance at the ball? Yes; with a gentleman who appeared to be an officer; I danced a great deal, till I was quite tired. I then said, Madam, did Lord Wellington or your partner treat you as if you were a fool and an idiot? She answered, No; they treated me with great respect. Did your grandfather ever treat you as a fool? She answered, Never. Did you know the gentlemen who lately came and asked you a variety of questions? No; but

I have been told they were friends of Alderman Kelly. I said, Madam, you have been deceived; they are all of them doctors—mad-doctors, and I am one myself. She here became very much affected and wept, and said, I hope, sir, you do not think me a fool? I said, by no means. She said, I know that my grandfather's fondness spoiled me: he told me he would leave me a fortune, and I am sensible that I have neglected my learning; I never liked figures, but at one time I could pretty well go through the multiplication table; and without being asked by me, began with twice one is two, and went on to twice ten, twenty; but no further. She insisted that she was not married, as the ceremony was not performed in a church. I said, Madam, do you wish to know the actual situation in which you are placed? Yes. Then, madam, in a very few days you will be brought before a jury of gentlemen, who are, by directions of the Lord Chancellor, to inquire whether you are a fool and an idiot, and, although of age, incapable of being married to any other person, or using the fortune left you by your grandfather, and deprived of the liberty exercised by a human being. Some of those doctors who visited you as friends of Alderman Kelly, have stated you to be incompetent, and a fool. She shed tears, and took my hand with great earnestness, and in a plaintive tone, said, "You knew my dear grandfather; I hope, sir, you will be my friend." She then said she felt very nervous, and requested, if I did not object, to have half a glass of wine, which she took. I impressively recommended her to speak the truth when before the jury. At my next visit I opened a Prayer book, and desired her to read the seventh commandment, which she did correctly. Do you know, I said, what is meant by committing adultery? Her answer was, when persons are married they shall not have any thing to do with any other man than their own husband. Of her own accord she lamented her deficiency in arithmetic, and her ignorance of the value of money. I then said to her, Do you think you could soon acquire it under proper tuition? She took up those words—Yes; under proper tuition, being allowed to have money and keep accounts, I have no doubt I could learn like other persons: my grandfather was so fond of me that he would not allow the teachers to snub me for neglecting my lessons. From these three interviews I am able to form a correct opinion of her state of mind. She is not a lunatic, she is not an idiot, nor is she of unsound mind.

Mr. Pollock: Is she of *sound* mind? I never saw any human being who was of sound mind. That is no answer to my question: is she of sound mind? I presume the Deity is of sound mind, and he alone. Is that your answer, sir? I presume that the Deity alone is of sound mind. How many years have you been a mad-doctor? About forty. Where did you learn that the Deity was of sound mind? From my own reflections during the last fourteen years, and from repeated conversations with the best divines in the country. Is Miss Bagster of sound mind? Competently sound. Is she capable of managing herself and her affairs? I do not know what affairs she has to manage. Is she competent to act as the mistress of a family? When properly instructed, I think she would be: I mean to state that when certain defects have been supplied, she would be as capable to conduct her affairs as any other woman. Do you understand that with the exception of the defects in education, she is competent to the ordinary affairs of life? I do. Do you think that a part of her disabilities might be removed by education? I am persuaded of it. Supposing she was taught in the best manner, and by the ablest masters, what length of time would it take? You must state the quantity of learning to be obtained; she is particularly deficient in arithmetic. And what else is she deficient in? Music. Those qualifications are not necessary to enable her to manage the affairs of life? They are certainly not essential. How often have you given evidence before commissioners of lunacy and a jury? I cannot tell; I don't know. Have you any notion? Notion is very much like knowledge, and I don't know. Have you any idea? An idea is a visible perception, and a direct recollection. Have you any belief? I cannot say that I have any belief, for that is a direct recollection. Can you, from any faculty of your mind, give information on the subject? Many times and oft I have given evidence on a commission of lunacy; but when I was first examined, or how much time has intervened since, I cannot tell. I did not visit Miss Bagster to repeat arithmetical questions, because she told me she knew nothing about them. I did not go there to puzzle her, though I could easily have done that with many others besides. She is competent to learn arithmetic, but she has not been allowed the materials—I mean money, the great source of all arithmetic.

The business of the day was concluded by the evidence of Wilson, who accompanied the parties in their elopement.

TENTH DAY, *Friday, July* 13.

Miss Emma Newton proved that Miss Bagster was prepared for the elopement, and not taken by surprise.

Mr. Alderman Kelly considered Miss Bagster childish, imbecile, not possessing the proper reasoning powers, and by no means capable of managing her own affairs. He should pause, he said, if she was his child, before he would consent to her being married, even to a gentleman every way her equal. He gave two instances of her violence, (the only unruly conduct of which he

ever knew her to be guilty,) both subsequent to the elopement.

Mr. Woodfall concurred with the Alderman in his opinion of Miss B.'s mind.

Dr. Munroe recalled, said he did not think that if Miss B. had been placed under a mad-doctor, any benefit would have resulted.

The Chief Commissioner informed the court that the evidence was now closed.

Mr. Pollock addressed the jury at great length in favour of the petition.

ELEVENTH DAY, *Saturday, July* 14.

Mr. Follett, on the part of Mr. Newton, addressed the jury, and argued strongly that notwithstanding all the evidence, it was not made to appear that there was ground for a commission or a verdict in the case.

The Chief Commissioner then summed up the evidence, and the jury, after retiring for about half an hour, brought in the verdict, that "Rosa Matilda Mary Bagster is, since the first of November, 1830, of unsound mind, so that she is not fit to take proper care of her goods, chattels, messuages, tenements, &c."

DEATH OF MR. WOOD, FROM CHOLERA.

(From a Correspondent.)

MR. JOHN WOOD, of Bridge-Street, Blackfriars, fell a victim to cholera on Monday the 9th inst. after less than twelve hours illness.

This promising young surgeon, on the morning of his death and previous evening, had been assiduously attending some of the persons in Bridewell, in which establishment cholera had just appeared. On Monday morning he felt indisposed, and had one or two loose motions. He was enabled, however, to pursue his ordinary avocations till twelve o'clock, when an intelligent friend observing a remarkable change in his countenance, sent for Mr. Lawrence. On his arrival, Mr. L. found his friend and former pupil in a state of collapse, and with symptoms that left no doubt on his mind of the nature of the disease. Dr. Tweedie visited him shortly after.

The disease resisted every effort to arrest its progress, and at ten o'clock in the evening Mr. Wood expired.

Those who knew Mr. Wood will testify to his intrinsic worth. Few young men of the present day gave greater promise of future eminence in their profession, and no one excelled this amiable young man in moral worth, and in the most exemplary discharge of every social as well as professional duty.

COLLEGE OF SURGEONS.

MR. VINCENT has been appointed President, and Mr. Guthrie and Mr. White, Vice-Presidents, for the ensuing year.

CHOLERA IN THE METROPOLIS.

WE stated last week that cholera was progressively on the increase in the metropolis, and we are sorry that we have nothing of a different nature to communicate on the present occasion. Up to the time of our writing (Friday 20th), the number of cases occurring under care at the various parochial establishments has undergone a very considerable augmentation, and continues still on the advance (though not with the rapidity that some apprehend); while the experience of almost every medical man must have convinced him that the disease is no longer confined to the more destitute classes of the community. It is quite evident from what has passed in the House of Commons, that ministers are extremely averse to the publication of reports with regard to the disease in London; and, while we acknowledge the question to be one of much difficulty, we cannot but look upon it as bordering upon simplicity, to admit, as a member of the Government did, that concealment is requisite for the interests of trade,—as if this admission were not calculated to spread general alarm, and to produce the very effect which the said concealment is intended to avoid.

It would have been satisfactory to have been able to state that any mitigation had taken place in the malignity of the cases now occurring,—but the very reverse is the fact, the rate of mortality in cases of cholera with collapse having recently risen from less than one in three to more than one in two. The disease is also more diffused than it was, but still a very great majority of the cases occur in the same localities as before: we may particularly mention Lambeth, Southwark, Bermondsey, and Whitechapel.

Though we admit that cholera prevails to a considerable extent, yet it is to be kept in mind, that the months of July and August never pass in London without a great many cases of severe stomach and bowel complaints, and that all such are now magnified into cholera by the fears of the patient or his friends.

METEOROLOGICAL JOURNAL,

July 1832.	THERMOMETER.		BAROMETER.	
Thursday . 12	from 53 to	75	29·85 to	29·83
Friday. . . 13	53	75	29·74	29·72
Saturday . 14	54	69	29·95	29·99
Sunday . . 15	46	73	30·20	30·29
Monday . . 16	51	73	30·22	30·14
Tuesday . 17	54	82	30·11	30·04
Wednesday 18	50	69	30·00	30·04

Prevailing winds W. and S.W.

Except the three first days, clear; rain on the 12th and 14th. A storm of thunder, accompanied by vivid lightning, in the evening of the 12th.

Rain fallen, ·95 of an inch.

CHARLES HENRY ADAMS.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

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OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 28, 1832.

ON OCCLUSION OF THE BILIARY DUCTS.

By W. Twining, Esq.

The severe acute diseases of the liver are for the most part sufficiently distinct in their character, and the result of medical treatment is generally satisfactory, if the patients apply early for advice, and follow with fidelity the treatment directed for their cure. Quite the reverse of this happens in many of the chronic cases, where morbid changes are slowly taking place in the functions and structure of the liver: some of these are the result of long residence in tropical climates; others depend on excesses in food and drink. The diagnosis of many of these affections is very difficult, and the proper treatment doubtful; but the difficulty of the subject ought not to deter us from endeavouring to add to our knowledge, although it may require much time and patient industry to collect sufficient facts for the establishment of positive rules to direct our treatment of those diseases. The following cases of occlusion of the biliary ducts are placed before this society, in hopes that they may lead to useful discussion on some of the obscure diseases of the liver, and elicit the communication of facts that may enable us to ascertain if there be any symptoms denoting the early stages of such diseases, whereby we may form a just discrimination of their nature, and follow a correct system in their treatment. In these cases, the early period of disease appears to have been inflammatory, having been in two of the patients attended with more or less pain, and relieved by leeches.

Case I.—Keating, aged 28, a slight made and delicate European, who had been employed in Bengal for several years as an indigo planter, came under my care on the 18th March, 1828. He had been suffering from slight pyrexia, and constipation for two days; there was general tension of the belly, and enlargement of the liver, with pain in that organ, and at the right side of the neck. Leeches were repeatedly applied, and mercurial purges administered, which operated very freely; and the mouth was affected by the calomel on the morning of the 20th March. The pain in the right side was moderated, but the liver remained large. Either aloes, castor oil, or senna and salts, were given daily until the 4th April, so as to keep the bowels very free. He had then a return of occasional pain in the right hypochondrium, and at the right side of the neck. His mouth having not quite recovered from the effects of the calomel, he was ordered to take blue pill, colocynth, and aloes, every night.

On the 7th April there was some pain in the region of the gall-bladder, in consequence of which leeches were again applied daily until the 11th, when the pain had nearly subsided. A slight enlargement of the spleen was now observed, and it continued progressively to increase until he died, on the 4th July.

He suffered during this period from frequent returns of fever; and latterly had a troublesome cough, with mucopurulent expectoration, and difficult respiration, from the encroachment of

the spleen on the left side of the chest; the enlargement of the liver having entirely subsided. After the spleen began to enlarge, the use of mercury was suspended, and his treatment consisted in the repeated application of leeches, and the exhibition of purgatives, with quinine, when required by returns of fever. All the remedies usually employed in cases of tumid spleen were tried, with careful attention to diet, but without effect.

On dissection, the liver was found diminished in size; of a livid red colour, inclining to brown; its consistence was soft and doughy. Some concretions resembling yellow soap were found in the pori biliarii. The cystic duct was obliterated, a mere membrane of extreme tenuity remaining in its place; so that we may believe this morbid change to have been of long duration. The gall-bladder was shrivelled, its coats thickened, and it contained a small quantity of dark coloured thin paste, like a mixture of charcoal and oil; insufficient in quantity to distend the diminished gall-bladder, which, therefore, had the lax shrivelled appearance just noticed. There was abundance of yellow bile in the duodenum. The spleen was much enlarged, indurated, and friable in texture.

I have ascertained that Keating had ague in January 1828, attended with cough and oppression at the præcordia, and some tenderness at the epigastre on pressure; he was then relieved by venesection, leeches, and purgatives. But I can learn nothing of his state of health previously; he was a man in distressed circumstances, from failure in business. The enlargement of the spleen seems to have taken place on subsidence of a tumid liver; and under such circumstances, I have always found affections of the spleen most intractable. Considering the relations of the venous circulation in these two viscera, it is surprising that we do not find the spleen invariably become enlarged when the liver is indurated, and also in all cases of obstructed or torpid circulation of the liver.

In the early part of Keating's illness, and before any preparation of iron was given, the stools do not appear to have differed from those of a healthy person, in whom the cystic duct is pervious. After the employment of purgatives, combined with the preparations of iron, in the treatment of the tumid spleen, the stools were of a dark colour, which most probably depended on the medicine.

Case II.—Macan, a delicate recruit, aged 20, arrived from England on the 30th May, 1829; he had then a diseased knee-joint. While in hospital, observing a state of rest, and living on a mild diet, pulmonary disease shewed itself, and abscess of the liver formed, of which he died on the 7th September.

On dissection, the right lung was found adherent to the diaphragm; the liver was enlarged, its convex surface adherent, and there was an abscess at the upper part, which had opened into the right lung. The gall-bladder, and capsule of Glisson, were covered with an adventitious membrane, apparently not of recent formation. Gall-bladder elongated and flaccid; contained about ʒiiss. of a straw-coloured fluid, like varnish. Cystic duct closed, and coherent at its upper part, so that the wire of a very small child's catheter could not be forced along the canal. There was a small indurated scrofulous tumor near the cystic duct, which, in the natural position of the parts, had probably, by its pressure, determined the closure and cohesion of the duct. There was a vast number of enlarged and indurated glands in the mesentery and mesocolon. The cœcum was enlarged, its coats thickened, and ulcerated internally; universal adhesions in right side of the chest; right lung decreased in size; it contained many suppurating tubercles. A few adhesions in left side of chest, not recent; and several tubercles in the left lung, in an indolent state.

From the appearances of the morbid changes in the gall-bladder and cystic duct in this subject, we may conclude, that the closure of the duct was not of very recent formation. I find, by reference to the daily reports, that the patient passed either black or green stools on the 15th, 16th, 17th, 18th, 25th and 26th August; which colour cannot be ascribed to the presence of cystic bile, now the state of the cystic duct is ascertained.

Another instance of closure of the cystic duct will be found in the Appendix to vol. 1st of Mr. Annesley's great work on the Diseases of India. A patient of intemperate habits came to hospital on the 12th February, affected with vomiting, purging, and a sharp

pain at the epigastric region; he had suffered from those symptoms for several days before. During the first days that he was in hospital the alvine evacuations are reported to have been *black;* and for the next sixteen days they were mostly of a dark-green, and sometimes of a black colour. The patient died on the 8th March, twenty-four days after admission. On dissection, the cystic duct was found "impervious, and reduced to a cord," which is beautifully represented in the 2d figure, Plate 21, of the work referred to. As this man was freely purged on the first days of his illness, so as to remove any old accumulation from the intestines, we cannot attribute the dark-green colour of his stools at a later period to the presence of cystic bile; and as the duct was *reduced to a cord,* the obliteration can hardly be supposed of very recent occurrence.

When we find practitioners of eminence and experience speaking of the morbid state of the alvine evacuations, as characterised by the presence of cystic bile; and recommending the continuance of a particular system of treatment, until the cystic bile is purged off, and there is an appearance of hepatic bile in the stools; it becomes a matter of some importance to ascertain whether we can be sure of distinguishing the appearance of cystic bile with such degree of certainty as to make that symptom a guide for our practice. By inquiries as to the appearance of the alvine evacuations, which may have been observed in cases where the cystic duct is found obliterated, and in subjects where no gall-bladder existed, we may hope to make one step towards precision on this point; for we shall often have occasion to notice the dark colour of the stools (which is usually ascribed to the presence of cystic bile), in cases where the cystic duct is closed. Mr. Abernethy, and many other authors, have long ago asserted, that such dark coloured evacuations are morbid secretions of the intestines.

Dr. Cholmeley relates a case in the 6th volume of the London Medical Transactions, somewhat bearing on this question. The patient, a child, died after an illness of five weeks; among other symptoms of disease, a "vomiting of green bile" is mentioned. Dissection after death shewed that no gall-bladder existed in this subject.

The foregoing facts would shew, that we have no just ground for supposing that black or green evacuations depend always on cystic bile. Indeed we have sometimes reason for doubting whether the dark colour of the stools depend on bile of any description. Although we may admit that the dark colour of the stools is, in many instances, a good criterion for directing our practice, it appears that we should hesitate to acknowledge that colour of the alvine evacuations as evidence of the presence of cystic bile: therefore, a revised phraseology may be expedient in describing such symptoms; and a modified opinion as to the cause of them, may occasionally influence us regarding the remedies prescribed. Old, pale, and leucophlegmatic subjects, suffering from impaired digestion and torpid bowels, are constantly imagining their complaints bilious, on account of the dark colour of the evacuations; but the propriety of large and repeated doses of mercury in such cases may be sometimes doubted.

Case III.—Mary Arbin, a small and delicate woman, aged 20, was sent to the General Hospital on the 13th June, 1829. She was of English parents, but had resided all her life in Bengal; her habits had been intemperate for several years, and gin her favourite liquor. She said she had been subject to pain in the right side for about a year, to such a degree that the ordinary pressure of her clothes was frequently distressing, and obliged her occasionally to apply leeches; but she had taken very little medicine, and followed no regular plan of treatment. Said she had ague for five days in January; and since that time the catamenia had been irregular, appearing for a day or two once in five or seven weeks.

On admission, the liver was found very large, occupying the whole right side of the abdomen, as low down as the iliac region; the left lobe was also enormously enlarged, extending below the left false ribs. Pressure over the liver caused pain, but she never had any pain in the right shoulder. The patient was emaciated and pale; conjunctivæ remarkably bloodless, although the eyeballs were vascular. There was no pyrexia, bowels regular, and stools of pale grey colour. She had suffered from nausea and vomiting for fourteen days before she came to the hospital.

Leeches were repeatedly applied to the right side, in small numbers; and she was ordered to take purgatives, with calomel and squills, twice daily; which operated sufficiently.

June 17th.—No amendment. Calomel omitted; aloes, combined with pil. scillæ c. and pulv. scillæ, were given every night, and she had ten grains of rhubarb, and as much sal. polychrest every morning.

22d.—She was freely purged by the above medicines, and thought herself better; the liver was smaller. The pills frequently produced vomiting; therefore extract. colocynth. comp. was substituted for the pulv. scillæ, and a large blister was applied to the belly.

By these means the liver became somewhat smaller, but the patient gradually sunk into a low state, attended with despondency and indifference to life, and died on the 5th July, 1829, twenty-two days after admission.

Dissection.—Subject emaciated generally; but there was some fat above the abdominal fasciæ, which, as well as the cellular structure in other parts of the body, was of a pale yellow colour. The liver, though decreased in size since the patient's admission into hospital, was still enormously large, and of a pale fawn colour, being in firmness and colour not unlike boiled cow's udder; its structure was granular, like the pancreas, and though firm when pressed, it could be easily torn. The gall-bladder was much shrunk in size, and quite flaccid, containing a very small quantity of green bile. The cystic duct was pervious, but the hepatic duct completely closed, and degenerated into a minute film of membrane.

This was one of those cases which, as Van Swieten says, "requires to be treated with the utmost gentleness; and is hardly ever to be cured." It affords an example of enlarged liver, in which, if any power of secreting bile existed, the ordinary influence of mercury in exciting that secretion must have been injurious, in consequence of the obliteration of the excretory duct. The calomel taken in the first four days after admission was certainly of no service, but rather the reverse, and therefore was omitted.

In a country where the frequent occurrence of liver diseases affords opportunity to trace the progress of those complaints, and especially when we consider the numerous hands with which the society works, we might with much advantage endeavour to ascertain in what chronic diseases of the liver the use of mercury may be deemed of doubtful propriety, and inquire what are the most obvious symptoms of those diseases. We might also endeavour to ascertain what are the circumstances in the patient's habits of life, or local residence, which appear to lay the foundation of the pale soft degeneration of the liver, as well as of the pale hard (or cirrhose) change of structure in that organ. In what temperaments do those kinds of disorganization most frequently occur? What are their characteristic symptoms? Is jaundice a frequent attendant on either of these forms of disease; and are these pale degenerations of the liver, when existing, generally as much benefitted by mercury as most other forms of liver disease?

P.S.—Since the above paper was presented to the Medical Society, I have met with another case of obliteration of the ductus communis choledochus, and one of closed cystic duct; short accounts of which are subjoined.

Case IV.—About the middle of January 1831, I was requested to see a man who had been for six years past addicted to drunkenness, but for several months had fallen into such a state of sottish indifference, that he was never sober when he could get liquor; and had become so nearly helpless, that he would have been starved if his friends had not humanely taken care of him. He was 44 years of age, of light complexion, and below the middle stature, but pale, emaciated, and miserable. I was informed he had been twenty-seven years in Bengal, chiefly in the upper provinces, in the middle station of life, and for many years sober, active, and industrious, but never obliged to follow any laborious occupation. When I saw this patient he appeared quite exhausted, and worn out by protracted intemperance, without any very distinct marks of acute disease. He remained in bed almost in a state of fatuity. When questioned as to his ailments, he made very little complaint, except occasionally of uneasiness in his belly; for which some colocynth and blue pill were given at night, and a mild aperient next morning. His stools were passed in bed, and removed before my visit.

I was informed they were of a very dark colour, and glutinous. Small quantities of soup, or sago, and wine and water, were given. His urine and stools continued to be passed in bed, and he died in eight days.

On dissection, some old adhesions were found in the right side of chest. The liver enlarged and indurated; the convex surface rough and tubercular; its texture hard, granular, and tough; in colour and firmness not unlike boiled cow's udder—being of a pale yellow colour, with slight pink tinge, and bleeding little when cut. The intestinal extremity of the ductus communis choledochus obliterated for 1½ inch; gall-bladder large and lax, contained ʒiiss. of pale yellow fluid, like glue. The peritoneal surface of stomach and intestines pale, and void of vascularity; the interior of reddish brown colour, and covered with tenacious viscid mucus of the same colour. Root of mesocolon and mesentery very vascular, and ecchymosed in some parts.

Case V. was obliteration of the cystic duct. The patient, J. W. King, a stout and fat man, of light complexion, 45 years of age, and 19 years in India; for several years prone to intemperance, and much more so for 18 months before his death. His usual drink was rum. He was brought to hospital on the 14th May, 1831, labouring under ardent fever of four days' duration, attended with slight incoherence. There was high arterial action, heat of skin, and jaundice; a tumid belly, and some of the symptoms of delirium tremens. Altogether, his case required antiphlogistic treatment; notwithstanding which, he died on the morning of 17th May.

On dissection, the subject was observed to be rather fat; skin and eyes jaundiced. Arachnoid over the upper part of the hemispheres opaque and thickened, having some albuminous and serous deposit between it and the pia mater. There was about ʒij. of serum in each lateral ventricle; and a considerable albuminous deposit in the texture of the plexus choroides, on each side. Lungs gorged with blood, and heavy; some old adhesions in both sides of the chest. Liver of light drab colour, slightly indurated, and bleeding but little when cut. Gall-bladder small, flaccid, and covered with a firm, thick, false membrane; it contained a spoonful of thick, viscid, black, glutinous matter, more like tar than bile. The cystic duct was closed through its whole length, appearing as a small white filament; the superior gland in the capsule of Glisson indurated and enlarged. Hepatic duct enlarged, and containing some thin bile, like pale yellow transparent varnish; there was abundance of this sort of bile in the duodenum and upper portion of small intestines. Coats of stomach pale, and little corrugated. Coats of duodenum much thickened and firm, but not much morbid vascularity about that part. The interior of stomach and small intestines was thickly coated with a tenacious white mucus, such as is found in many cholera cases*.

ON THE POWERS ON WHICH THE CIRCULATION OF THE BLOOD DEPENDS.

By A. P. W. Philip, M.D. F.R.S. L. & E.

It is remarkable that, notwithstanding the great importance of the circulation in the animal economy, the length of time which has elapsed since its discovery, and the constant attention it has obtained, there is hardly any department of physiology respecting which there appears to be greater uncertainty and contrariety of opinion, than the sources and the nature of the powers on which this function depends. I propose in the following paper, by comparing the principal facts on the subject, and by such additional experiments as seem still to be required, to endeavour to determine these points. Much has lately been written and many experiments have been made with this view, and it has become customary to look for the causes which support the circulation to other sources beside the powers of the heart and blood-vessels.

It has been supposed that what is called the resilience of the lungs—that is, their tendency to collapse, by relieving the external surface of the heart from some part of the pressure of the atmosphere, is a principal means of causing it to be distended with blood,

* From the Transactions of the Medical and Physical Society of Calcutta, Vol. V., just received.

the whole weight of the atmosphere acting on its internal surface, through the medium of the blood which is thus propelled from the veins into its cavities; and in this way it has been supposed that the motion of the blood through the whole of the venous part of the circulation is maintained. A similar effect has been ascribed to the act of inspiration, which it is evident must operate on the same principle; and this opinion has even been sanctioned by the Report of a Committee of the Royal Academy of Sciences of Paris*, and in this country by men whose authority is deservedly high; and the effect of these causes, it is asserted, is increased by the elastic power of the heart itself.

However successfully such opinions might be combated by reasoning on the data we already possess, as direct experiment is the most simple as well as decisive way of determining the question, as reasoning on physiological subjects has so often deceived, and the experiments may here be made on the newly dead animal, and consequently without suffering of any kind, I have thought it better that the point should be determined in this way, especially as it is by experiments, which at first view seem to countenance the foregoing opinions, that their supporters attempt to establish them, with the effect, as it appears to me, of withdrawing the attention from the powers on which the circulation actually depends, and introducing considerable confusion respecting a question so immediately connected with the phenomena and treatment of disease.

With a view, therefore, to submit the foregoing opinions to this test, the following experiments were made, in which Mr. Cutler was so good as to assist me.

Exp.—A rabbit was killed in the usual way by a blow on the occiput, and the chest opened on both sides so as freely to admit the air. The lungs were then inflated eight or ten times in the minute by means of a pipe introduced into the trachea; the circulation was found to be vigorous. On laying bare one of the femoral arteries, it was observed to pulsate strongly; and on wounding it, the blood, of a florid colour, indicating that it had undergone the proper change in its circulation through the lungs, gushed out with great force; and on introducing the hand into the thorax, the heart was found to be alternately distended and contracted, as in the healthy circulation.

Exp.—All the vessels attached to the heart in the newly dead rabbit being divided, and the heart removed, it was allowed to empty itself. Its contractions continued to recur, and in their intervals it assumed a perfectly flat shape, proving that the elasticity of the heart in this animal is so small, that it cannot even maintain the least cavity after the blood is discharged.

It appears from these experiments that the circulation was vigorous when none of the causes to which the motion of the blood in the veins have been ascribed existed. In the first experiment the chest being freely opened on both sides, so that the play of the lungs on inflating them could be seen, all effect on the heart, either of the resilience of the lungs or the act of inspiration, was evidently prevented; and in the second, it was proved that no sensible elasticity of the heart existed; yet while artificial respiration was performed, we could perceive no abatement in the vigour of the circulation.

It is to be observed, that all these means can act only in one way in promoting the circulation, namely, by giving to the heart the power of suction—that is, by producing a tendency to vacuum in its cavities, in consequence of which the pressure of the atmosphere propels the blood from the veins into them, that of the arteries being prevented from returning to the heart by the valves at their origins. But all, as far as I know, who have either made experiments with a view to prove the supposed effect of these means on the circulation, or who have sanctioned the inferences from such experiments, have overlooked the circumstance that the veins being tubes of so pliable a nature that when empty they collapse by their own weight, whatever may be said of the effect of such causes in favouring a horizontal or descending motion of the blood, it is impossible that an ascending motion could be produced in them on the principle of suction. As far as the heart may possess any such power, its tendency must be to cause the vessel to collapse, not to raise the fluid it contains.

That the resilience of the lungs as far

* Report on Dr. Barry's paper, by Baron Cuvier and Professor Dumeril.

as they possess this property, and the act of inspiration, tend to dilate the heart and large vessels within the chest, is evident; but the former is very trifling, if it exist at all, except as far as it depends on the mere weight of the lungs; and the latter in common breathing is little more efficient, although the effect of respiration on the brain, when any part of the cranium is removed, sufficiently attests that it has a certain effect. When the breathing is so laborious as essentially to influence the circulation, it evidently tends to derange the regular flow of the blood towards the heart, inspiration of course acting interruptedly; whereas it is only necessary to inspect the chest of any of the more perfect animals immediately after death, and while artificial respiration is being performed, provided death has not been caused by great loss of blood, or an extreme and instantaneous impression on the nervous system, to see that the blood flows uniformly towards the heart with no interruption but that which the contraction of the heart itself occasions.

The elasticity of the heart is greater in some animals than in the rabbit; but it is in all cases very inconsiderable. The heart of the tortoise is the most elastic I have examined; yet even it may be compressed during its diastole by a force not sensibly greater than is sufficient to compress other muscles in a state of relaxation. Besides, the auricles possess little or no elasticity; and whatever the elasticity of the ventricles may be, it can have no effect on the blood in the veins, because they receive their blood from the auricles which are contracting during the diastole of the ventricles. To these statements it may be added, that in many of the inferior animals the foregoing supposed causes of the venous part of the circulation evidently have no existence, and that, with the exception of the elasticity of the heart, they have no existence in the fœtal state in any.

We have just seen from direct experiment, that the circulation of the blood goes on as usual when all these causes have wholly ceased to operate.

I shall now take a rapid view of the facts which, as far as I am capable of judging, leave no room of doubt respecting the sources of the power on which this function depends.

It is so evident to those in the least acquainted with the animal economy that the contractile power of the heart is one of the chief of these sources, that it would be superfluous to enumerate the proofs of it; yet even this position has been denied, and that by a writer of no mean abilities. The opposite error, however, is the more common; and not a few have ascribed, and even still do ascribe, the motion of the blood throughout the whole course of circulation to the contractile power of the heart alone, although it would not be difficult to prove that to drive the blood through one set of capillary vessels, and still more through two or three sets of such vessels,—for in man himself, in one important part of the circulation, it is carried through two, and in some animals through three, sets of capillaries before it returns to the heart,—I say it would not be difficult to prove that to drive it through one set of capillaries, at the rate at which the blood is known to move, would require a force capable of bursting any of the vessels. But here, as in the former instance, it is better to appeal to the evidence of direct facts than to any train of reasoning; and there is no want of such facts to determine the point before us, some of which I formerly had the honour to lay before the Society, and others are stated in my Treatise on the Vital Functions. The most decisive is, that the motion of the blood in the capillaries continues long after the heart has ceased to beat, and the animal, in the common acceptation of the term, is dead, even in the warm-blooded animal, for an hour and a half or two hours, and it is not for some time sensibly affected by the heart's ceasing to beat; nor does this arise from some imperceptible impulse still given by the heart, because when all the vessels attached to this organ are secured by a ligature and the heart cut out, the result is the same.

That the circulation in the capillary vessels is independent of the heart, may be shewn by various other means. On viewing the motion of the blood in them, with the assistance of the microscope, it may generally be observed that it is moving with different degrees of velocity in the different vessels of the part we are viewing, frequently more than twice as rapidly in some than in others. Were the motion derived from a common source, this could not be the case. It is impossible, in the

motion of the blood in the capillaries, in the least degree to perceive the impulse given by the beating of the heart, which causes the blood in the arteries to move more or less per saltum, the motion of the blood in the former being uniform as long as they retain their vigour, and the necessary supply of blood is afforded from the larger vessels. I have found, by experiments very frequently repeated *, that the motion of the blood may be accelerated or retarded in the capillaries by stimulants or sedatives, acting not through the medium of the heart, but on these vessels themselves. Nay, so little effect has the action of the heart on the motion of the blood in the capillaries, that I have found that when the power of the capillaries of a part is suddenly destroyed by the direct application of opium to them, the motion of the blood in them instantly ceases, although the vigour of the heart, and that of every other part of the sanguiferous system, is entire †.

If the circulation in the capillaries be thus independent of the heart, it is evident that the influence of that organ cannot extend to the veins. On comparing the whole of the foregoing circumstances, is it not a necessary inference that the motion of the blood in the veins, like that in the capillaries, depends on the power of these vessels themselves? But that we may not trust to any train of reasoning, where it is possible to have recourse to direct proof, I made the following experiment, with the assistance of Mr. Cutler.

Experiment.—In the newly dead rabbit, in which the circulation was maintained by artificial respiration, the jugular vein was laid bare for about an inch and a half; a ligature was then passed behind the part of the vessel nearest to the head, and the animal was so placed that the vein was brought into the perpendicular position, the head of the animal being undermost, so that it was necessary for the vein, in conveying the blood to the heart, to convey it perpendicularly against its gravity. The ligature, which was placed at what was now the lowest part of the exposed portion of the vein, was suddenly tightened, while Mr. Cutler and myself observed the vessel. The blood in the part of the vein between the ligature and the heart was instantly and completely expelled, as the transparency of the vessel enabled us to perceive. The vessel itself wholly collapsed, proving that all its blood had entered the heart, so that to a superficial view there seemed to be no vessel in the part where a large dark-coloured vein had just before appeared. In the meantime, on the other side of the ligature, the vein had become gorged with blood.

In the foregoing experiment we see the blood rising rapidly against its gravity, where all causes external to the vessel on which the venous part of the circulation has been supposed to depend, had ceased to exist, and the vis à tergo was wholly destroyed by the ligature.

By a similar experiment, the power of the arteries in propelling the blood may also be demonstrated.

Experiment.—In a newly dead rabbit, the circulation being supported by artificial breathing, the carotid artery was laid bare for about an inch and a half. The animal was so placed as to keep the vessel in the perpendicular position, the head being now uppermost. A ligature was passed behind that part of the vessel which was next the heart, and Mr. Cutler and myself observed the vessel at the moment the ligature was tightened. The artery of course did not collapse as the vein had done in the preceding experiment; but the blood was propelled along the vessel, so that it no longer appeared distended with it. It was at once evident, from the change of appearance in the vessel, that the greater part of the blood had passed on in a direction perpendicularly opposed to its gravity. It is worthy of remark, that the blood of the artery was propelled neither so rapidly nor so completely as that of the vein, the cause of which will be evident in the observations I am about to make on the nature of the function and powers of these vessels.

When the whole of the preceding facts are considered, it will, I think, be admitted that the circulation is performed by the combined power of the heart and blood-vessels themselves, and that no auxiliary power is necessary for its perfect performance. Here, as in other cases, the more we study the operations of nature, the more direct and simple we find them. The resilient power of the lungs and elasticity of the ventricles

* My Treatise on the Vital Functions.
† Ibid.

of the heart, as far as they exist, favour the free entrance of the blood into these cavities, an office adapted to the feebleness of such powers, which, in many animals, we have seen, have no existence. Their operation is similar, but probably much inferior, to the elastic power of the arteries, by which the ingress of the blood suddenly impelled into them by the systole of the heart, is rendered more free than it would have been had these vessels tended to collapse in the intervals of its contractions. Had the blood flowed into them in a continued stream, and been carried through them by their own powers alone, their elasticity would evidently have impeded, not promoted, the circulation through them. Thus the veins, where these conditions obtain, are so pliable that they collapse by their own weight, and hence it was that in the preceding experiments the vein carried on its blood so much more rapidly and completely than the artery, which felt the want of the impulse it receives from the heart, that at once assists in propelling its blood, and through the blood stimulates the vessel itself. The action of the vein was perfect; it possessed all its usual powers, which reside in itself alone.

It only remains for us to inquire into the nature of the power by which the heart and blood-vessels maintain the circulation. Respecting the nature of the power of the heart there cannot be two opinions. It is evidently a muscular power. The structure of its parietes is similar to that of other muscles, and they obey all the usual laws of the muscular fibre.

Is the power of the vessels of the same nature? This is a question which has frequently been discussed. The chief arguments which have been adduced in favour of the affirmative are, the nature of their function; the fibrous appearance observed in some of the vessels, which is more evident in some other animals than in man; and the minuteness of most of the vessels, which, if they are muscular, accounts for the difficulty with which the muscular structure is detected in them. The chief arguments against the muscularity of the vessels have been, that they could not be made to obey an artificial stimulus in the way that the heart and other muscles are found to do, and that their chemical analysis gives no evidence of fibrin. Of the latter of these objections Dr. Young observes, that a part may be muscular although it does not contain fibrin, and refers in support of this opinion to the crystalline lens. The former of these objections no longer exists, the vessels having been found to obey both stimulants and sedatives as readily as parts more evidently muscular. It appears from many experiments related in my Treatise on the Vital Functions, that the action of the capillary vessels is as easily influenced both by stimulants and sedatives as the heart itself; and although the larger vessels are not so easily excited artificially as the heart and muscles of voluntary motion, yet several physiologists have succeeded in exciting them both by mechanical and chemical agents. But there is another argument in favour of the muscularity of the vessels, which, I think, may be regarded as no less powerful. I endeavoured, in papers which I had the honour to present to the Society, and which appeared in the Philosophical Transactions for 1815, to ascertain the relation which the heart bears to the nervous system, which is different from that of the muscles of voluntary motion. It appears from the facts there adduced, that this organ is not only independent of that system, although capable of being influenced through it either by means of stimulants or sedatives, and that even to the instantaneous destruction of its power; but that it equally obeys either set of agents, whether applied to the brain or spinal marrow; while the muscles of voluntary motion obey no stimulus acting through the nervous system, unless it be applied to their nerves themselves, or to the particular parts of that system from which their nerves arise. I found, from repeated experiments, that the vessels bear the same relation to the nervous system as the heart does, their power being independent of this system, but equally with the heart capable of being influenced by either stimulants or sedatives applied either to the brain or spinal marrow, and that even to the instantaneous destruction of their power. They in all respects bear the same relation to the nervous system with the heart, which affords the strongest argument for believing that their power is of the same nature*.

From the various facts stated or re-

* My Treatise on the Vital Functions.

ferred to in the foregoing paper, the following inferences appear to be unavoidable; — That the circulation is maintained by the combined power of the heart and blood-vessels, and that the power of both is a muscular power*.

HYDROPHOBIA.

To the Editor of the London Medical Gazette.

Lower Phillimore-Place, Kensington, July 9, 1832.

SIR,

I BEG leave to transmit to you the particulars of a melancholy case of hydrophobia, which terminated fatally in this place on Tuesday last; exactly three weeks, to a day, from the period of the unfortunate girl having been bitten. I regret I cannot enclose an account of the post-mortem examination: on the day of her death I expressed my anxiety to inspect the body, but I was informed that an inquest was to be held on it the next evening; and from this the jury and witnesses were not relieved until one A.M. on Wednesday. Preparations were complete by noon on this day for her interment. Thus I was disappointed. Not that it would, in all probability, have afforded any thing new, but that it would have enabled me to make my case complete. Two or three circumstances connected with this and similar cases are, I think, highly interesting and deserving of attention: I mean the peculiarity in the action of animal poisons upon the human body, and especially that from animals of the feline class, in originating what is the most striking feature of the disease—exquisite fear. I do not refer solely to fear of water, but of all objects whatever, as well as air, light, sound, &c. Another point worthy of consideration is, whether the discharge from the bladder, which occurred simultaneously with the introduction of fluid into the mouth, in any form and in the smallest possible quantity, is to be referred to the effect of fear upon the sphincter (if so, why did it not act also upon the sphincter ani?), or whether the muscular fibres of the bladder were specially acted upon by the single stimulus of the fluid upon the mouth, or by the united influence of this stimulus and fear. I would have added one or two remarks on the therapeutics of this disease, and especially on the inflammatory view which is taken of it by many authors; but I fear I have already encroached too far upon your pages by the length of this communication.

I have the honour to be, sir,
Your obedient servant,
FRANCIS BADGLEY.

Mary Ann Fergusson, æt. 11 years, 6 months, one of the children at the Kensington Parish School, was bitten, on Tuesday the 12th June, by a dog belonging to a woman who had come to the house where the school is kept, at the Gravel Pits, begging. This girl and another had opened the door to the beggar, and, while her companion was in the act of giving her a halfpenny, this unfortunate child stooped down to pat the dog, when he seized her by the forepart of the right arm, about three inches from the wrist, and before the arm could be extricated from his grasp an extensive lacerated wound had been made. She was sent to the medical attendant on the parish, by whom the wound was *washed, and its edges brought together by sutures and simple dressings;* and after it had been dressed in this way for several days it healed, leaving an elevated purplish cicatrix, crossing the forearm, nearly two inches long. Nothing particular was observed about the girl until Thursday, the 28th ult., when she complained of considerable headache and disinclination to exert herself; but she did not refer to her arm as being the cause of any of her feelings, nor did she make any allusion about the dog to her schoolmistress, although she had minutely detailed all the particulars of the accident to a woman who had been working in the house some days before. The schoolmistress conceiving that it was only a derangement of the stomach, and being confirmed in this opinion by having another girl indisposed in the house at the same time from headache and sore throat, determined to give them both a little medicine; and on the next day she administered some salts to each of them, from the operation of which this girl appeared considerably relieved on the Saturday. On Sunday, however, she complained to her mistress of feeling very unwell, having passed a sleepless and very restless night; and she ex-

* Philosophical Transactions.

pressed a wish that she might be permitted to remain in her room, to be separated from the other girls. She was excused from going to church, and, although she signified a dislike to her breakfast in the morning, she ate with tolerable avidity some pudding at her dinner. On Monday morning she had become evidently worse, and her mind appeared to be not quite settled. Between ten and eleven, a message was brought to my house, requesting that I would visit a girl at the school at the Gravel Pits, but as it was very indefinite, the girl who brought it not mentioning any particulars, except that this school had formerly been in Gore Lane, but was now at the Gravel Pits, and from these divisions of the parish coming under the superintendance of other medical gentlemen, I desired my assistant, should the girl return, to state distinctly to her that she was to apply to Mr. Hora; but at all events, if she would bring a written order from the overseers of the parish, one of whom resides within two doors of myself, I would attend upon her immediately. In the evening the girl came back, during my absence from home, when my message was delivered to her; but she, in reply, said that the girls had been attended by my predecessor, Mr. Wright, and that, on former occasions, no orders had been required. My assistant soon afterwards went to the school-house, and, on ascertaining her condition, he dispatched a note to me, begging me to go there, as soon as possible. Immediately on reading his note, I prepared some opium and camphor pills, and with these, accompanied by a friend, I hurried off. By mistake I had knocked at the door of the next house, but on that of the school-house being at the instant opened by the distressed and terrified mistress, we heard very plainly the screams of a young person evidently labouring under great mental agitation and alarm. These became louder as we entered the room where she was. She was sitting on a sofa, recoiling from sight and noise, and at times talking perfectly incoherently. I sat down on the sofa, at the right side of her, and my friend occupied a chair to her left. As soon as she had, to a certain degree, got over the alarm which our presence had occasioned to her, she entreated of me most earnestly that I would cut out "that part of her arm where the mouse's tail was twisted up," and that I would apply some leeches to her chest; saying, in conclusion, that she was convinced she should then be quite well. The request to have the part removed was repeatedly urged by her, and I only succeeded in pacifying her by telling her that I would do something to it in the morning, but that, in the meantime, I would bandage it. I persuaded her to take some of the pills that I had brought with me: from the convulsive paroxysms coming on during her attempt to get them to her mouth, she crumbled two of them between her fingers before she could succeed. The paroxysms returned at intervals of about two minutes; during their continuance, her whole body was violently agitated; her legs were alternately drawn up, and forcibly thrown from her, as would have been her arms also, but that she had given a hand to my friend and myself to hold. Her chest laboured excessively; her mouth was open; she gasped for breath; her pupils were widely dilated. She begged the light of a single candle to be removed, and that the girls might be sent away from the door. Her pulse was 110, moderately full and hard; the heart pulsated most rapidly and forcibly; a cold perspiration gathered on her forehead; she complained of her tongue being too large for her mouth—that there something very dry and thick upon it; it was, in fact, covered with a coating of thick frothy mucus. She begged of us not to sit too closely to her, for our breath came upon her like a solid body. She would not allow me to touch her, except she took my hand in her own, and when she endeavoured to wipe her forehead she did it with the utmost rapidity, anxious apparently to have it concluded. I once applied my hand to the back of her neck, over the spinous processes, but it induced such a dreadful convulsive paroxysm that I did not repeat it. She appeared to have an indistinct remembrance of her mistress, addressing her by the titles of Majesty, Rosa, &c.; still she was perfectly obedient to her. She asked several times for something to drink, but as soon as a cup containing some water was placed in her hand a paroxysm was the immediate consequence, and she at the same instant set it down again. After several attempts to get a teaspoonful of water to her mouth, she at length, summoning up all her resolution, and at the

same time adding that if she could but take a good drink she was sure that she would feel better, succeeded in throwing a few drops into her mouth; when by far the most severe convulsion that I saw followed, and at the same time a discharge took place from the bladder. She could not succeed better with a little thick gruel coloured with treacle, nor, indeed, with any other fluid. I moistened the end of her handkerchief in water, and got her to put this into her mouth, in order to relieve the painful sensation of thirst of which she complained, but she was still very much agitated in using it. She asked for some bread and butter, which she swallowed well. While I was with her, which was fully three hours, I got her to take three pills, containing three grains of opium and nine of camphor; and I left orders that the pills were to be repeated every hour, or hour and a half: this I did from observing that during the last hour that I remained with her the paroxysms evidently returned at longer intervals, and the girl expressed herself relieved of some of the pain at her chest;—she was also more composed, not speaking so quickly nor so incoherently. Towards five in the morning all the symptoms had become much aggravated, her movements were much more violent, and she absolutely raved. This was the report I received from the nurse who sat up with her. On my seeing the poor girl at the workhouse next morning, and to which I had, the night before, advised the mistress of the school to have her removed as early as possible, she was, indeed, much worse. I found her arms confined by a straight-waistcoat (which it had been found absolutely requisite to procure for her, before her removal could be attempted), and her body secured to the bed where she lay. She was in a state of incessant convulsion, thrusting her tongue from her mouth, spitting out the thick mucus which covered it at those who came about her; speaking (or rather screaming) loudly, and abusing all around her, or singing, in an under and almost indistinct tone, alternately; her eyes looked fiercely, and her head was constantly thrown most violently about her. In this way she struggled on until between twelve and one o'clock on the Tuesday, when she brought up a large quantity of brownish fluid, and, in doing so a second time, she expired.

From the first moment of my seeing her, and hearing the particulars of the case, I felt how utterly hopeless would prove all my endeavours to save her. I ran over in my mind all that had been recommended for this horrible malady, but I turned away from them all (from large bleedings down to the administration of the guaco), and brooded over the melancholy fact, that, as far as our experience at present went, no substance whatever, introduced into the body either by the stomach and bowels or the circulation—no application to the surface, and no surgical operation—could promise any relief: when the symptoms are fully formed, death alone can do so.

SALINE INJECTIONS IN CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

It may be superfluous to add to the number, already recorded in your useful Gazette, of the cases of malignant cholera, in which saline injections into the veins has been resorted to; but on the introduction of a new remedy in the treatment of a disease which has baffled the skill of the most eminent of the medical profession, it appears to me to be a duty incumbent on the members of that profession to report the result, whether successful or otherwise, of their experience of that remedy; and it is solely on that account that I offer the following communication.

At the urgent request of Mr. Carter, surgeon of Billericay, in this county, I reached that town between three and four o'clock of the afternoon of the 5th inst. and was informed by him that two cases of cholera had occurred in his practice in the course of that day. The first patient we visited was Mrs. Barrett, aged 62, of full habit of body, the wife of a respectable tradesman. It was stated that on the preceding evening she had eaten of currant pie and turnip radishes, and had also taken three of Morrison's pills. About seven on the following morning (5th) she was seized with diarrhœa, which was ascribed to

the pills, but as it increased as the day advanced, and was accompanied with great debility, occasional vomiting, and cramps of the legs, the advice of Mr. Carter was requested between eleven and twelve o'clock. The countenance was much altered, and other symptoms of approaching collapse were manifest. At the time of my visit the state of collapse was perfect; and notwithstanding prompt and active measures, the patient became progressively worse, and died about half-past eight.

The second case was Miss Burrows, aged 30, of spare habit of body, an inmate in the seminary kept by two very respectable maiden ladies of the name of Evans. Miss B. was reported to have dined on the preceding day off tainted lobsters, and to have eaten freely in the evening of strawberries. This morning, about nine o'clock, while at the house of a neighbour, whom she was about to accompany on a journey, she was seized with slight vomiting, which induced her to return to her home; but as this symptom subsided, and she felt better, she went to her friend, intending to pursue her journey; but about eleven o'clock, feeling weak and faint, she again returned to her dwelling, and was seized with vomiting, purging, and cramps. At the time of my visit, half-past four, symptoms of collapse had taken place. The symptoms already enumerated were incessant, accompanied by excessive thirst and great pain in the back; hands purple, cold, and shrivelled; pulse not to be felt. This patient also fell a victim to the disease about eleven at night.

In the course of the evening, during our attendance on this young lady, the younger Miss E. aged 57, of poor constitution from distorted spine and organic diseases of the pelvic viscera, under which for some years she had laboured, complained of weakness and faintness. About eight o'clock she stated that her bowels had been three times relaxed. She was put into a warm bed; stimulants, with ammonia and astringents, c. tint. opii were administered, and directed to be given after every loose evacuation, and she was at ten o'clock left to the care of Mr. Carter for the night. Before, however, taking my leave, the other sister, aged fifty-eight, of more sanguine temperament, and who had been absent from home the early part of the day, reported that her bowels had been disordered during the day, and that she had vomited once or twice in the early part of the morning. The same plan was recommended to her as had been advised for the sister.

On the following morning, the 6th, I arrived at B. about half-past eight; and on my way to the house of the Misses E.'s, was met by Messrs. Carter and Butler, of Ingalestone, from whom I learned that the symptoms in both the sisters had progressively increased during the night; that neither medicine or diet was retained; in short, that both of them were in a state of perfect collapse. This account I found too true from personal observation. The youngest sister was in a far more advanced stage; eyes sunk, and surrounded by dark areola; features shrunk; general coldness of the surface; hands blue, shrivelled; pulse not to be felt; lies in a state of unconsciousness; in short, death seemed to be approaching rapidly. Having too frequently experienced the inefficacy of various modes of treatment adopted by me in some cases of collapse, I determined to try the effects of saline injection into the veins, and for this purpose had taken over with me the apparatus. The apparently dying state of the patient at first made me hesitate as to the operation; however, having finally resolved on the trial, the following mixture was thrown in at ten o'clock.

℞ Mur. Sodæ, ʒij. Carbon. Sodæ, ℈ij. Aq. Calidæ, ℥lx. (Temp. 110° to 115° Fahr.)

Before the operation was concluded, the patient revived surprisingly; the countenance became more plump; the pulse more distinct; and she expressed a sensation of warmth about the chest. In a very short time after the fluid was thrown in, the voice, which had been that of a whisper, became stronger; the lividity of the face decreased; respiration more natural, and the chest could be fully expanded; a general warmth was diffused over the trunk and extremities, and she expressed herself as feeling much better. There had been no vomiting for some hours; the tongue was whitish, but no thirst Brandy and arrow-root, with a cordial mixture and pills of camphor, capsicum, and a quarter of a grain of opium, were administered; frictions, and sinapisms to the epigastrium and spine, were applied.

Eleven, A.M.—Features more contracted; pulse more feeble; tips of the nose and fingers cold, but general warmth of the body continues.

The saline injection was repeated to ℔ij.

The effects of this injection were almost magical. Pulse 120, increased in volume and strength, but with occasional intermissions; voice so much improved in strength that her inquiries of her sister in the adjoining room are audible to every one; has had one profuse watery dejection; expresses a wish for chicken-broth, and urges some refreshment on her medical attendants, to whom she expresses her obligations, and comments warmly on the importance of the new mode of treatment, to which she now ascribes her being alive.

Diet and medicines continued.

One P.M.—Pulse more feeble; respiration laborious, and somewhat stertorous; eyes closed, and apparently asleep; dejections continue; hands still livid and shrivelled, but general warmth of body and extremities continue; retains her medicine and diet.

Repet. Inj. Mist. Salin. ad ℔ij.

Three P.M.—Pulse of good strength; respiration improved, more free and natural; skin of an agreeable moisture; heat generally diffused over the body and extremities, even to the tips of the fingers and toes; has had no vomiting or purging for nearly two hours; feels inclined to sleep.

Continue her diet and medicines according to circumstances.

Half-past six P.M.—Pulse more languid; skin colder, with clammy perspirations.

Repet. Inject. Salin. ad ℔ij. To have strong beef-tea, with Madeira wine, to which she gives the preference.

Other engagements at Chelmsford requiring my presence, the patient was left in charge of Mr. Toush, surgeon, of Homerton, who had arrived from town in the evening, and Mr. William Butler, jun. of Ingatestone.

7th.—Half-past eight my visit was repeated, and I learned that the patient had been somewhat restless, but, on the whole, had passed a tolerable night; a considerable quantity of beef-tea, with Madeira wine, had been taken. At five this morning, the pulse beginning to fail, ℔ij. more of the saline injection were repeated, after which she rallied; and at the time of my visit the pulse was 120, soft, and of good strength; skin comfortably moist and warm; lividity of the hands entirely disappeared. Tongue brownish and dry, with some thirst; only two dejections offensive during the night; diet and medicines retained; some drowsiness.

To have effervescing saline draughts; Hydr. c. Creta according to circumstances, and a stimulating sinapism to the nape of the neck.

Diet and wine left to the discretion of Mr. Toush, who was in continued attendance, being a near relative of the patient. At twelve I took leave, on my return home, and have been favoured by Mr. T. with the following notes.

Three P.M.—Transfusion was repeated to a pint, but without the good effect before experienced; chilliness increased; pulse less, and weaker; low delirium; took nourishment very frequently. Carb. Ammon. c. Tinct. Cardam. Comp. et Mist. Camphor. c. Aq. Menth. Pip. were administered.

Half-past four P.M.—Repeated the transfusion to a quart; no benefit; pulse rallied a little; warmth none; coldness continued to increase, also the weakness and delirium, till three o'clock A.M. of the 9th, when she expired, having become tympanitic some hours previous.

The second case in which the saline injection was used, was the elder Miss Evans, aged 58, of a more sanguine temperament than her sister. Countenance not so much collapsed as her sister's; hands livid, shrivelled, and cold; some warmth of the trunk; less prostration of strength, being able to change her position in bed; no vomiting or purging since twelve o'clock last night. Pulse 100, feeble; voice not much altered; slight cramps of the legs.

July 5th, ten A.M.—℔ij. of saline mixture, of the same nature as that used for her sister, were transfused. During the operation a general sensation of warmth was experienced; pulse increased in volume; expresses herself as being better, and feeling stronger.

Pills of Camphor, Capsicum without Opium, were given and ordered to be repeated according to circumstances. Arrow Root with Brandy, as diet.

12th.—No vomiting or purging; retains her diet; skin warm; pulse not so full nor strong as after the injection; no thirst; lies composed, and sleeps occasionally. Tongue furred, but moist.

One P.M. — Pulse more languid; countenance more collapsed; skin not so warm and relaxed. The transfusion was repeated to ℔ij. but was not followed by improvement in the pulse or heat of skin. Respiration strong.

Seven P.M.—Heat of skin has returned; moisture abated; pulse improved; eyes suffused; drowsiness increased, with heat of head and other symptoms of cerebral congestion.

Injection not to be repeated. Saline efferv. draughts; cold lotions to the head; arrow-root, or beef-tea, with Madeira wine, according to circumstances.

During the night the strength began to decline, the pulse to flag, with diminution of heat generally. The saline mixture was transfused without any rallying of the system, and she died about half-past ten of the morning of the 8th. Both sisters are said to have dined off tainted lobsters on the 4th instant.

I shall be very brief in my remarks on these cases. Notwithstanding their fatal termination, I shall not be discouraged. Neither of the ladies was of strong constitution, and both had passed the meridian of life. That their lives were prolonged by the saline injection, must be obvious to every one: its effect on the younger sister surprised all the bystanders, of whom no less than six were of the medical profession, all of whom witnessed the rescue from impending dissolution.

My experience in the treatment of this fatal disease has been limited to eight cases; in not one of which, after the stage of collapse has occurred, have I seen the least improvement in the pulse, breathing, or animal heat, from the use of blood-letting, calomel and opium, hot-air baths, frictions, or stimulants of the strongest kind, given by the mouth and rectum, or applied externally in the form of liniments or sinapisms; whereas, from transfusion, the most immediate and decided benefit ensued.

With many apologies for having trespassed so much on your time,

I am, sir, yours,

SAM. MILLER, M.D.

Chelmsford, July 10, 1832.

P.S.—It is due to Messrs. Carter and W. Butler, jun. to say that the transfusion in every instance was very satisfactorily performed by them, and with very trifling loss of either blood or saline mixture.

THE TREATMENT OF CHOLERA

By the Injection of Saline Solutions into the Veins not deducible from the Pathological Condition of the Blood.

BY JAMES M'CABE, M.D. CHELTENHAM.

(Transmitted to Central Board of Health.)

OF the several modes of treating cholera morbus which have from time to time occupied the attention of the medical profession and of the public, the treatment by the injection of saline solutions into the veins appears to have attracted most the attention of the profession. Whether a remedy be found out by fortunate chance or scientific deduction, the medical faculty are equally bound to avail themselves of the discovery; but a remedy laying claim to scientific foundation is sure to meet with greater favour, and is more likely to be adopted without sufficient inquiry, than that which originates in mere experiment. Having premised these general observations, let us now inquire how far the treatment by saline injections into the veins is warranted by, or deduducible from, the pathological condition of the blood in cholera.

Chemical analysis has shewn that, in the blood of patients labouring under cholera, there is a deficiency of its watery and saline parts; and Dr. Latta states, that an article published in the Lancet by Dr. O'Shaughnessy, in which this pathological fact is stated, first suggested to him the idea of treating the disease by restoring to the blood its serum and saline particles. This he first attempted to do by saline solutions given by the mouth, and also introduced by the rectum; but finding this plan unsuccessful, he had recourse to the more direct means of injecting it into the veins.

We find in all the analyses that have been published on the subject, that the deficiency of the watery and saline parts of the blood, in cholera, is referred to the profuse discharges which had previously taken place from the stomach and bowels. Now these dis-

charges from the blood must be occasioned by some morbid condition of the system; and we can scarcely suppose that that morbid condition can be remedied by merely replacing by injection into the veins what that morbid condition had allowed, or probably had caused, to be effused. If we adopt, in our explanation, the humoral pathology, and suppose the cause of these discharges to be the state or condition of the blood itself, we must also adopt the language of the older physiologists, and refer it to an acrimony of the blood and humours. What this acrimony can be, unless it be an excess of saline particles in the blood, it is difficult to imagine; and if so, and that the discharges are occasioned by the vellication of the coats of the arterial exhalents by the saline spiculæ of the blood, it is evident that saline injections would be pathologically contra-indicated. If, on the other hand, we refer the immediate cause of these discharges to the pathological condition of the solids—that is, to the state of the containing vessels—we meet with the same difficulty; for we cannot expect to remedy the relaxed state of the solids by merely refilling the vessels with a fluid analogous to that which they had already effused.

It is, therefore, evident that in founding the treatment by saline injections on the pathological condition of the blood in cholera, an effect has been taken for a cause, and made the basis of a practical deduction. The loss of the watery and saline parts is the effect of some antecedent cause, over which the return of them again into the system by injection can exert no remedial power. Could it be shewn that an excess of salts in the blood was the cause of the profuse discharges, the injection of pure water, of the temperature of the blood, into the veins, would probably be beneficial by diluting the thickened crassamentum; but if the effusion from the exhalent vessels be the effect of diminished vitality of the solids, no benefit would be likely to result from any injection by the veins, as it would pass through the same channels.

There are many circumstances in the history of cholera that would favour the opinion, that an excess, and not a deficiency, of the watery and saline parts of the blood, is a predisposing cause of the disease. In the first place, it attacks most frequently the poor and ill-fed portion of the population, more especially those whose constitutions are broken down by disease or intemperance;—individuals labouring under organic obstructions, and in whom dropsical effusions might be expected to occur, from which, in fact, the discharges in cholera differ only with respect to the situation in which the effusion takes place; for were the fluid which in cholera is poured into the alimentary tube, to be effused into the cavity of the abdomen, or cavities of the pleuræ, it would constitute ascites or hydrothorax. We know that the frequent use of aperient medicines, whether drastic or saline, has the effect of draining off through the mucous membrane of the bowels, a portion of the watery and saline parts of the blood. We know that among the better classes of society in this country, considerable attention is paid to the state of the bowels; in fact, that aperient medicines are frequently taken, and consequently a considerable part of the watery and saline constituents of the blood removed in that way; while among the poorer classes, less attention is paid in this particular. We know that the French, generally, as a nation, are very much averse to the use of aperient medicines, and in fact seldom or never take any. In France, the cholera has attacked the rich and the poor indiscriminately;—in this country, its ravages hitherto have been very much confined to the poor. In France, the rich and the poor systematically and intentionally avoid taking aperient medicines;—in this country, the poor generally neglect to take them. In the progress of cholera hitherto through both countries, are we merely to consider it as a singular coincidence, or are we to view it in the relation of cause and effect, that the disease has principally attacked those persons who habitually or systematically neglect the use of aperient medicines?

From the latest reports published on the subject of the saline injections, it would appear that excruciating pain frequently follows the use of the injection, particularly when the fluid is again making its way into the stomach and intestinal tube.

From the various facts and circumstances here stated, and the inferences which appear to me to be fairly deducible from them, I am of opinion, that there is no deficiency, but probably an

excess, of the watery and saline parts of the blood in cholera, previous to the discharges which accompany the disease; and, consequently, that the treatment by saline injections into the veins, if found successful, ought to be referred to fortunate experiment, and not to the pathological condition of the blood.

PROPOSAL

TO

ADMINISTER SALINES IN CHOLERA,

By the Natural Processes of Absorption and Assimilation, instead of injecting the Veins.

To the Editor of the London Medical Gazette.

Newcastle, July 20, 1832.

Sir,

If I saw any good reason for believing that either new light had been thrown upon the nature of cholera, or any material improvement had taken place in the method of treating that formidable disease since its first visit to this place, I should not have troubled you with this communication; and it is only from a conviction that many hazardous experiments are constantly made use of in the treatment of a disease which, under the most judicious and philosophical management, is too often necessarily fatal, that I am induced again to endeavour to draw the attention of my professional brethren to the necessity of accurate reasoning, and of forming distinct and definite indications of cure, while prescribing for cholera patients. But it will probably be maintained by the advocates of the favourite practice of venous injection, that it is founded upon accurate chemical analysis and strictly philosophical argument—that the immediate effect of the disease is to deprive the blood of its watery and saline constituents, and that the most obvious remedy must therefore consist in replacing them with similar materials. But granting, for a moment, that it were possible thus directly to restore the deficient principles of the circulating medium (though perhaps a more direct method still would be to inject into the veins the matter discharged from the intestines), is it philosophical to suppose that we can thus remove the diseased action by which its qualities have been so remarkably altered? If even we could succeed in bringing the circulating mass to the condition in which it was at the commencement of the disease, it by no means follows that the diseased action which *has* already *will not* again deprive it of its defective parts. But how different is the process we pursue from the more elaborate one by which the blood has been originally formed! Compare it with the mysterious processes of chymification, chylification, assimilation, and gradual admixture; and how crude and imperfect must it appear! The whole theory, too, of injecting the veins, must depend, for its consummation, upon the matter introduced becoming intimately mixed with the black, tarry, deteriorated blood which remains in the vascular system; but how is this to be accomplished? How often must it run the round of the circulation, vigorously impelled, before such a thorough combination of two heterogeneous fluids can be brought about? Forgetting, for an instant, the desperate nature of the disease for which this desperate remedy has been suggested, and considering it in the abstract, what effect should we expect to result from it? I would answer, precisely the effect which it has been found actually to produce—a temporary stimulation of the action of the heart and arteries, to be soon followed by painful oppression, and, before long, its complete extinction. I repeat, that, reflecting upon the probable result, such would have been the anticipated, and such have been the actual consequences, of injecting large quantities of foreign unassimilated matter into the veins. Of course I speak of the general rule, for the few exceptions that have taken place are but to be considered as anomalies—remarkable ones it must be confessed, and well calculated to excite attention and give rise to interesting inquiry; but they are too few to warrant a general pursuit of the practice, and the recoveries which have taken place ought more properly to be considered as having occurred *in spite*, than in consequence, of the treatment.

For the truth of the preceding remarks, I need only refer your readers to the various cases recorded in the late numbers of your own journal—temporary amendments followed by a more speedy death than would otherwise pro-

bably have taken place, is the history of nearly all; and I must think the exceptions are fewer than would have taken place under a different plan of treatment. But though it appears to me that a wrong application has been made of the knowledge which we have attained (through the labours of Dr. O'Shaughnessy in particular) of the chemical alterations sustained by the blood in the course of an attack of cholera, I am very far from undervaluing such information: on the contrary, I consider it as affording a very useful indication in the treatment of the disease: and if the crude notion of supplying the deficient materials immediately to the circulating mass appear to me unphilosophical, far otherwise is the intention of introducing it through the medium of the absorbents and assimilative process. Such an indication may, with strict propriety, form a part of a rational and philosophical plan of cure. But it may be asked, how can it be accomplished? I answer, by introducing into the stomach and intestines the same matter which has been so profusely thrown into the veins. Four pounds of warm water, with saline and alkaline substances in solution, may be injected at one time into the intestines, and large quantities of gruel, salted to the taste of the patient, may be drank with little or no hazard of being rejected, especially if the gastric and intestinal irritation be previously allayed by a dose of calomel and opium. This fact I have satisfactorily put to the test three several times during the present week. The first patient (a man about sixty years of age), in whom the vomiting and purging of enormous quantities of rice-water had gone on for seven hours, whose extremities were blue and wrinkled, and whose pulse was totally imperceptible, rallied to such an extent as, but for his advanced age (which I consider a perfect barrier to recovery in such severe cases), would have warranted good hopes of success; and in the other two (both females), whose cases were less severe, though sufficiently marked, convalescence has been the result. I would suggest, therefore, in addition to the principles of treatment which it was my endeavour to establish in my lately published Essay on Cholera, that the deficient ingredients of the blood—those which have been removed by the profuse discharges which characterize the disease—should be supplied to the circulation, not by direct injection into the veins, but through the natural processes of absorption and assimilation. As I conceive it is the direct tendency of the treatment recommended in the work referred to, to restore organic function, I cannot consider it a valid argument against this practice that no such processes as absorption and assimilation can go on during the continuance of the deranged action which constitutes the disease. It is true that our attention must be directed to the twofold object of checking this action and of restoring the havoc it has made on the stamina of life: the first may be done with considerable certainty; but in the second appear difficulties which I fear neither the venous injections nor the substitute I suggest, will be able always to overcome.—I am, sir,

Your obedient servant,

T. M. Greenhow.

CHOLERA AT PLYMOUTH.

To the Editor of the London Medical Gazette.

Plymouth, July 19.

Sir,

I published in your journal, July 13, the report of a case treated by Dr. Budd. I, having been summoned to the case previously, found Dr. B. in charge of it. He requested me to see it, and mentioned the plan of treatment which he was adopting. I accordingly sent you an account of it, without his consent, not deeming it necessary. He is much annoyed about the matter; in consequence of which I beg to state, that I am sorry I have wounded his feelings, and that I had no intention whatever of questioning his treatment, which he imagines that I did in the case alluded to. I am not aware that I have acted in an uncourteous or unprofessional manner towards him.

I have made the above explanation at the request of Dr. Budd, who, having recently begun his professional career in this place, has zealously devoted his attention to several very malignant cases of cholera; but I must express my conviction, that nothing in my former communication will be interpreted by any

unprejudiced mind to convey a censure on that gentleman. He must be aware that I cannot be actuated by professional jealousy toward him; and no well-bred member of a liberal profession should nurture a morbid sensitiveness of honour, which is generally the offspring or the parent of party-spirit.

The official returns to the Government by the Board of Health for Plymouth, will exhibit the progress of cholera since the date of my former paper. Those returns are indeed lamentably incorrect, from many cases having been attended by persons who are not authorised to act as medical practitioners, and who have not made any returns to the Board; whence the epidemic appears to have been less extensive than is the fact; and its *relative mortality is overstated,* from the *more malignant* cases only having been reported by some of the medical men.

Many cases have occurred within the last ten days which strongly illustrate the value of the treatment I have already mentioned. Blood-letting has been employed after extreme collapse, and its immediate effect has been most salutary in some cases, although in others it has only delayed the fatal issue. Notwithstanding the ill success of calomel in the hands of Mr. Searle, at Warsaw, I am confident that the *small doses* employed by myself have been really efficacious in restoring the secretion of the bile, and thereby promoting the decarbonization of the blood.

It is due to my venerated friend, Mr. Dunning, to state, that he has long held and expressed the doctrine which Dr. Stevens has elucidated, of the essential cause of cholera being a *vitiation of the blood*, from a specific miasm which permeates the mucous membrane of the lungs; that vitiation may also be referred to an impediment of those functions which maintain a healthy state of the blood—from a depression of the vital powers of the *solids*; and it is in this latter mode, I conceive, that the variable quantities of *electricity* in the atmosphere operate.

I shall only add, that many facts have come under my observation which establish the contagiousness of the disorder.

I am, sir,

Your obedient servant,

EDWARD BLACKMORE.

SALINE TREATMENT OF CHOLERA.

To the Secretary of the Central Board of Health.

Rochester, July 21, 1832.

SIR,

BEING strongly impressed with the hope that the chemical pathology of the blood, as shewn in Dr. O'Shaughnessy's able analysis, would lead to a more rational and certain mode of treatment in this disease than any hitherto laid down; and, at that time being strongly excited by the simple mode of treatment recommended by Dr. Stevens (on this principle), and lauded by him with such praise and success in his practice in the Cold-Bath-Fields Prison, I was determined to give this remedy a fair and unbiassed trial on the first opportunity that should occur.

An opportunity soon presented itself, by my having charge of an hospital ship attached to the convict establishment at this port, where the cholera had assumed a very malignant character.

Three cases from among the admissions during one day, were selected for trial.

CASE I.—The medicine was given every hour, as recommended by Dr. Stevens, together with dry heat, frictions, mustard poultices, and injections of hot salt and water; but notwithstanding the most unremitting attention of myself and assistants to the faithful exhibition of this remedy, the patient died in about twenty hours after admission, or after he began to use the medicine.

CASE II.—Used the same remedy as Case I. immediately upon admission; died forty hours after he commenced to take the medicine. Neither of these cases shewed the least symptom towards re-action during the whole period under treatment.

CASE III.—Was likewise put under the same mode of treatment as the two preceding cases, but observing them sink so rapidly under the saline treatment, and believing, from much experience in this disease, both in India and in England, that I had superior remedial powers at command, the saline treatment was accordingly laid aside. A mustard emetic was immediately given; bleeding by leeches from the region of the heart and præcordia; ten grain doses of calomel, five of capsicum,

given every hour; the mustard sinapisms, dry heat, and diffusible stimuli, given as occasion pointed out, with a table-spoonful of cold water, as the craving for drink was excessive. The salt water enema was continued, with an addition of ℥j. of the ol. terebinthinæ to each; by which treatment re-action was soon established, and he is now at duty.

From my own practice and experience, and in that of some of my friends, of the saline treatment, I have no hesitation in stating my opinion, that it is a remedy *per se*, totally inert in the collapsed stage of cholera.

I can, however, state, that the hot saline enemata, when combined with the ol. terebinthinæ, and the mustard poultices, as recommended by Dr. Stevens, are most valuable remedies in the stage of collapse from cholera.

I have the honour to be, sir,
Yours obediently, &c.
JOHN ANDERSON, M.D.
Surgeon, R.N.

Extract of another letter from Dr. Anderson, same date:—

During the last week I have had 12 boys in a complete state of collapse; they have all been treated under the plan pointed out, and I am happy to say, that re-action has been established in all of them, with only one exception; and even this case, I trust, will still do well. It is rather singular, but true, that I have as yet lost no case where reaction has been once fairly established.

(Signed) JOHN ANDERSON.

CHOLERA IN COLD-BATH FIELDS PRISON.

[LETTER FROM THE GOVERNOR.]

To the Editor of the London Medical Gazette.

SIR,

IT is not my practice to indulge in public statements on subjects connected with my duty as Governor of this Prison; but a controversy having arisen respecting the *treatment* of the cholera here, which in its progress amongst medical men has raised a further question as to the late extent of its existence in the prison, and the degree of credibility due to our returns, I find myself compelled, either by my silence to acknowledge a participation in fraud, (for my name has been necessarily mixed up with the subject) or by a simple statement of facts, to vindicate all concerned from such unfounded imputations. And here permit me to observe that I wish carefully to abstain from any expression which can by possibility convey offence to Sir David Barry, and the members of the Central Board of Health, whose conduct to me personally was extremely courteous; but I would just remark, that I think it somewhat unreasonable that a hasty inspection on their part of about half an hour on two* successive days, when the disease was manifestly on the decline, should appear to justify so serious an accusation against the surgeon, and indirectly against others, of fabricating accounts of cholera which never existed, for the purpose of magnifying the skill of one professional man. If the surgeon had an interest in such deception, (which, by the way, I sincerely believe him to be the last man to countenance) I beg to say that I and others† could have nothing in fact, or in prospect, to encounter but labour and anxiety. We could expect no remuneration—anticipate no credit from such a visitation, however magnified. The discipline and order of the prison, my chief care and natural pride, must be greatly relaxed by the measures of precaution to be taken, while trouble was multiplied in every possible way, without any attendant satisfaction, unless it were to be found in the very questionable one of being thought to exist beset with infection, from which every friend would anxiously fly. But let me revert to facts.

In the official return to the Secretary of State for the preceding year, the total number of deaths in this prison during twelve months was *sixteen;* and within one little month from the 3d of June, on this last melancholy occasion, the cholera within these walls proved fatal to the *same number*. In ordinary times, at this season, the infirmaries were vacant, and the prisoners healthy; while on this occasion, in every part of the prison, and at all hours of the day and night, to the destruction of the rest of my officers, who were harassed to death, were we called

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* It is by no means wished to conceal that Sir David Barry did visit the Prison on the 27th, as well as on the 28th June. The misconception on that head arose from an error of the reporter, who attributed to me the speech at the meeting of the Middlesex magistrates, delivered by Mr. Hoare, the chairman of the Visiting Committee. Had he been aware of the order of sessional proceedings, he would have known that, unless specially called upon, none but magistrates speak on the County day, and that the chairman of the committee is the organ of communication with the court.

† I abstain from saying any thing respecting the motives of the magistrates, because as I write this with a view to the justice of the case, without having consulted them, I do not consider myself authorized to mix them up in the discussion, though my reasoning obviously applies equally to them.

to some unhappy being seized with diarrhœa and vomiting, but most frequently the latter, to the number at one period exceeding 100. Many of these could not be removed so speedily ere they exhibited the extreme symptoms of this awful malady; and who could tell how soon the slightest case, if for the shortest period neglected, might not terminate in death? Here was no deception or collusion; for my chief officer, a man of principle and veracity, can vouch that cell after cell was nightly drenched with the fluid rejected from the stomachs of their inmates, many of whom were likewise violently affected by cramp. Of course, the persons so attacked were instantly removed and placed under observation; and not a whit too soon, as the after agonies of numbers of them testified.

Now although it is affirmed that the majority of these cases were not of cholera, because the parties were not "in a state of collapse without pulse," what else (when deaths daily occurred with the character of the disorder most unequivocally marked, and while without the walls also the disease extensively prevailed)—what else, I say, in common candour, could have produced this singular deviation from the health of former years; the labour was the same, the food the same, the building, clothes, bedding, and ventilation, all the same, and yet 100 poor wretches were simultaneously seized with purging, vomiting, and cramp, very many of them thrown into a state of almost hopeless prostration, and were only recovered by incessant care and sleepless watchfulness. In one short month sixteen died, and yet we are accused of spreading a false alarm, and that too in order to enhance the discovery of a medical man almost a stranger to us; and here, in justice to Dr. Stevens, let me add, that numerous recoveries under the saline treatment were, on the former irruption of cholera in this prison, as also on this last occasion, most striking. I could give name after name, on oath if it were necessary, and have my testimony amply confirmed by disinterested witnesses, where death was momentarily expected, and recovery deemed impossible; and yet they survived. But why, it may be asked, ascribe their revival to this mode of treatment? Because, I answer, too well do I remember in April last the rapid fatality of the first cases treated with opium and stimulants, and the horror with which we beheld so frightful a disorder spreading amongst us, apparently without remedy; for all efforts to save appeared unavailing, until Dr. Stevens's treatment was adopted, and then we saw cause to hope. Although in some instances it failed, at that time *seventeen* out of *twenty-four* extreme cases recovered. Then, as on the last occasion, we had the same hourly alarms from persons suddenly attacked, as I have above described, who were, to the number exceeding eighty at one time, removed from their cells and placed under observation. I am not a professional man, and cannot, of course, venture upon medical reasoning; but to my plain comprehension it appears that such numbers, contemporarily seized, must have laboured under incipient cholera, since we had cause to remark that, where the sufferer too long concealed those first symptoms, his recovery from the most malignant form of the disease became almost impossible. Dr. M'Cann, from the Central Board of Health, at that time paid frequent visits to the prison, saw the patients, fully acknowledged the existence of malignant cholera, and laudably made every inquiry to trace its origin and progress in the prison. At that period, we returned none but the extreme cases to the Central Board; and, from my perfect recollection of a casual conversation with Dr. M'Cann, I apprehend we were then thought rather to underrate than to exaggerate the state of the case.

When Sir David Barry visited the prison on the 27th June, he found, he says, no case of cholera, but on the 28th he acknowledged to have seen two cases. Now the disease had existed since the 3d June; and if the short space of twenty-four hours could produce this change even in the decline of the disorder (when the wards were crowded chiefly with persons very slightly affected, or who were retained from the fear of premature dismissal), what number, by a parity of reasoning, may not even that ratio have exhibited since the 3d? But on the 27th, when it was said no cholera existed, a man named Harris was in the Infirmary, who two days before was in the worst imaginable state, and momentarily expected to expire. This was the report to me of my Infirmary Turnkey, who had himself seen upwards of twenty deaths from cholera, and was therefore well acquainted with the disease. The surgeon, on the 27th and 28th, would certainly designate that case one of cholera, and who can doubt the propriety of doing so, since the least neglect would assuredly have produced relapse? Although the remedies had worked a favourable change, the man was decidedly in a critical stage of the disease. Others there were under similar circumstances; but I mention the case of Harris, because it was an extreme one. On the 29th, a prisoner named Allen was in a state of collapse; I remained in the Infirmary upwards of three hours with the surgeons, who devoted so much time to that man, so urgent was the case considered to be. Transfusion was resolved upon, and repeatedly tried in vain; the tube could not be introduced into the vein, and the patient was left to the ordinary saline remedies: his recovery was despaired of, but the next morning the pulse had returned, many of

the urgent symptoms had abated, and that man also was precisely in the state of Harris, and might, with equal justice, on the succeeding day have been denied to be a case of cholera.

Surely then, sir, if inquiry were demanded, and truth the object to be elicited, instead of the hasty deductions drawn from half an hour's inspection (whereby Dr. Stevens, Mr. Wakefield, and all concerned with this place, have been exposed to sinister observations), a more matured investigation, and a calmer discussion of the number of cases, and merits of the treatment, ought, in common justice, to have been adopted. The evidence of the medical men should have been treated with more respect, and the testimony of disinterested witnesses (whose sad experience has, alas! forced upon them some judgment in this disease, and who could at least have deposed to facts)—these, I say, should have been consulted before the reputation of professional men was assailed, and the statements of others impugned. Permit me to say, sir—more in sorrow than in anger—that we who really know what has occurred, and consider *facts* alone, can hardly conceive it possible that a grave and momentous question should be thus decided. The attendants and nurses *know* that upwards of twenty patients have, on this last occasion, recovered from the state of collapse under the saline treatment; many of these cases I saw prior to my leaving town for a few days, and since my return; and yet, with the full knowledge of such facts, that professional rivalry should operate against the real truth of the case, is indeed deplorable.

I do not question the competency of Sir David Barry and other gentlemen in their professional duty, nor do I wish to speak disrespectfully of them, but I say that no man or set of men could on such loose data form a correct judgment. With respect to the saline treatment, such is my confidence in it, *simply from what I myself have seen of its effects*, that if I should unhappily be attacked with cholera, (and who for a moment is secure against its ravages?) I shall assuredly desire to be put under that treatment; for although in this dreadful malady no prescription seems to be unerring, still I have seen enough to know, that it has produced surprising recoveries.

In conclusion I beg to say, that I am animated solely by a wish to support the truth; that Dr. Stevens is ignorant of the purport of this communication; and that a sense of public duty alone has impelled me thus to occupy your pages.—I am, sir,

Your very obedient servant,

G. L. Chesterton.

House of Correction,
Cold-Bath-Fields, July 25, 1832.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

The Dublin Journal of Medical and Chemical Science. No. III.

The first paper in this number contains several interesting points, on one or two of which we must offer a few observations.

Dr. Graves on Double and Single Vision.

An object, it is well known, will, under certain circumstances, appear double when viewed by both eyes in their ordinary healthy condition: for example, a hole that admits light through a window-shutter will present two images, if the eyes be immediately directed, not to the hole, but to some object that intervenes. On the other hand, this intervening object will appear double, and the hole single, if the axes of the eyes produced form an angle at the latter. Again, when two images are thus formed, they will be found to cross each other, or be seen by opposite eyes, if the object which is seen single be more remote than the other object; but they will not cross if the latter be the more remote.

This fact of the regular crossing or not crossing of the images of any object not immediately looked at, has been pointed out, so far as we are aware, for the first time by Dr. Graves; but we do not think that he is very clear in his explanation of it, or that the diagram which he employs is well adapted for his purpose. He supposes two cases: in the first, the object to which the eyes are immediately directed is placed beyond another object which is in the same field of view, and Dr. Graves properly concludes that the images of the latter cross each other, because their lines of visible direction intersect. In the second case, the secondary object (if we may term it so) is the more distant, and its images do not cross each other; *because*, says Dr. Graves, *their visible directions do not intersect*. And certainly, in the diagram, the supposed lines of direction (if such they be) are very conveniently disinclined to each other.

But we think the explanation defec-

tive, if not absolutely incorrect; for there is nothing more certain than that the lines of visible direction do *in both cases* intersect, and always will, as long as the axes of vision make any definite angle. It would be better, in our judgment, to assign for the circumstance some such reason as this. The eye, as it appears to us, in viewing any number of objects at once, *projects* their images into one and the same plane, or sees them as if the pictures of those objects were represented to it on a determinate surface: and the distance of this plane or surface is determined by the intersection of the axes of vision, or the place of the primary object;—the other images will cross each other or not, according as the object producing them is nearer or more remote than that intersection. Let three objects, for example, be placed before the eyes at different distances, and if none of the images overlap (which may be readily avoided, —the *two* images of the primary object, of course, *must* coincide,) there will be five images of those three objects all reduced to one and the same plane: if the first or furthest object be the one in which the axes meet, the images of both the other objects will cross each other, to attain their projections in the plane of the first; if the middle object be the primary, the images of the nearest will be projected by crossing as before to the plane of the primary, while those of the farthest will be, as it were, intercepted; and finally, if the nearest be made the primary object of vision, none of the images of the others will cross,—*not* because their visible directions do not intersect, but because they are *projected* before their actual intersection.

Dr. Graves's strictures on the common opinion of the influence of habit in producing single vision with two eyes, are rational enough, and well founded: we have no doubt that, so far as *habit* is concerned, it only enables us to see the better with one eye at a time, dispensing with the other,—as when we look through an opera-glass, with the unarmed eye wide open. "Habit does not operate in the way that Mr. Herschel supposes, for single vision is gradually acquired, not by new points of both retinas becoming associated together, or rendered *corresponding* (as it is termed), but by so complete a cessation of all association and correspondence between both retinas, that the mind receives impressions from the one, but none from the other."

The consequences to which the principles laid down in Dr. Graves's paper lead, have so much the air of novelty about them, that we give them for the reader's satisfaction in the author's own words:—

"*No object but the one is ever seen distinctly by the eyes at one time.* Thus, when we look at a printed page, we only see one letter distinctly; the letters on each side of it are seen, but much less distinctly, and those at a considerable distance excite no accurate perception of their shapes whatsoever. The amazing rapidity with which the motions of the eyes are performed, it is true, prevents us from feeling any practical inconvenience from this indistinctness of objects seen obliquely; and the attention we always for the moment bestow upon the object directly before our eyes, prevents us from perceiving the comparative indistinctness of all other objects within the field of view. It is obvious, also, that objects near the intersection of the axes are not seen double, for their images, although not coincident, still very nearly overlap each other. To be seen truly double, therefore, an object must be removed to a certain distance from this intersection, and in proportion to this distance, it is less and less attended to; and consequently, the circumstance of its appearing double is entirely overlooked, unless our attention be forcibly drawn to the fact."

Mr. Kane's remarks on some properties of the Hydracids.

There is much both of originality and perspicuity of detail in this and the other paper, *On the Iodide of Platinum, and its Salts,* with which Mr. Kane has enriched the present number: we shall avail ourselves of his results at another opportunity.

Dr. Maunsell on the management of the Placenta.

The object of this paper is to enforce the advantages that arise from never suffering the uterus to relax after the child is expelled: the prompt and continued employment of pressure is strongly recommended.

On the Treatment of various Diseases, by Dr. Graves.

We must content ourselves with one

or two extracts from the interesting histories grouped together under this title; our limits, however, oblige us to be as brief as possible.

Under the head of *Habitual Constipation,* Dr. Graves recommends the following electuary, the object of which is the removal of costiveness, without the inconveniences which attend the use of ordinary remedies:—

"℞ Electuarii Sennæ ℥ij.
Pulv. Supertart. Potassæ ℥ss.
Carbonatis Ferri ʒij.
Syrupi Zingiberis q. s.
Ft. Electuarium.

"For the first two days I generally add about two drachms of sulphur to this electuary; but as soon as its operation has been established, the quantity of sulphur may be diminished one-half, and at the end of a week it may be omitted altogether. The dose must be regulated by its effects, but in general a small tea-spoonful in the middle of the day, and at bed-time, will be sufficient.

"The value of the carbonate of iron as a tonic aperient has not been duly appreciated; I have succeeded in curing, with it alone, a practitioner of eminence in this city, who had been long subject to extreme constipation, and had been reduced to the necessity of taking an enormous dose of purgatives almost every week. * * *

"When the tendency to constipation is habitual, and the patient is not effectually relieved by the daily use of injections, and when the peculiar circumstances of the complaint render the administration of aperient medicines by the mouth inadmissible, great advantage may be derived from the application of purgative liniments to the abdomen. The one I have found most useful consists of four parts of castor oil and one part of tincture of jalap. This must be diligently rubbed into the region of the stomach every morning before the patient rises, and it must be done under the bed-clothes, lest the unpleasant odour should sicken the stomach. I am indebted to a medical friend for this suggestion, which I used with success in the case of a young gentleman, whose state had become almost hopeless.

"In constipated habits I have likewise occasionally derived very remarkable benefit from the use of nitric acid given in sufficient doses. It seems, like the carbonate of iron, to possess the advantage of combining tonic with aperient qualities.

"In connexion with this subject I may remark, that long-continued and repeated attacks of constipation, by enlarging the cæcum and colon, lay the foundation of other diseases. This happens most frequently in females, but is not uncommon among males. In such cases the enlargement of the guts may occasion either of two distinct forms of disease, both attributable to the retention and accumulation of hardened fæces. In one form the symptoms are calculated to mislead the medical attendant, by inducing him to believe that his patient is labouring under chronic hepatitis. Pain and tenderness, and in some, hardness, or even a degree of enlargement, are perceptible in the right hypochondrium, while the patient's aspect is bilious, and he not unfrequently complains of pains in the right shoulder. At times he is subject to violent fits of colic, or to what he compares to cramp in the stomach, particularly after the bowels have been confined, after eating vegetables calculated to generate flatulence, or after exposure to cold.

"In the other form, the general health suffers less; the pain and other local symptoms referred to the right hypochondrium are not complained of, but the patient is occasionally subject, particularly on exposure to the action of the causes before enumerated, to violent attacks of vomiting and pain in the belly, which are accompanied by the characteristic symptoms of intestinal obstruction. The circumstance that the immediate attack was apparently induced by some palpable and known cause, such as an error in diet, or exposure to cold, may here deceive the practitioner, and cause him to overlook the fecal accumulation, without whose removal recovery cannot take place. I and two other practitioners were several times deceived in the case of a gentleman, of a robust constitution and great strength of body; and the true cause of the sudden and dangerous colics to which he was subject, was not discovered until he happened to mention, that when a young man, he seldom went to stool more than once a week. This led to the suspicion of an enlarged colon, and ever since the attacks have readily yielded to large injections administered by means of Read's syringe, without which instrument he now never ventures to travel. The practical point that strictly claims our attention, is, that the period of life at which the patient becomes subject to these attacks, is often long subsequent to the cessation or diminution of the habit of constipation, and consequently the physician will not perceive the true cause of the complaint unless he questions the patient very accurately."

We cannot refrain from giving also the following:—

"*Milk Powders.*

"About six years ago a lady came to Dublin to be confined at her mother's house,

when I happened to be in attendance on another member of the family. A few days after the accouchement I was informed that she was in great distress on account of having so scanty a supply of milk, that it was declared impossible for her to go on with the nursing. Under these circumstances, and as her medical attendant had given up the matter as hopeless, her mother applied to me for something likely to produce the desired effect. Upon inquiry I found that her daughter, who was a strong healthy young woman, was peculiarly anxious to be able to nurse this her first child, and could scarcely rest, so frequently did she give the child the breast, in order to try whether, to use a vulgar expression, the milk was coming. I immediately suspected that her over-anxiety about the matter, and the manner in which her whole attention was constantly turned to the secreting organ, had a sinister influence upon its functions, and thus prevented the secretion of milk. My object, therefore, was to divert for a time the current of her thoughts from that subject, in order to give the mammary glands an opportunity of performing their office undisturbed by her state of mind. To accomplish this it was necessary to defer her hopes of having a supply of milk to some future day; and I therefore gave her powders, consisting of calcined magnesia and aromatic powder, which I assured her would have the effect of bringing abundance of milk to her breasts at the expiration of two days. I directed that one of the powders should be taken every third hour, both night and day, and that the infant should not be put to the breast until the two days had elapsed. I laid great stress upon their being taken precisely at the hours specified, and told her not to uncover or examine the breasts until my next visit. The powders were marked, "German milk powders," and their whitish colour, I hoped, would favour her confidence in their efficacy. My expectations were realised. Before twenty hours the flow of milk was abundant; and in two days afterwards I had a visit from her accoucheur, who came to beg, as a special favour, my recipe for the German milk powders!"

MEDICAL GAZETTE.

Saturday, July 28, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

MEDICAL EVIDENCE ON LUNACY INQUESTS.

THE more we reflect on Miss Bagster's case, the more reason we have for being dissatisfied with the verdict of the jury. The unfortunate young lady has most assuredly fallen a victim to a monstrously-mismanaged education, rather than to any actual unsoundness of mind; yet as assuredly might she have escaped that fate were it not for the concurrence of circumstances which led to the late commission on the state of her mind. She had escaped the trammels of her nonage—became legally the mistress of herself and her property—had speculations of matrimony upon her hands—and was persuaded to let herself be run away with. But the husband whom she got in this way happened to be the "detestation" of her mother, who vowed that he "should have her heart and life's blood first before he should have her child:" a vow so strongly expressed was not allowed to evaporate in forgetfulness: her child could only be wrested out of the hands of the "detested" husband by impugning the soundness of her mind: it was proposed to try a lunacy commission: it is done: the eccentricities, perversities of temper, and ignorances of the unhappy girl, are raked up from her earliest childhood, and set forth with all due show of plausible evidence, and the commission *succeeds.*

There was a dilemma, undoubtedly, presented to the mind of the public in forming a judgment as to how this case ought to be determined. On the one hand, few could satisfy themselves that the ends of justice would be answered by such a verdict as would confirm the abductor's right to his rich prize, especially as there were more than rumours abroad by no means favourable to his character; whilst on the other, both the leading features of the case, and the nature of the evidence, were of too questionable a description on which to found a decision depriving the object of inquiry of the rights and privileges of a rational human being. But the latter consideration was the

only one which was proper to be entertained with reference to the ends of justice: the only question that should, consistently with purity of inference, be asked was, whether Miss Bagster at the time of her elopement was fit to be intrusted with the management of her own affairs, in the same way as the generality of females of her station in society are wont to be: the problem for the jury to solve was, whether there was any thing in the lady's history to prove that at the time of her running off to Gretna-Green she was mad or idiotical. They found that she was of "unsound mind."

The phrase, "to be of unsound mind," is one of great obscurity, and what is worse, is every day becoming more and more so. Every new lunacy inquest adds something to the difficulty of explaining what it means. It is a "bottomless" phrase, that swallows up in the undefined capacity of its extension all the proofs and facts of whatsoever kind offered to its undistinguishing appetite. It is a phrase, we will venture to say, in the meaning of which no two professional persons —be they lawyers, physicians, or divines, can be found to agree: yet it falls to the lot of a jury, composed, as for the most part the juries of this great metropolis are, of commercial men, to determine whether this person or that, whatever may be the mode of life, education, or sex of the said person, be of unsound mind. What is more natural than that such referees should decide as the habits of *their* lives and *their* notions of soundness and unsoundness should dictate? And, accordingly, what extraordinary verdicts do we not witness from them!

We made some remarks last week on the application of an arithmetical test in cases of alledged lunacy: it seems to be the great, if not the only test, which our London lunacy juries are in the habit of applying. Miss Bagster is brought in of unsound mind, apparently from being exceedingly ignorant of the simplest arithmetical facts. Davies, the tea-dealer, of whose insanity there could be infinitely less question than of Miss Bagster's imbecility, was not long ago pronounced to be of *sound* mind, because, we suppose, he was a most expert man in his counting-house. Davies was particularly clever at his trade, and used to make large purchases at the India House: he netted from 3 to 4000*l.* per annum by his business; and, in fact, was conducting that business for the very people, and at their earnest request, who, at the very time, were procuring a commission to pronounce on his insanity. This it was, no doubt, that saved Davies with the jury; and there is certainly some show of consistency in their conduct in applying the same test and rule of soundness to the less fortunate Miss Bagster. But if from the jury we turn to the medical men who gave evidence in the respective cases, we find, so far from consistency, nothing but doubt and confusion in what seem to be their views. The same Sir George Tuthill, Dr. Munro, and Dr. Sutherland, who gave evidence in Miss Bagster's case, and who laid so much stress upon her ignorance of arithmetic as to pronounce her, chiefly upon that ground, incompetent and imbecile, were the very parties who came to the unanimous opinion that Davies was insane—Davies, whom the jury afterwards found to be sane, and who for many years had shewn himself to be, and at the very moment of the investigation upon him was, a better arithmetician by far than all his doctors put together, even though Dr. Roots were reckoned among the number. Dr. Macmichael was a witness in the same cause, and we believe the only medical man called on this occasion who stood out for the sanity of Davies; but whether he did so or not, on the arithmetical principle, which he insisted upon so peremptorily in Miss

Bagster's case, we do not at this moment remember.

But let us take a view of the medical evidence in order: who knows but by its *rapprochement* we may be able to illustrate what is meant by "unsoundness of mind?"—and come we first to the evidence of the doctor last mentioned. In her conversation with Dr. Macmichael, Miss Bagster gave a fair account of her elopement and marriage; but when asked whether she considered herself the wife of Mr. Newton?—she said, "No; because the ceremony was not performed in a church and by book"—the very thing which she was taught to say by her mother and all who were about her after her return from Springfield. In Alderman Kelly's family, where she then resided, it was constantly impressed upon her that she was not the wife of Newton, and that what had taken place was no marriage. Dr. Macmichael further procured such information from her as satisfied him that the marriage was consummated; and he did not perceive any appearances of modesty in the lady when giving him that information. But now comes *the* test:—

"I examined her *respecting figures;* but she seemed perfectly *ignorant how many pence were in a shilling.* I think she has no reasoning faculty whatever. She said there were six weeks in the year, and six days in the week, and that Sunday was kept holy. I wrote down a common *sum in addition; but she could not manage it, and said she was very stupid. She could not add shillings and pence* together. I do not think she is competent to manage her own affairs, nor to be trusted with any money. In my opinion *she is of unsound mind.*"

The conclusion is positive enough, and, we presume, is to be understood as resulting from something more than appears in the evidence, at least to us it does not seem quite inferrible from the premises,—unless, indeed, "unsoundness of mind" be understood in some new acceptation. The passage of the evidence before us shews that Miss Bagster was very backward and ignorant respecting her figures, and her "tables;" but surely ignorance even many degrees darker than this bears no relationship to unsoundness; if it does, we fear a very large proportion of the rich but unarithmetical vulgar stand in a much more awkward predicament than they imagine. "She said she was very stupid." So has many a sensible woman, and many an eminent man, when attempting to accomplish a difficulty. It is known that the late Mr. Canning, when Chancellor of the Exchequer, used to say the very same thing, when puzzling over some of the simplest calculations; and every one will recollect some or other of his acquaintances who are equally inexpert. True, they may not be so grossly ignorant as to mistake the number of pence in a shilling, or the number of days in the week: but all this is relative; and the pence-table no doubt was as intricate a piece of mental exertion to poor Miss Bagster, as making up the quarter's accounts to the youngest officer about the Exchequer; and unsoundness of mind were as chargeable in one case as the other.

Dr. Sutherland learned from her that she thought the whole business of the elopement a capital "bit of fun;" that she laughed during the marriage-ceremony, thinking it "all fun;" that she considered her name to be Bagster and not Newton; and that she disliked her mother, because they could never agree. The doctor also examined her in arithmetic, and he too found that she was—of unsound mind.

Dr. Gordon drew from Miss Bagster certain disclosures relative to the "marriage state," the recital of which drove the female auditors out of court, and imposed silence on the reporters. He said she gave her answers without any reluctance, or a blush; and he had come

to the conclusion that she was very imbecile.

It is with considerable surprise we read what has transpired on this trial, relative to the gaining and subsequent betrayal of confidence on the part of medical witnesses. The course pursued in criminal cases is humanity to this; there the accused is put on his guard, and warned to say nothing that may criminate himself; here certain mad-doctors are introduced, in the disguise of friends of Alderman Kelly, who, like confessors of the Holy Office, worm from the victim every minute particular that may afterwards be turned to her confusion. We cast no imputation on the individuals who acted in this questionable way; it is the system that we condemn—a system, too, as ineffectual frequently as it is ungenerous.

Perhaps the least favourable specimen of this inquisitorial mode of proceeding is to be found in Mr. Bowling's evidence. This gentleman had been for ten or twelve years the family surgeon in Miss Bagster's family, and to him, it appears, "as an old friend," Miss B. had no hesitation in telling, upon being questioned, all that took place both before and after the marriage. The simple girl never once dreamt that all she said was to be repeated in a court of lunacy inquiry: but her simplicity was thrown into the scale of her "unsoundness."

Dr. Munroe, we were surprised to find, was another of those witnesses who attached paramount importance to arithmetic. He found that she stumbled a little at adding 6 and 4, though she presently set herself right: but she could not tell how many shillings were in a crown or a sovereign; nor give even a good guess at the amount of Alderman Kelly's fortune or her own. "She is decidedly of unsound mind," says the Doctor: "I should not call her idiotic or lunatic; but I should say extremely imbecile." Much to the same effect was the examination of Dr. Roots, of which, with reference to its details, we must take leave to say, that it was a far more complete and curious sample of simplicity on the part of the Doctor, than of imbecility on the part of the patient. After some general conversation, says Dr. Roots,

"I asked her, *if she had* 10,000*l. in the four per cents, how much ought to be her income*? She answered, 100*l.* per annum. I asked her how she should live on a hundred a year. She said she should take a great house in the West end, and keep a number of servants; but she doubted if she could afford to keep a carriage at the same time. . . . Among other questions, I asked her what would she give for *a quartern loaf?* She said, two shillings. She was aware that twenty shillings made a sovereign, and twenty pence one and eight-pence; but she said that 50 sovereigns were twice as much as a 50*l.* note. . . . She complained of her mother for not being kind to her. I asked her, what would be *the expense of a carriage and pair of horses?* She said, I should think from 20*l.* to 30*l.* a year. He considered her to be of unsound mind."

Here is an *experimentum crucis* for the sanity of young ladies! The four per cents, carriage and horses, and the price of bread! Why, Dr. Roots surely must have very odd notions on such matters. He is sent to examine the state of mind of a young female of neglected education, and to form an opinion whether she has capacity enough to manage her own affairs: he is told that she has a large property, and he sets literally about his work: he tries her on "questions for exercise," from the four per cents, in simple interest, down to the price of bread as fixed by the last assize: he conceives that having money in the funds, it will be necessary for her to be accomplished in the art of buying and selling stock; and that, as she might become the mother of a family, she ought to be attentive to the price of quartern loaves; and, no doubt, if she had answered his problem about the

four per cents, he would have given her something more knotty, such as the extraction of *roots*, or the solution of cubic equations. What lofty notions the Doctor must have of the intellects of young ladies in general! and how fortunate will they be who can pass muster, should he ever be sent to take measure of their ability for managing their own affairs!

We have now done with the arithmeticians, or, at least, those of them who make a knowledge of "figures" a *sine qua non* in estimating soundness of mind. And it is refreshing to turn from them and their system to the evidence of Dr. Morison, than which we do not recollect to have ever read any thing more creditable and (to us, at least) satisfactory delivered in a court of justice. It was a valuable body of facts and opinions collected in an honest, straightforward way, without any recourse being had to deception or masquerading, — for we are persuaded that there was but too much of this sort of wise contrivance adopted in the several examinations of Miss Bagster. "The great excuse for the falsehoods," says the *True Sun*, (which has ably followed up the discussion of the most prominent points in Miss Bagster's case,) "is, that the necessary information could not be obtained without them. We deny this assertion by a counter-assertion, and insist that it could. The address of mingled kindness and truth is at all times better than that of falsehood; Dr. Morison, observe, found it so: and supposing that, in some instances, it were not, then comes the great and all-important question, what right any set of human beings have to make use of their sophistications at all with a fellow-creature, whose education and right treatment have still to commence, and who has already been deeply injured by the folly of convenient falsehoods?"

In the report of Dr. Morison's evidence we find the following:—

"I visited Miss Bagster four times. She was a little deaf—from nervousness, as she said. She complained of being very nervous, from seeing so many persons and having to answer so many questions as to the state of her mind. On asking her how she became so defective in her arithmetic, she said her grandfather was very fond of her, and indulged her too much, and would never allow her to be teased about it. Speaking of her fortune, she said *her mother never let her have any money—not even a penny to give to a beggar; and that she had had no opportunity of knowing the value of money.* At my last visit I said, 'Now you are married, how do you think you ought to sign your name?' She said, 'I think it ought to be Newton; but *those about me say it was no marriage, as it was not done in a church.*' She asked a great many questions about the law proceedings which were about to take place, and said, 'I have been very violent and passionate, and undutiful to my mother, for I have sometimes pulled and slapt her; and I am very sensible of the impropriety of my conduct.' * * * * Miss Bagster is *a little deaf, and in that respect only is different from other young ladies.* I did not observe in her the slightest disposition to laugh without cause: she had none of the unmeaning laugh and titter of those who are weak in mind. I do not believe that her governess had fallen on the right method of instructing her, and *I would undertake, in six months, to teach her arithmetic and the use of money.* She has begun to think now, and her mind is more opened than it was a year ago; and if pains were taken with her she might be instructed. Such a communication as she is represented to have made to Captain Kelly was very indecent and strange, but, in my opinion, not inconsistent with sanity. I think that late occurrences, coupled with her repeated conversations with the medical men on indelicate subjects, so lessened her sense of modesty as to account for it. *A deficiency of education will account for all the appearances observed in Miss Bagster.* Her incompetency to manage her affairs arises, not from unsoundness of mind, but from ignorance. *She is capable of instruction, so as to be able to manage her affairs.* The indulgence of her grand-

father, the conduct of her mother towards her, and the frequent change of her teachers, were calculated to produce the results which we see."

If we had room, we should confirm this excellent testimony with several passages from Miss Bagster's own evidence before the Commissioners; but we must be content to refer the reader to it: it is given in our last number, and we will venture to say, that if any man will but peruse it impartially (recollecting, at the same time, the circumstances under which it was given), and then consider it in connexion with the above evidence of Dr. Morison, he will at once admit that a stronger case was never made out in favour of any alledged lunatic.

Dr. Haslam was the last of the medical witnesses examined: we shall not, however, trust ourselves with a comment on what he deposed, further than just to say, that along with much to commend there was also a great deal to reject, in what was elicited from him; and we can only wonder that one who professes to be such a master of evidence and of the best mode of delivering it in the witness-box, should allow himself to be betrayed into conceits and extravagances, and ridiculous sparring with counsel.

Here we must bring our observations to a close; and in doing so we cannot refrain from once more expressing our unqualified dissent from the verdict of the jury—that is, so far as we can understand what they mean by "unsoundness of mind." The result of the decision, however, is plain and intelligible enough, whatever its language may be, and excites our strongest sympathy. It deprives the convicted mentally-unsound person of liberty and property at once, never again to be enjoyed but by the most precarious of all chances—the successful issue of another inquiry, completely reversing the late one. For such an issue, nothing that we are acquainted with in the history of convicted lunatics gives us any very flattering reason to hope; but we have a right to expect—to demand—that such pains be bestowed upon the unfortunate object of the late Commission as that ignorance, and thereby confirmed imbecility, be not entailed upon her; and that the remainder of her days may not be spent in unmeaning helplessness, destitute of the solace of education.

DEATH OF BARON PORTAL.

We are sorry to announce the death of Baron Portal, which took place at Paris on the 23d instant, at half-past ten in the morning: he died of a fever, occasioned by a long continued affection of the stone. He was born at Gaillac (Tarn) on the 5th of January, 1742, and consequently was in the 91st year of his age. The Baron was a member of the Institute; Professor of Anatomy at the Royal College of France; a member of the Council General of the Hospitals, &c.: formerly, he was physician to Louis XVIII. and Charles X.

CHOLERA IN LONDON.

We have made it our business to ascertain, as nearly as possible, the state of the metropolis as to cholera, and the result has convinced us that the extent of the evil has been much exaggerated. The occurrence of some deaths among persons moving in the higher walks of life has led to a panic, and its usual consequences, for every rich case is immediately known to all the town. We have made out, on what we believe to be satisfactory grounds, that though the actual number of cases now under treatment at the various public establishments be greater than it was on this day last week, yet this is owing to the gradual accumulation which necessarily takes place from the continuance of the disease, and not to an actual increase in the number of attacks occurring each day: on the contrary, the result of our inquiries has satisfied us that there has been upon the whole a diminution to a decided, though not as yet to a very great, extent.

EFFECTS OF FRUIT AS REGARDS CHOLERA.

We have seen rather a curious document, drawn up by some of the chief growers of fruit and vegetables in the villages round London. It is stated on the authority of twenty-one such persons, whose names are appended, that up to July the 24th (when it is dated), of 1010 labourers of either sex employed in their gardens, one only was indisposed, and not one had had cholera. Their inference is, that fruit and vegetables are not favourable to the production of that disease; but it does not appear to us that the premises warrant the conclusion. Is it the fact that those labourers eat a larger portion of fruit and vegetables than others? It is notorious with regard to pastry cooks, confectioners, and such persons, that they do not consume more—if so much—of their commodities as others; and certainly persons so situated as the thousand and ten above mentioned, are much less likely than others to commit any excess in regard to the articles in question. It is not against the use, but the abuse, of "the kindly fruits of the earth," that we protest; and we are quite sure that many cases of cholera have been produced by unripe fruit and raw vegetables (as cucumbers), taken even in moderate quantity; and that great caution is necessary in this respect, notwithstanding the declaration of the growers.

DR J. JOHNSON IN REPLY TO MR. ARNOTT.

To the Editor of the London Medical Gazette.

Sir,

I am rather surprised that Mr. Arnott should consider me base or envious enough to detract from the merits of his discoveries, for no earthly reason that I can discover. The fact is, I never wrote or read a line of the critique complained of till after I saw Mr. Arnott's letter in the Medical Gazette. Of this I have tendered you proof by offering to refer Mr. A. at any time to the writer of the article. That writer forwarded a reply to Mr. A.'s accusation; but as I understand from you that his reply cannot be inserted in the Medical Gazette, being anonymous*, the matter must stand over till my own journal is published, when I shall pay my respects to Mr. Arnott. In the meantime, I beg to protest against the practice of singling out, by name, the editor of a journal, and charging him personally with every sentiment and expression in the different reviews of the journal which he superintends. The profession will judge how far Mr. Arnott was justified in his personal attack on me when the reply alluded to appears in the Medico-Chirurgical Review.

I am, sir, with respect,
Your obedient servant,
James Johnson.

26th July, 1832.

MIDDLESEX HOSPITAL.

Dry Gangrene of the Foot and Leg.

There is now in Northumberland ward a man who lately presented a remarkable example of this disease. W. S. 64 years of age, came into the hospital the 22d of May. The left foot, and half of the leg, were of a black, or rather dark-brown colour, shrunk in size, shrivelled, and perfectly dry, precisely like a piece of mummy, and singularly contrasting with the living parts to which they were attached. The transition from the dead to the living surface was abrupt, and not marked by any line of separation; that of demarcation was not circular, the mummified portion extending higher up the outside of the leg than the inner; in the former to within a couple of inches of the head of the fibula. On the living part of the leg there is one isolated spot, and on the tip of the knee another, of black, thick, and projecting skin, each about the size of a shilling. The patient does not complain of pain in the part; he is thin, but his complexion is clear, his appetite good, and his pulse 65.

This man is a working gardener. He states that in the course of last winter he was in destitute circumstances, and suffered severely from rheumatism, more especially in the left hip, and afterwards in the neck and shoulders. When he had so far recovered as to be able to walk with the assistance of a stick, about ten weeks ago the left foot and leg swelled, felt as cold as a stone, but was not discoloured. A spot then appeared on the instep, to which some herbs were applied for a day or two, and on removing these, a black spot, the size of a penny, was seen; this subse-

* We did not choose to insert an anonymous answer to a letter addressed to Dr. Johnson, and bearing the signature of the writer; but if Dr. J. will guarantee the authenticity of the answer, or if the author of it will give the sanction of his name, it shall appear.

quently extended to the ankle and the toes, and then up the leg, sometimes, as *he* terms it, standing still for a week, and then starting again, still unaccompanied by pain. He has not been in the habit of living upon rye bread, and until the present winter has always been in good health. The femoral artery of the left side feels perhaps a little more cord-like than natural.

This man had been sent up from the country to have his leg amputated; but experience having shewn that in such cases this operation (when accomplished by the knife) seldom succeeds, the stump usually assuming an unhealthy character, it was not resorted to. It was determined to see what nature would effect, if properly supported. The patient was ordered full diet; first a pint, then a pint and a half of porter daily; six ounces of wine, and a drachm of laudanum at night to ensure sleep, and which had the desired effect. He also took quinine; and when this disturbed his bowels, he had decoction of sarsaparilla with the extract.

Under this plan of treatment the case proceeded favourably; the disease did not extend; a narrow border of red integument formed round the dead part, and ulceration commenced. By the 29th of June, the separation of the soft parts was so far completed, that after the tendinous parts had been divided with the scissors, the bones of the leg were sawn through. The end of the tibia bleeding somewhat freely, it was stopped by pressure with lint. The few points of sloughing matter attached to the stump afterwards separated, and the latter assumed a florid and healthy appearance, and is now (20th July) considerably advanced towards cicatrization.

Entropium—Operation.

J. B. æt. 40, came into Stafford ward with catarrho-rheumatic ophthalmia of the left eye, and entropium of the upper lid of the right. It appeared that he had been attacked with inflammation of both eyes three years ago, and that whenever he had taken cold subsequently, they were again affected. By low diet; by calomel every eight hours, so as to affect the mouth; by the application once a day of a solution of four grains of nitrate of silver in an ounce of water; and by anodyne fomentations, the disease in the left eye was removed. Attention was then directed to remedy that in the left.

The inversion did not occupy the whole of the upper eyelid, only about a half, the outer, but it was complete, the ciliæ being directed fairly against the globe, the conjunctiva of which was strewed with red vessels, the surface of the cornea hazy, and vision greatly impaired. It required some exertion to evert the part affected; and when this was done, it was seen that its ciliary margin was altered in form, and that on its mucous surface there was an appearance as if an ulcer had here existed; and that by the contraction of the cicatrix, the entropium had been produced, an opinion which was strengthened by the account of the patient. Under these circumstances, Mr. Arnott pointed out that the mere removal of a portion of the skin of the eyelid would not be sufficient to cure the disease; the membrane investing the cartilage had been affected—perhaps the cartilage itself; and it would be therefore necessary to divide them. On the 29th of June the following operation was performed by Mr. A.

One blade of a pair of straight scissors being carried beneath the upper eyelid, exactly at the internal limit of the inverted portion, the lid was divided perpendicularly upwards to the extent of about a quarter of an inch; and the blade of the scissors being again carried under the upper eyelid, close to the outer canthus, a similar division of parts was effected here. The inverted portion being thus set free, it was readily everted; but to retain it so, a piece of the integuments of the eyelid covering and opposite to it, was removed by the scissors; and the lips of this wound being brought together by two stitches, the ligatures forming these were carried up to the forehead, and retained there by adhesive straps, so as to keep the part which had been inverted everted during the cure. The perpendicular wounds were allowed to heal by granulation, Mr. Arnott twice breaking them down with a probe, so that they might not do so too quickly: this took place in less than three weeks, the horizontal one of the integuments healing by the first intention.

The success of the operation was complete, the faulty position being permanently remedied; the cornea recovered its transparency, and vision was restored, with no perceptible deformity, the small notch on the middle of the upper eyelid being unobserved, except when attention was called to it. The patient leaves the hospital on the 24th instant.

METEOROLOGICAL JOURNAL,

July 1832.	Thermometer.		Barometer.	
Thursday . 19	from 45	to 68	30·06	to 30·11
Friday. . . 20	40	65	30·13	30·16
Saturday . 21	42	63	30·18	30·20
Sunday . . 22	40	62	30 14	30·13
Monday . . 23	38	63	30·13	Stat.
Tuesday . 24	48	67	30·11	30 15
Wednesday 25	49	69	30·15	30·11

Prevailing wind, N.W.

Except the three first days, cloudy; a few drops of rain in the evening of the 24th.

Charles Henry Adams.

NOTICE.

The Reviewer in the Medico-Chirurgical Review.—We cannot insert an anonymous reply to a letter bearing a real signature.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 4, 1832.

CASES OF HYDRENCEPHALOID DISEASE.

COMMUNICATED BY

MARSHALL HALL, M.D. F.R.S. L. & E. &c.

To the Editor of the London Medical Gazette.

SIR,

SEVERAL friends having communicated to me cases of the hydrencephaloid disease, of which various notices have already appeared in the Medical Gazette, I think it may not be amiss to add them to your pages.

For the first case I am indebted to Mr. Heming, of Kentish Town. Mr. Heming observes, in a letter addressed to me,—

"I was first requested to see Miss G. aged 5 years, on the 18th of February: I found her pallid, with a cold skin, the tongue a little coated, the gait in walking feeble and tottering, the pulse feeble, and about 100; to these symptoms were added frequent vomiting, especially on taking food, and constipation of the bowels.

"I observed to Miss G.'s parents, that vomiting and constipation frequently denoted serious affections of the brain, and that the utmost attention must, therefore, be paid to their little patient's case. Calomel and purgative medicines were given. The bowels were purged, and the vomiting ceased; the evacuations were dark coloured and extremely offensive.

"The purgative medicines were continued until the 28th. The vomiting did not recur. The evacuations were still dark and offensive. The countenance was pale, the skin cold; she walked unsteadily, and had not her wonted cheerfulness. Still my patient's friends seemed to think there was little further need of medicine, and I did not see her again until the 1st of April.

"In the evening of that day I learnt that Miss G. had been apparently mending, although she continued pale; that on the preceding 28th of March, she had partaken of cakes, &c. at a party of children; and that early in the morning she had been seized with convulsions, for which the jugular vein had been opened, leeches applied, and purgative medicines given. I found her with a frequent pulse, hot skin, and furred tongue; the pupil contracted on exposure to light.

"A fortnight after this period, this little girl was pallid, the whole surface was blanched, the tongue was clean, but, with the whole lining of the mouth, very pale, the surface was cold, the pulse feeble, and the limbs incapable of supporting her. In a bright light, the pupils contracted nearly as those of other children; but in the shade, they were unusually dilated; when asleep, they were found also greatly dilated. The alvine evacuations were still dark coloured and offensive. The bowels were moved at the moment of my visit, and the exhaustion was so great as to induce the parents of the little patient to consider her case as hopeless. With ammonia and beef-tea, however, she rallied, and from this period, with great attention to cordials and nourishment, she gradually improved, and ultimately recovered her health.

"This case appears to me to possess great interest. It combined the symptoms of disease within the head, first from deranged bowels, secondly from improper food, and thirdly from exhaustion, consecutively, so distinctly, as perfectly to confirm the statements made in your little pamphlet on this subject."

To Mr. Heming I am also indebted for the second case:—

"E. Simpson, aged 2½ years, was remarkably healthy up to the time of the present attack, which commenced on the 20th

of January, with a hot skin, quick pulse, and a white and furred tongue. On the 22d, the left labia pudenda was perceived to be red and swollen. On the 23d, this redness had considerably extended itself; and, by the 25th, when I first saw her, it was spread over the whole of the nates, covering the back part of the thighs more than two-thirds of their length downwards. It extended over the back, from the lowest part of the sacrum nearly to the inferior angle of the scapula, and came round on each side as far forward as the spine of the ilium. The bowels had been attended to, and quinine was given, but, as it was thought at this time to disagree, it was discontinued. The argenti nitras was applied so as to form a line of the breadth of three-fourths of an inch, (which was black in some places, and blistered in others,) completely encircling the inflamed parts. This was done in the evening at 8 o'clock, and for the next twenty-four hours the inflammation had in no part extended over the boundaries formed by it, and all within the line had become very much paler. The inflammation, however, after this period began again to spread and extend beyond the boundaries made by the nitrate of silver, passing over that part of the line first which was blistered; but it never occupied so great an extent as before the nitrate of silver was applied, for although new parts became affected on the outside of the line, it became quite well within.

"On the 1st of February, a grain of sulphate of quinine was prescribed to be given every four hours, and an opiate at night; and by the 6th, the inflammation had entirely subsided. A few days after this date, I was much disappointed to find this child not well. But it was now affected with a new disease. The mother asked me if I thought the child had water in the head, and upon my inquiring why she put such a question, she said that it had lain perfectly insensible, had neither smiled nor cried, and had scarcely taken any food for the last twenty-four hours. The child was lying on its back, very pallid, the skin cold, the pulse weak, the eyelids half closed; it was incapable of being roused from its insensibility by any thing that was done to it; the pupils were dilated, and did not contract upon the application of the strongest light. A few drops of the Sp. Ammon. Aromat. were given frequently; as much beef-tea as could be got down; and when it could be taken, some solid animal food; and in twenty-four hours, this child noticed any thing presented to it, smiled, played, and seemed comparatively well.

"This case seems to me to be one of more than usual interest. Nothing is more common than to hear of children dying with effusion within the head, as a consequence of some of the exanthemata; and it is probable that many of these have been such cases as the one related.

"Such a state of exhaustion is not at all unlikely to come on in children in the later periods of scarlatina, measles, erysipelas, &c. particularly where these diseases have been more than usually severe, or of long duration.

"I would here just remark, that frequently too little attention is paid to the diet of children in acute diseases, and that purgatives are apt to be continued too long. We substitute for their usual nourishment in the commencement of acute disease, a diet of a much less nutritious quality; and the acute character of the complaint has frequently long subsided before we think of their returning to food of a more substantial nature. A child who has suffered with an acute disease, and has been, when the disease has disappeared, too long kept upon a low diet, frequently indicates its state of lowness by a peculiar sucking motion with its lips; it becomes sleepless, irritable, and cross; and after a single meal of more solid food, will often immediately go to sleep, and awake smiling, playful, and well.

"I do not, however, mean to deny that inflammation of the brain or its membranes sometimes comes on in eruptive fevers—an inflammation too, most dangerous, and often rapidly fatal, in which it would be as certainly destructive to the little patient to omit bleeding, as it would be in such a case as that I have detailed, to adopt such a measure. They are cases that bear bleeding well, and to save such patients a large quantity of blood must be taken. The cases which Gölis has called water-stroke, are evidently cases of inflammation of the brain in the most aggravated form, supervening upon eruptive fever; and it is not a little surprising to find a man of his extensive experience in diseases of children, if not absolutely condemning bleeding, stating that he has never found it useful. A surgeon who has very recently quoted this part of Gölis's work holds also the same opinion as respects bleeding. This view of so beneficial a remedy must surely have arisen from their not having carried it far enough. The first case of this kind which I saw came on in consequence of measles. I did not bleed, and a few hours terminated the life of my patient. I have seen several subsequently; they were copiously bled from the jugular vein, and all got well.

"I confess that I have not seen a sufficient number of cases to say much on the diagnosis of these two very important disorders, so similar in their symptoms, but requiring such different modes of treatment. The importance of such a distinction must be sufficiently evident. If we attend to the period of the attack, the heat of the body, the pulse, the colour and temperature

of the skin, as well as the colour of the eye, we may be somewhat assisted in forming our judgment. In the former cases the disease comes on always in the late period of the disease; the patient is very pallid, the tunica albuginea is white or bluish, the skin cool, the pulse weak, and for the most part the eyelids half closed. In the latter cases the vessels of the eye are injected, so as to give the tunica albuginea a red appearance; the skin is warm, and the face is often flushed; the pulse is strong, and there is sometimes stertorous breathing. This attack comes on early in the disease, sometimes during the eruption; if it do occur at a much later period, it is frequently consequent upon an inflammatory anasarca, which often follows eruptive diseases."

The third case was communicated to me by my friend Dr. Harwood, of Hastings, with the following note:—

"My dear Sir,—The following case appears to me to approximate so nearly to those detailed in your interesting and useful tract on 'A Morbid Affection of Infancy,' and to assist so much in corroborating the justice of your view of treatment therein given, that I have been induced to transmit to you a copy of it from my note-book, with the hope that, should you consider it worthy of publicity, you will afford it in what way you please.

"Believe me, my dear sir,
"Yours truly,
"Wm. Harwood.

"Hastings, October 14, 1831.

"Oct. 1st.—I was requested this evening to see a child aged nearly three years, which was represented as being very dangerously ill, and to meet the surgeon who had been for some time in attendance upon it. On arrival I found it in its mother's arms, very feeble, moaning, and restless; its pulse 130, rather sharp; skin hot; countenance a little flushed; tongue much coated, but moist; breathing somewhat hurried; pupils obedient to the impulse of light; abdomen rather distended with flatus; motions very offensive, green, curdled, and loose. I understood also that during the day it had been heavy, comatose, pallid, and rather cold than otherwise; and that on waking, when asked where its pain was, it occasionally put its hands up to its head. This state of things, it appeared, had existed for some days, it being heavy and comatose during the day, and feverish and restless in the evening; but it had become daily more disposed to dose, and indisposed to every kind of movement, and the feverish paroxysms were now less urgent than heretofore. It now slept with its eyes half closed, and frequently rolled the balls of them upwards. I found also that its head had appeared to be affected, chiefly during the last fortnight, within which time leeches had been twice applied, and a dose of calomel had been given every night, succeeded in the morning by a saline aperient draught, and antimonial diaphoretics at stated hours through the day. Its flesh and strength had not only been materially reduced during this time, but also previously, by a diarrhœa which had existed six or seven weeks, accompanied by five or six motions a day, and which still continued, when the head first appeared affected. The medical gentleman who attended, considering the case decidedly hydrocephalic, had recommended the constant application of cold lotions to the head, which appeared grateful to the child, and the depletory measures before spoken of, and had expressed his belief to the friends that the child could not live a fortnight. Six leeches had been applied on the evening of my visit, and strict injunctions had been given that the child should take nothing better than tea, or the thinnest gruel, on which it had now lived for a fortnight.

"Judging from the state of the pupils that effusion had not actually taken place, and from the absence of fever during the day that no great inflammatory action existed in the brain, and thinking it likely that the evening paroxysms might arise from irritability of the system, dependent on debility kept up by the calomel purgatives, and feeling assured also that irritation in the bowels induced by this remedy might explain the nature of the motions and tend to maintain the coated state of the tongue, I was induced to recommend the discontinuance of all medicine, besides a little rhubarb and magnesia, should it be required, and advised giving the child nourishment in the form of a little strong beef-tea every two or three hours.

"October 2d.—The child has taken nearly a small tea-cupful of the beef-tea during the night, but has been very heavy and comatose. Pulse this morning 130, small; no motion; no flush, or indication of fever. 8 P.M.—Pulse 13; more restless and heated than in the morning, but not so much so as last night; one liquid motion during the day, of better colour. To continue the beef-tea, and sop in it a little toasted bread.

"October 3d, 10 A.M.—Has taken a tea-cupful and a half in the night, but has been more restless. Pulse 120, and rather more full than yesterday at this time; skin moist, and of natural temperature. 8 P.M.—Child in all respects more comfortable, and has asked for the broth once or twice during the day.

"From this time the child made marked improvement; its tongue began to clean, its evening fever abated, its motions became more healthy and natural, requiring

to take only one dose of the rhubarb and magnesia; it increased in strength, and on the 7th was so much recruited that I discontinued my visits."

The fourth case I owe to the kindness of Mr. Toulmin, of Clapton.

"Clapton, 10th January, 1832.

"Dear Sir,—The following are the facts connected with the case which I mentioned to you the other day.

"I was requested to visit the child of a chemist and druggist, whom I found to be two years and three months old. It was pallid; the subcutaneous vessels very visible; had scarcely any hair on the head, and that of a light and silky character. The fingers were swollen, not pitting on pressure, and slightly transparent, like alabaster. The pulse was rapid and feeble. The child lay on its mother's lap, with the eyes half open, and noticing nothing passing around, but when aroused moaned and cried. The pupils of the eyes were slightly dilated, but the iris perfectly irritable to the stimulus of light. The sclerotic blue, and conjunctiva bloodless.

"I was told that the child had had disorder of the bowels, which was attributed to teething; that it had become heavy and oppressed; that the head was suspected of having its vessels loaded, and that hydrocephalus most probably would supervene; and that thereupon leeches were applied to the temple, calomel purgatives were daily given, or hydr. c. cretâ, with rhubarb; and that, for the last fortnight, numerous daily slimy and greenish motions were induced, and the medicine repeated, because of the morbid motions produced by previous dosing. The child had been kept on the lowest diet.

"I was strongly impressed on seeing this child that its symptoms were induced by a lowered condition and deficient quantity of red blood. I therefore advised that the bowels should on no account be purged, but simply regulated by the smallest doses of castor oil; that strong beef-tea and milk should be given, and also five minims of sal volatile three times a-day, with a little infusion of cascarilla. The child in three days manifested improvement, and speedily got well under this treatment, and although very delicately organized, has remained tolerably healthy up to the present time.

"I am, dear sir,
"Yours, very faithfully,
"Frederick Toulmin."

The fifth case fell under the observation of my friend Mr. Hodding.

"W. E. ætat. thirteen months, a stout and hearty child, and still at the breast, was seized on the 12th October, 1831, with violent shivering, jactitation of the arms and legs, with great heat about the head, &c. As there was no tooth yet through, I scarified the gums deeply, and administered a brisk purgative, which operated copiously; but in the evening the patient was much worse, lying in a torpid comatose state, with that puffy swollen state of the features so peculiar to congestion about the head. Two leeches were applied behind each ear, and a blister to the nape of the neck, with cold lotion to the head. The leeches bled very freely, but still there was no amendment; but, on the contrary, convulsions, with contraction of the hands and feet, had taken place, and early in the morning there was coldness of the extremities, with symptoms of speedy sinking. I immediately administered cordials with weak warm brandy and water; and by persevering with the same, had the satisfaction of witnessing a gradual improvement, and at the end of a few days, the perfect recovery of the patient."

The cases of hydrencephaloid disease from intestinal irritation, and from exhaustion, and the case of real encephalic disease, are now, I believe, becoming generally understood, and are at once properly distinguished and treated. It is a source of sincere satisfaction and congratulation to me, to receive verbal communications almost daily confirmatory of the views which I had the good fortune first to publish on this interesting point in the diagnosis and treatment of some diseases of children.

I am, sir,
Yours faithfully,
Marshall Hall.

14, Manchester Square,
July 30, 1832.

NOTICE OF DR. PHILIP'S PAPER ON THE CIRCULATION OF THE BLOOD.

By Marshall Hall, M.D. F.R.S.L. & E. &c.

Observing that Dr. Philip's paper is republished from the Philosophical Transactions, in the Medical Gazette of last week, I think it may not be ill-timed for me, through the same medium, very briefly to point out several inaccuracies of fact and of reasoning, into which I think that gentleman has fallen.

First, the person least skilled in phy-

siological reasoning will discover that the experiment given in the first column of page 534, in opposition to Sir David Barry's views, really proves nothing at all. The chest is open; the effect of inspiration on the blood in the *veins* is removed, and yet the blood issues from the wounded pulsating femoral *artery*. Does such an experiment prove, that when the acts of inspiration are really performed, they have no influence on the *venous* circulation? By no means.

There is a mode of experimenting, however, which I fear leads to great doubt on the subject of Sir David Barry's views: it is that of inspecting the circulation in the wing of the bat. It is easy to see the effect of each pulsation of the *heart* on the course of the blood in the *arteries*; but I have not been able to detect any acceleration of the blood along the *veins*, at each inspiration. But the experiment requires repetition.

Dr. Philip observes, page 535, that "the motion of the blood in the capillaries continues long after the heart has ceased to beat," &c. Now I believe this is a mistake. It is a point, however, only to be determined by experiment. I should be happy to perform such an experiment before competent witnesses. The *appearance* may be *produced*, but it does not exist naturally.

Dr. Philip next asserts that the blood moves with "different degrees of velocity in the different" capillary "vessels of the" same "part," &c. page 535. I have inserted the word *capillary* to make the sense of the paragraph complete: it is incomplete without it. I believe the entire statement to be erroneous, and that, if there be no *external* force applied to one capillary which is not applied to the rest, the motion of the blood is of one uniform velocity in all. A simple experiment will prove it, and I shall be happy to make it. As in the former case, the *appearance* may, however, easily be *produced*.

Dr. Philip next states, page 535, 536, "it is impossible, in the motion of the blood in the capillaries, in the least degree to perceive the impulse given by the beating of the heart," &c.; and page 536, "if the circulation of the capillaries be thus independent of the heart, it is evident that the influence of that organ cannot extend to the veins," &c. I venture to assert that all this is wrong. I can prove it to the *eye* of any one in five minutes at any time, and shall be glad to do it. I think Dr. Philip's memory must have failed him.

Lastly, there is an experiment related (page 536,) to prove the action of the veins. It is impossible to characterize it without giving offence. I prefer, therefore, stating simply that a "collapsed" vessel is not an active, acting vessel, and that the experiment is altogether inadequate to the object in view. Besides I have repeated the experiment, and when this is done with proper precautions, to prevent the vein from being stretched and pressed to the soft parts, the result is any thing but that stated by Dr. Philip.

The real state of the facts on these points may be seen in my Essay on the Circulation, pp 98, 86, 87, xv.—xviii. 75, 163, &c. But the truly scientific mode of determining such points is that adopted by the Institute of France, of appointing a commission to witness the repetition of experiments, and to report upon them. A few hours devoted to this object by our own physiologists would not be unprofitable to them, or to their science; whilst years might be occupied in fruitless verbose discussions.

I must be allowed to add, that there is no circumstance connected with this paper of Dr. Philip which will ever redound to the honour and reputation of that gentleman, or of any one concerned.

REMARKS ON THE MALIGNANT DISEASES OF THE EYE*.

By R. Middlemore,
Assistant-Surgeon to the Birmingham Eye Infirmary.

To the Editor of the London Medical Gazette.

Sir,

In the number of your journal for April 21, an Inquirer, under the signature of "A Student," has asked me a question, in reply to which, or rather in continuation of my remarks on fungus hæmatodes of the eye, published in a former number of your journal, I have written the accompanying observations "On the Malignant Diseases of the

* This paper and the accompanying note, dated April 30th, were only received July 20th.—E. G.

Eye," which comprise not, I hope, an intemperate review of the opinions of many excellent writers upon this class of maladies, and at the same time recals attention to a new mode of attempting the cure of the fungoid affection of the eyeball, first proposed by me in the sixth volume of your journal.

I beg to remain,
Your very obedient servant,
RICHARD MIDDLEMORE.

Birmingham, Temple-Row,
April 30, 1832.

"A Student" has called my attention to the following statement, contained in your sixth volume:—"We find that the eye has been successfully extirpated when affected with fungus hæmatodes, sufficiently often to justify the practice, even at an advanced stage of the disease." I have in distinct recollection every fact contained in the observations to which "A Student" has referred, and can assure him, that nothing was advanced in those remarks without due investigation. Before I ventured upon the assertion to which "A Student" alludes, I had selected, at a considerable outlay of time, a great part of those cases which had been recorded by ancient and modern writers as instances of fungus hæmatodes (not, however, generally so named) of the eye, successfully cured by the extirpation of the diseased organ. The paper upon which those references were written has been mislaid, and I have therefore searched a second time, and can inform your inquirer, that if he will examine for himself he will be able to add very largely to the following list*. I do not positively assert, that all the authorities to which I have referred, accurately describe in every instance the true medullary structure, or that the cures they represent to have followed their operations, continued to the end of their patients' lives; but, I affirm, that in the majority of the cases to which I have directed the attention of "A Student," the disease will be considered to be fungus hæmatodes of the eye, and the cure to have been sufficiently complete and permanent, to justify the expression I employed in your journal nearly two years ago. However, I must beg your readers to peruse the whole sentence, of which "A Student" has republished only a part, and I think they will find that my language particularly referred to the attainment of a degree of benefit from the extirpation of the eye, adequate to repay the patient for submitting to the pain and unpleasantness connected with the performance of the operation. The following statement, by Mr. Rodman, of Paisley, who extirpated the eye, on account of a disease of this description, fully illustrates the chief intention of the remark, which has excited "A Student's" astonishment. "Diarrhœa ensued, and she died in the eleventh week after the operation. Being sensible of approaching dissolution, the day before death she remarked, with a considerable degree of gratitude, that *she suffered more pain in one day, before the operation, than she had ever done since**."

As my attention is recalled to the subject of fungus hæmatodes of the eye, permit me to allude to the operation I formerly recommended for its cure†. I stated that the morbid growth should be removed at a very early period of its existence, lest a disease, local at its commencement, should become constitutional in its progress, and, as the usual operation for its removal was an exceedingly severe one, and particularly when it is considered that the diagnosis of fungus hæmatodes of the eye, at its very early stage, is by no means easily accomplished, I proposed that when the disease was first distinctly recognized, an opening should be made in the cornea, the contents of the eyeball evacuated, the fungoid growth detached from the retina, and, if any portion remained which could not be easily removed, that its structure should be well-broken down with an appropriate instrument. However, I had not long promulgated my opinions before I was told of numerous objections to my proposal:—it was impossible; it would occasion dangerous hæmorrhage, and produce suppuration of the eyeball; and was, in short, open to many objections of *equal im-*

* Edin. Med. and Surg. Jour. vols. 15, 16, 19. Mémoire sur plusieurs Maladies du Globe de l'Œil; où l'on examine particulièrement les cas qui exigent l'extirpation de cet organe, et la Méthode d'y procéder: en Mémoires de l'Académie Royale de Chirurgie, tome 5. Institvtiones Chirvrgicæ, Lavrentii Heisteri, p. ii. sect. 2. Remarks on Surgery, by B. Gooch, vol. 2. Observations on Fungus Hæmatodes, by James Wardrop, pp. 46 and 82. Lancet, vol. for 1829, pp. 200 and 729.

* Lond. Med. and Phys. Journ. vol. 11.
† Lond. Med. Gaz. vol. 6.

portance. I think it will not be deemed necessary to occupy your time in refuting these objections, which were surely made without due reflection, and without inquiring into the mode in which this surgical measure would operate in producing a cure of the morbid growth. The only rational objection to this mode of procedure, arises from an apprehension that it is scarcely so radical a means of cure as the complete extirpation of the globe; for, if it is found to answer the same purpose as the entire removal of the eyeball, it comprises every advantage which a safe and trifling operation can possess over one that is extensive and painful, and even disfiguring and dangerous. If the whole of the eyeball be removed, an artificial eye cannot be worn; but, of course, if the muscles and the collapsed sclerotic coat remain, a contrivance of this kind may be conveniently employed, to diminish the personal defect, which would otherwise be extremely great.

Now, in order to place the subject fairly before your readers, I will explain my views more in detail than I have hitherto done. I shall assume that the fungus is perceived at an early stage of its existence; that its characters are decided; and that it is consequently too minute to occasion any material hæmorrhage, when detached from its connexions or broken down in its texture. If the greater part of it be removed and the remainder comminuted, its vitality will be destroyed, suppuration of the eyeball will take place, and the product of such suppurative action will be discharged through the opening in the cornea. It is on the destruction of the vitality of that portion of the fungus which is not discharged by the aperture in the cornea, and on the inflammation excited by the operation, by means of which the natural character of the inner membranes is destroyed, and the eyeball eventually collapses, that I depend for the cure of the disease. If the disease be limited to the retina, you destroy by these means the part from which it arose, and the cavity containing it; and, as I imagine, the chances of success are as great as they can be rendered by the performance of any operation for the cure or relief of this frightful malady. It was from the perusal of cases, where sloughing either occurred spontaneously, or was caused by various applications, that I was first induced to propose this method of treating fungus hæmatodes of the eye at its early stages*; and my opinion with regard to this practice, which was first expressed nearly two years since, remains unchanged.

There are three malignant diseases of the eye, sufficiently frequent of occurrence, and uniform in their characters and history, to merit particular description; namely, scirrhus, fungus hæmatodes, and melanosis, but I do not think we are at present acquainted with any other form of malignant disease of the eyeball which is entitled to a separate name. There are, it is true, what are called anomalous affections, but they occur very rarely, and defy classification; and, indeed, many of these presumed instances of malignant disease of the eyeball, which are stated not to belong to either of the preceding maladies, are nothing more than disease of its appendages, or morbid secretions from the hyaloid membrane of the vitreous humour. It is said that the eyeball is subject to genuine carcinoma, (I do not mean simple scirrhous induration, but refer to true cancerous disease); such is the almost universal admission of the older writers on ophthalmic maladies, but I have not seen any disease at all corresponding to correctly defined cancer of the eye, although I have witnessed the scirrhous change in that organ in more than six instances during the last seven years.

Nothing can be more perplexing and contradictory than the statements of authors respecting the malignant affections of the eye; indeed it would almost appear that they had entirely forsaken all pathological accuracy when treating of these maladies, and we have not, in any language, a good or complete dissertation upon this class of morbid derangements. In the year 1806, Laennec first described melanosis† (which disease Dupuytren had previously mentioned in his lectures), and yet Mr. Wardrop, who wrote on fungus hæmatodes three years afterwards, makes no distinction between these two widely different maladies‡; and Mr.

* See Wardrop on Fungus Hæmatodes, Cases 9 and 10; Hecker's Annalen. Sess. 1829; and M. Louis's Observations in the 5th volume of the Memoirs of the Royal Academy of Surgery, to which I have previously referred.

† Bulletins de la Societé de l'Ecole de Medecine.

‡ Wardrop on Fungus Hæmatodes, p. 17.

Travers, a new edition of whose Synopsis of the Diseases of the Eye was published in 1824, actually attributed the black discolouration produced by the existence of melanosis oculi to a profuse secretion of the choroid pigment * ; and Mr. Lawrence represents melanosis of the eye as being the second stage of fungus hæmatodes. He says, when lecturing upon this subject, " On this other preparation, which I have pointed out to you before, you see the progress of *conversion* from the *fungoid* to the *melanoid* state, in an eye where the disease had existed longer, and where all traces of the natural structure of the organ is lost †.

In directing attention to the pathological errors of former authors, who have published on the malignant diseases of the eye, and the consequent diminished value of this portion of their writings, I may refer your readers to the following remark of Bichat, the Editor of the Surgical Works of Desault :—" Le carcinome de l'œil attaque tous les sexes, se manifeste à tous les âges ; cependant il semble, plus que les autres tumeurs de cette nature, s'attacher à l'enfance. L'observation l'a démontré à l'Hôtel-Dieu, où plus du tiers des malades qu'y a operes Desault, étoient au-dessous de douze ans."‡ After having made this unfortunate statement, he proceeds to give a description " de l'œil carcinomateux," which, as might be expected, cannot properly be applied to any form of disease to which that organ is liable ; and is, so far, a valueless series of observations. The late Mr. Freer, of Birmingham, related a case of fungus hæmatodes of the eye, and accompanied his description with an engraving§, which, so far from exhibiting the characters of the morbid growth he presumed it to represent, displays a good example of the most usual form of orbital aneurism, for the cure of which disease Messrs. Travers and Dalrymple have very successfully tied the common carotid artery||.

An acquaintance with the foregoing facts leaves me at liberty to discover, with little regret, that one only of my statements respecting fungus hæmatodes of the eye, is contradictory to what " A Student may have been taught in lectures," or " have read in books."

Having thus pointed out the discrepancy of opinion which exists among writers of deserved celebrity, both ancient and modern, on the subject of scirrhus, fungus hæmatodes, and melanosis oculi, I will only remark, that if not anticipated in my intention, I shall introduce these subjects to the notice of the profession in a more complete and extended form, as early as my engagements will permit.

* Synopsis of the Diseases of the Eye, p. 431.

† London Medical Gazette, vol. vi. For some of the best observations hitherto published on the subject of melanosis, I beg to refer to Cooper's edition of Good's " Study of Medicine," vol. iii. ; to the first volume of Andral's Pathological Anatomy, and to the 90th No. of the Edinburgh Medical and Surgical Journal.

‡ Œuvres Chirurgicales, de Desault, par X. Bichat. Tom. 2.

§ Observations on Aneurism, p. 32.

|| Medico-Chirurgical Transactions, vols. ii. and vi.

REMARKS

ON

PROFESSOR BURNS' ACCOUNT OF SOME PREPARATIONS IN THE HUNTERIAN MUSEUM,

Which illustrate the Structure of the Human Placenta.

By Robert Lee, M.D. F.R.S &c.
Physician to the British Lying-in Hospital.

In my paper lately published in the Philosophical Transactions, I have stated that the human placenta does not consist of two parts, maternal and fœtal, that no cells exist in its substance, and that there is no connexion between the uterus and placenta by large arteries and veins. These conclusions are founded on the following facts and statements.

1st, If a gravid uterus be examined, in which the natural connexion between the placenta and the inner surface of the organ has not been disturbed by the forcible injection of wax or other extraneous matters into the uterine vessels, no blood-vessel of a large size can be detected passing from the uterus through the decidua into cells in the placenta. Numerous small blood-vessels are observed proceeding to the decidua, but they are not peculiar to the placenta, as they are found to exist universally throughout the whole extent of the membrane, even where there is no placenta.

2dly, If air be forced into the uterine vessels, while the placenta adheres to

the uterus, it raises the inner membrane of the uterus, but the air does not pass through the decidua into any part of the placenta, which it must readily do if a free communication by great vessels actually existed between these organs. Where the placenta adheres to the uterus there are numerous large semilunar openings in the inner membrane, which in the natural state are closed by the apposition of the decidua; but these have smooth edges, and present nothing of that ragged irregular appearance which they would exhibit if they were the extremities of torn vessels.

3dly, The uterine surface of the placenta is invariably covered with deciduous membrane, and no openings in it corresponding with the large openings in the inner membrane of the uterus can be perceived, nor any appearance of a vascular tube continued through this membrane into cells in the placenta. In the substance of the placenta no appearance of a cellular structure can be observed, and at no period of gestation can this organ be separated into maternal and fœtal portions.

4thly, In the Dissections of Rodera and Monro, the appearances they describe in the placenta were evidently produced by extravasated wax, and the same was found to be the case with the injected preparation of Mr. Hunter, in the Museum of the Royal College of Surgeons in London, and which, before it was properly examined, had been considered as demonstrating the existence of a communication by great vessels between the uterus and cells in the placenta. In this preparation, which was taken out of the spirits and carefully examined by Mr. Clift, Mr. Owen, and myself, flattened portions of injection were found between the uterus and decidua in some parts, and in others the deciduous membrane had been lacerated, and deposits of injection formed in the vascular part of the placenta. There was no appearance of a blood-vessel of any magnitude discovered passing between the inner surface of the uterus and placenta. In the course of last autumn, the preparations of the gravid uterus in the Hunterian Museum at Glasgow, were examined at my request by Dr. Nimmo; and in none of them did it appear certain that any great blood-vessels passed from the uterus into cells in the placenta; but in many the deposits of injection, causing the appearance of cells, were observed evidently to be the result of extravasation. No preparation in the collection seems to have been expressly made for the purpose of proving or disproving the fact that the deciduous membrane passes over the uterine surface of the placenta; but in reference to preparation R.R. No. 139, it is observed by Dr. Nimmo that no vascular openings are visible in the membrane interposed between the uterus and placenta.

No. 178 "is a small section of the uterus, with the veins injected green, and broken off where they were entering the placenta." The surface of the injected matter is smooth; the edges of the openings defined, and quite unlike ruptured vessels; their form in general elliptical, seeming as if they were holes cut in the side of a convolution.

No. 125. "A portion of uterus and placenta, the latter injected from uterine vessels." There is an opening which seems to be natural, corresponding to one of those in the uterus; but the majority of those whereby the injection has passed into the placenta, seem to be mere lacerations.

No. 101. "A section of uterus, with veins injected black, and the injected matter protruding by irregular plugs into the cavity of the uterus." The holes are semilunar and elliptical, with defined edges, and nothing resembling the continuation of vascular tubes to be seen.

R. R. 121, is described in the printed catalogue as follows: "A small portion of placenta and uterus, where the cells of the placenta have been injected from the veins of the uterus. The veins are seen very large, entering the substance of the placenta."

Dr. Nimmo makes the following observations on this specimen: "This preparation seems to be most in point. I would describe it differently. The cellular substance of the placenta has certainly been filled from the uterine vessels. These, however, instead of passing directly into the placenta, are distinctly seen applying their open mouths to the membrane of the placenta, where the injection in some instances stops. The membrane is thinner here than where no vessels are applied, consisting, so to describe it, of one layer, while a second layer covers all other parts. Where the injection is passed into the substance of the placenta, it has evi-

dently been forced to the side between the layers, and found some weak point, whereby it has entered into and been diffused throughout the cellular texture of the placenta *."

An account of the preparations in the Hunterian Museum at Glasgow, which illustrate the structure of the placenta, has since been published by Professor Burns †, from which it might be inferred that the preceding description is incorrect, and that the truth of the Hunterian doctrine has been called in question on insufficient grounds. Although he must have been fully aware that the appearance of cells in the placenta, and of vessels communicating with the uterus, had been proved to be fallacious by an examination of Mr. Hunter's preparation in the Museum of the Royal College of Surgeons here, yet Professor Burns, before publishing his description of Dr. Hunter's preparations, has not considered it requisite to subject them to the same test, but has drawn his conclusions from appearances which might also be the consequence of laceration of the deciduous membrane, and the formation of deposits of injection in the vascular structure of the placenta. Until these preparations are removed from the bottles in which they have been suspended for upwards of half a century, and carefully examined by competent witnesses, their actual condition will continue to be a constant subject of doubt and dispute.

From the following brief remarks on Professor B.'s description of the more important of these preparations, I think it will follow, not only that Dr. Nimmo's statements are correct, but that I was justified in asserting, that "in none of them does it appear certain that any great blood-vessels pass from the uterus into cells in the placenta; but in many the deposits of injection, causing the appearance of cells, were observed evidently to be the result of extravasation."

The following is Professor Burns' description of No. 21, which, he observes, "is a very important preparation. The uterus, at the sixth month, is injected and cut open, but the placenta is every where left adhering. *No vessels, therefore, can be seen passing from the uterus into it. But that the red injection has entered freely, and filled the cells, is proved by the colour being finely visible on the fœtal surface of the placenta.*"

It is here expressly stated that no vessel is seen passing from the uterus to the placenta; and it is assumed by Professor B. that the injection has flowed into cells, the existence of which he has not demonstrated. Indeed the preparation illustrates neither the structure of the placenta nor the nature of its connexion with the uterus; and the same may be said of No. 137, which merely "shews a decidua very distinctly injected from the vessels of the uterus."

No. 124 "is decribed as a small portion of the placenta and uterus, where the cells of the placenta have been filled from the vessels of the uterus. The fœtal portion is not injected. The placenta is detached from the uterus, and hangs down. The cells are filled with red, and among them we see cut portions of green. *No injected vessel, indeed, is seen passing into the uterine surface of the placenta*, but there are *several bristles put into unfilled orifices on that surface.*" In this description it is again distinctly acknowledged that no vessel is seen passing into the uterine surface of the placenta, but, because orifices filled with bristles are seen on that surface, it is at once assumed, without any proof being presented, that there are corresponding orifices in the uterus. This is stated more clearly in the description of Preparation No. 92, which "is a section of a uterus which was ruptured in the cervix; the side of the placenta had been attached over the os uteri; the rest of it adhered higher, and part of that is raised up so as distinctly to shew orifices on the surfaces of the uterus and placenta, corresponding to each other."

If these were natural orifices leading from the uterus into cells in the placenta, it is difficult to explain why they were not filled with injection in Preparation 124. But the fact of their being found empty, renders it probable that these openings were the mere effects of laceration of the decidua; for, as I have ascertained in my dissections, if the placenta is not separated carefully from the uterus, the decidua covering the openings of the uterine sinuses is torn,

* My friend Samuel Broughton, Esq. F.R.S., during a recent visit to the Hunterian Museum at Glasgow, examined the preparations of the placenta and uterus at my request, and authorizes me to say that his observations fully confirm the accuracy of Dr. Nimmo's statements.

† Medical Gazette, July 21, 1832.

and artificial openings are formed in it. The decidua, in apposition with these openings, as Dr. Nimmo and Mr. Owen have both described, is not only thinner than in other parts, but adheres more firmly, and is more easily lacerated.

No. 167 is a "Section of the Placenta with its cells filled with black and red. The decidua covers the uterine surface of the placenta. In some places *the wax is irregular, and might*, therefore, be considered as having been extravasated; but in other parts, more especially toward one side, the entrance or passage of vessels through the decidua is very distinctly seen." This preparation certainly does not appear to confirm in a very satisfactory manner the Hunterian views, for on one side only was the entrance of vessels into the placenta seen, the remainder of the wax being irregular in appearance, which could only be produced by extravasation. The existence of black and red wax mixed together in the placenta, is a decided proof that the wax had not flowed into cells. The preparations (marked No. 139—101, 178—and 121) described by Dr. Nimmo, have not been alluded to by Professor Burns.

I do not consider it necessary to offer any remarks on the other preparations described by Professor Burns, as they throw no light on the subject in question; and, indeed, it will appear from the following observations of Professor B. that he himself is not satisfied with his own account of the connexion between the uterus and placenta:—"I only premise," he says, "that I believe the communicating vessels to vary, both in size and firmness, at different periods of gestation. It is also evident, that a *prolongation of the uterine vessels into the placenta, cannot be accomplished by a continuation of the ordinary texture of the vessel, but by the interposition or intermedium* of a circle or portion no firmer in its fibre than the *decidua*, otherwise the secundines never could be thrown off." As I can form no conception of what this "circle or portion" interposed between the uterus and decidua may be, seeing it is neither blood-vessel nor membrane, I can only express my earnest hope, for the sake of physiology, that Professor Burns will speedily favour the world with an account of the preparations by which it is demonstrated.

IMPROVED MODE OF PERCUSSING THE THORAX.

By David Badham, M.B.
And Radcliffe Travelling Fellow, Oxford.

After a very little practice, several objections to the present mode of percussing the thorax will occur to the physician, performed after the usual manner, and, as must often be the case, not very adroitly. The patient is often found to complain of the force employed, though it has been inconsiderable; while the practitioner, not always able to guide himself by the sensations of his patient, and arrive at that firm yet light management of the hand (neither so great as to prohibit repetition, nor so slight as to make the result ambiguous) which is absolutely necessary, will frequently remain embarrassed and undecided. Another objection to simple, or *immediate* percussion, especially to persons little practised in the art, is that the superficial sound made by the fingers on the parietes of the chest, interferes with the perception of the deeper sound, which it is our object to elicit. The sound of the percussed surface being much more perceptible than the hollow resonance from beneath, obscures that evidence which the ear should receive, by rendering it of a *mixed character*. Hence we find percussion, in ordinary hands, an incomplete and uncertain means of diagnosis. Nor is the flat piece of ivory recommended by a French writer, as a medium to receive the ictus of the fingers, without objection. Ivory and other hard and compact bodies, yield a sound of their own upon being stricken; and though we are instructed to make allowance for this superficial exterior sound, it will be found an abstraction far more easy in theory than in practice. But the suggestion is a good one, and after many trials of different intermediate substances, I am so fully convinced of the superiority of that which I am about to recommend, that I can no longer hesitate to offer it to those gentlemen who find the ordinary mode of percussion defective, or to whom it is unfamiliar. If simple percussion be liable to the objections stated, there are but two modes of proceeding by which we may obviate them. The first is, to strike the chest—*not* with the fingers (for every one has not

the fingers or the touch of a Paganini)—but with some instrument or plectrum; a ball of soft leather, stuffed with some soft yet not unresisting material, such as horse-hair, and having a cane or whalebone handle, is not a bad one; or an implement always at hand, namely, a common hair-brush, or any light brush *with a handle*, having the bristles of moderate firmness, though it may appear a rough and awkward, is in reality, as I have shewn to many persons, by no means a despicable instrument for the purpose intended; in fact, such a brush made on purpose, about the size of a dollar, may be used with real advantage, where you wish to explore by percussion the seat of crude tubercle, or any other solid obstruction of small extent; and I believe that, by its employment (the ear being previously accustomed to this mode of interrogation), we may form a very near conjecture as to the seat of disease. Into the question of therapeutic utility I am not called upon to enter. The careful pathologist is glad to arrive at all possible evidence of the state of parts. Or, if the cui bono querist must be answered, is it not of importance to be able to assure a patient in a suspected state, that his lungs are every where accessible?

The second contrivance is, to procure from the cork-cutter a piece of well-smoothed cork, about as large as a small duodecimo, of moderate thickness (perhaps an inch), and slightly hollowed out on one side to accommodate it to the convexity of the chest. On this substance the ictus may be made, even with a hard body, as with the points of the fingers compressed together on the same plane, or even with the knuckles, with the most satisfactory and obvious varieties of sound as you travel over the large region of the chest. The brush should be neatly sewed into a case of soft leather; but the plate of cork is the medium that I now adopt; and if it be once fairly tried, and made to receive either the ictus of the fingers or the knuckles, or that of a small plectrum six inches long, made of whalebone, with an extremity about as big as a boy's marble, made of ivory or ebony, the practitioner will probably avail himself of it in future.—I have the honour to remain, &c. &c.

David Badham, M.B.

VACCINATION.—COMPARATIVE THICKNESS OF THE SKIN IN CHILDREN.

To the Editor of the London Medical Gazette.

Sir,

Have the kindness to insert the following in your valuable publication.

Yours, &c.
William Howison, M.D.

9, Nicolson-Square,
July 19, 1832.

The only remark I have to make at present, regarding vaccination, since the publication of the preceding ones, already detailed in the former numbers of the Medical Gazette, is with reference to the *thinness* and *thickness* of the skin of the arms of the children vaccinated, and the consequent care to be taken by the operator in inserting the virus. During the numerous vaccinations carried on by me for years past in the public vaccinating establishment of the capital of Scotland, in making the incisions into the arms of the children, for the purpose of introducing the vaccine virus, with the round pointed vaccinating lancet, which I invariably make use of (as formerly mentioned,) I have at times found the skin as thin as a piece of gauze paper, or gold-beater's skin or leaf; and turgid with circulating blood, which I must remark is rare, but it occasionally presents itself; and drops of blood following each other in succession, wash out the inserted virus, rendering the future inoculation ineffectual. In other infants, on the contrary, of the same age, and under the same circumstances, I have found the skin of the arm *thick*, like a piece of parchment; and not the slightest appearance of blood to be observed, until the lancet, by repeated scratches, had penetrated to a considerable depth in introducing the virus. Betwixt those two extremes I have met with every gradation.

In consequence of possessing a knowledge of the above fact, I conclude that it is necessary for the vaccinator to be always on his guard, whilst inserting the virus, lest the skin should prove thin, as already mentioned,—a circumstance which no individual can foretel

before hand, and if so, blood will instantly follow the slightest scratch of the lancet, washing away the virus, rendering the future vaccination useless, running down the arm, proving discreditable to himself, and hurtful to the feelings of the mother. Should the skin prove *thick*, as already alluded to, I consider it as an advantage, allowing of the free and slow absorption of the vaccine lymph, and proving of no consequence in any other point of view.

PROTUBERANCE OF THE ABDOMEN IN SOME PAPUAN CHILDREN.

Some Papuan children at Erromanga, one of the new Hebrides group, had a great protuberance of the abdomen, and the chest had a contracted appearance as they stood in the erect position. At first, from its tense feel and resemblance to tympanitis, I was inclined to attribute it to disease; but when I found that it existed among the whole, and they appeared otherwise to be in good health and spirits, I gave up that opinion.

One of these children (a female) was brought to England, and is now residing in this country, since which the abdomen has diminished in bulk. On the 20th of October, 1830, I had the curiosity to take measurements before any diminution had taken place, and the following are the results:—

	Feet.	Inches.
Height	3	4
Length of the sternum	0	4½
Length from the ensiform cartilage of the sternum to the crest of the pubis	0	10½
Circumference of the abdomen	1	10½
Breadth of the thorax	0	4½
Length from the anterior superior spinous process of the ilium to the sole of the foot	1	11½

Mr. G. Bennett's MS. Journal.

COUNTER-IRRITATION AMONG THE NATIVES OF MANILLA (Island of Luçonia.)

A Commander of a ship having a severe head-ache, placed himself under the medical charge of a native female, who employed a method of counter-irritation, by pinching the side of his neck, until it became in a bruised state; feeling also an oppression of breathing, from a cold, his side underwent a similar operation, from which, in both instances, he considers he had received much benefit. This remedy seems to be of Chinese origin; as Mr. Pearson mentions in the Medical and Physical Transactions of Calcutta, that, "instead of our vesicatories, the Chinese resort to means of producing counter-irritation, by drawing out and pinching with the fingers and thumb the skin and cellular substance, until the surface is completely blackened."—*Mr. G. Bennett's MS. Journal, August* 20, 1830.

THE PORPHYRA NAUTICA, OR SEA SCURVY.

Wherever this disease is found to prevail, we may readily attribute it to the bad quality of the provisions or water, or the foul condition of a crowded ship, as among the whalers, in which class of ships the disease is now almost exclusively found to prevail, the water in them being of bad quality, from being put into old oil casks; the continual dirt in which the employment of the crew necessarily places them; an unhealthy effluvia and deficiency of good water, are a sufficient combination of causes to produce the disease. That the purity of water tends to preserve the health of seamen was the opinion of our celebrated circumnavigator Capt. Cook, and is thus alluded to by Dr. Mason Good, who observes, "Pure fresh water is also another point of great importance, not only in curing this disease (sea scurvy), but in guarding against it; and of so much moment did Capt. Cook esteem its purity, as well as its freshness, that he had the old stock poured away, though procured only a few days before, whenever he had an opportunity of obtaining a new supply *." There is a custom prevalent among the whale ships of burying the patient in the earth for a certain length of time, as a remedy for this disease. On the ship arriving in port, among the islands in the Southern Pacific, the patients are landed, and buried up to the waist, and they consider this process as the only cure; some of the patients have only the legs buried, others are buried up to the waist, de-

* Good's Study of Medicine, vol. iii. p. 463.

pending on the extent of the disease. It is deemed requisite that some one should remain with the patient during the time he is thus partially interred, otherwise the pain produced would cause him, if left to himself, to give up the trial, before a sufficient length of time had elapsed; the interment is repeated several times, and eventually with complete success; the length of time they are placed in the "earth bath" is usually from fifteen to twenty minutes. As the patients, however, who are undergoing this mode of treatment are living on shore on fresh provisions and abundance of vegetables, and taking occasional exercise, a doubt may naturally arise whether the cure does not depend more on pure air and wholesome diet than on the earth-bath. The men employed in the whale ships have themselves attributed the disease to the indifferent quality of the water and provision, and the "mess of oil and stuff they are rolling in for days together;" and when they cut themselves, which occurs not unfrequently from the sharp tools that are lying about, necessary in their employment of cutting in the whales, the wound soon becomes black, heals slowly, and with difficulty.

There are numerous ships which now take long voyages, and, being amply provided with wholesome water and provisions, have not a single case of scurvy; indeed it is now a disease very rarely met with excepting among the South Sea whalers.

George Bennett,
M. R. C. S., &c.

London, 1832.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

Observations on Surgery and Pathology, illustrated by Cases, and by the Treatment of some of the most important Surgical Affections. By William James Clement, Surgeon, Shrewsbury.

The work which we have to introduce under the above title will, we are confident, gratify our best informed practitioners in surgery by its perusal. It is written on a plan which ensures a continual interest to the subjects which are discussed, and makes the contributions even of those who possess only moderate opportunities of seeing disease, of the highest value. This is Mr. Clement's first appearance as an author, and he has wisely selected his method of addressing the profession: indeed we would call upon others, who have ambition to give instruction to their brethren, to follow the same course which he has chosen, and to qualify themselves as he has done.

The work consists of several dissertations upon detached subjects, which are treated without the pretension of starting new and extended doctrines, but on the plan more humble, though no less useful, of detailing the common and varied occurrences met with in his own practice, and commenting upon them and the general opinions held by the profession upon such subjects. Now this is a system which, to be successfully followed by a medical author, demands no common ability, and requires above all things a sound and perfect education. To relate a case, however frequent the complaint may be, might be made the most exact criterion of the qualifications, or professional accomplishments, of a physician or surgeon. It puts to the test his habits of observation; and it is his extent of reading, and acquaintance with the opinions of varied authors, that best opens the mind to notice minute circumstances, or, in short, to *observe*. This is an acknowledged truth; yet among the multiplicity of cases presented in our cotemporary journals, and in books, how seldom do we find the progress of a disease recorded with intelligence. Either facts are omitted upon which the views of respectable writers hinge, or diffuse statements are made which have no necessary bearing upon the case, but are to be referred to some accidental complication of the disorder; or a colouring is given to all the details by some weak theory entertained by the narrator himfelf, founded upon ill-assorted gleanings of information, picked up without study, and applied without knowledge. The man who does not read—who does not refer beforehand to the most approved works which treat of the disease he has to examine, will omit, in his history of the complaint, numerous symptoms that would otherwise have openly declared themselves, and given, perhaps, a new turn to his conceptions

of the disease. This we think it right to insist upon, in opposition to some eminent lecturers (themselves authors!) whom we have listened to as they professed to their pupils, that it was at the bed-sides of patients *alone* that knowledge could be obtained; and that the speculations of such as took up the pen were to be disregarded, or even dreaded. We maintain that a surgeon may visit in his carriage forty patients a day, or go his rounds in an extensive hospital, and actually see less—that is, not with his external organs, but with his "mind's eye"—and carry away less real and accurate information, than the humbler practitioner, who, deeply versed in the literature of his profession, is called to give his aid to the tenth part of that number. It is for this reason that we feel very little influenced by the parade of numerous cases so commonly brought forward by authors, with the view of forcing conviction by an apparent accumulation of facts. We look rather to the amount of judgment which animates what the writer delivers, and gives token of his acquaintance with his subject. We repeat, that with a perfect education, it is open to every practitioner, however limited his means of visiting the sick may be, provided he is not altogether destitute of these, to contribute much valuable matter to the profession.

The work which is before us evinces, by its style and composition, that the author has had his mind well tutored for the task which he has undertaken, of recording cases, and making critical remarks upon their results. In the narration of these, we have to admire the fulness with which the symptoms are detailed, and yet the perfect freedom that there is from what may be termed, an excess of minute description—a common fault, which renders the business of reading published cases, and of listening to those that are poured into the ear by friendly practitioners so often, tiresome and unprofitable.

The subject which occupies the largest space in the volume, and carries with it the greatest interest, is that of *Hernia*. There are nine cases selected from the author's note-book, in all of which several distinct questions of importance are presented for discussion; and these are treated separately at the termination of the cases. We should have been glad to have copied into our pages some of the practical deductions, with the arguments that support them, as a means of giving our readers a fairer view of the manner in which the whole is conducted; but our limits constrain us, as we have often cause to regret in other instances, to speak more in general terms than we could desire. We would not, however, lose the opportunity of directing attention to one or two of the points which come under consideration.

The author is fully impressed with the imperative necessity that exists in cases of strangulated hernia, of releasing the intestine which is included in the sac, with as little delay as possible. We would recommend for perusal, in particular, his observations upon the great hazard that is often incurred by deferring the operation with the knife, in favour of several means which, to say the least, are extremely uncertain in their results, but yet have been highly lauded by different authors. It appears to us, when we consider the condition of the protruded portion of the intestine, and that of the bowels contained within the abdomen, that the danger is much more serious from their being allowed to remain, even for a short time, under such circumstances, than to make an incision for the relief of the gut. Although no one can doubt for a moment that a wound of the peritoneum has a risk attending it of causing inflammation, which may spread over the surface of the abdomen, yet it must, at the same time, be allowed that the pinching of the extruded portion of intestine, the injury that it is subjected to at the neck of the sac, which often cuts it like a cord, and the obstruction to the passage of the contents of the alimentary canal from this violent cause, with the consequent tormina, must have a still greater influence in creating inflammatory action in the bowels. Again, every moment that the excluded portion of intestine is allowed to remain within the sac, it is becoming less and less capable of recurring to its natural action of propelling forward the alimentary matters, when it is reduced: its coats are becoming thickened, gorged with serum or with ecchymosis, or they are converted into a softened state, approaching to mortification: in such a condition, the intestinal canal, even if it have escaped extensive inflammation, will refuse to pass the fæces through this part; and

the presence of such an injured portion of gut within the peritoneal cavity, will do more to excite inflammation than the incision could have effected. The only consideration that can put a check to our desire to operate quickly, is the common question, how long may the intestines be allowed to struggle against the obstruction? and how long may the knuckle embraced in the hernial sac be permitted to undergo the unnatural pressure to which it is subject? A division has been attempted to be made betwixt what are called the acute and chronic, and, by Richter, the spasmodic, kinds of strangulated hernia. But against all such specifications of the cases of hernia we earnestly protest. In our experience we know not what is meant by a "*spasmodic strangulated hernia;*" and as to the acute and chronic kinds, it has only a tendency to encourage dangerous procrastination to represent a hernia which is strangulated as capable of becoming chronic. It may be safe enough to form such a classification retrospectively, that is, with reference to cases after they have occurred; but in actual practice it is our duty, we maintain, to treat every case when we are brought to it, whether in old persons or in young, in a plethoric subject or in one who is emaciated, as if it were running with the most rapid course to a fatal termination. In regard to the question formerly put, how long may a hernia remain in a state of strangulation before it is relieved, and yet give a chance to the patient to survive? no one can answer it. The injury which shall cause death, may be inflicted in four hours: we have instances of the patient's dying in twelve hours from the time that the rupture descended; but, happily, though at the same time it is beyond the ken of the most experienced surgeon to observe the difference in the symptoms, the period during which hope may be entertained of a favourable issue is commonly of much longer duration. The rule of practice seems to be,—give, first, the taxis a fair trial; but make it a principal aim while pursuing it, to discover quickly whether the gut is in a condition in which it is probable that it can be reduced by that operation: if, with the addition of bleeding and the warm bath, the hernial tumor does not become distinctly softer or diminished in size, then wait not for more urgent symptoms; disregard all those proposals which have been made of *trying* what will be the effect of the tobacco-clyster, whether a bladder of pounded ice will do good, whether a weight left upon the tumor will reduce it, whether a purgative administered may possibly extricate the incarcerated gut, whether an opiate will relieve the "spasm," &c.;—have recourse at once to the knife. We must choose one evil as a substitute for another that is much greater; we must incur the hazard consequent upon an incision of the sac, in exchange for the constriction, bruising, tormina, and commencing disorganization of the intestines.

Before leaving the subject of hernia, we must request our readers to refer to the judicious observations which Mr. Clement makes on the mode of performing the operation for umbilical hernia, when strangulated. They will also read with much interest a remarkable case of cystocele, in which the whole of the bladder, with the exception of a small part at the neck, had escaped from the pelvis, and was lodged in the scrotum.

Besides the dissertation upon hernia, the work contains an essay upon the structure of the urethra, where it is attempted to explain the origin of strictures in that canal, in opposition to the views, principally of Sir Everard Home. This author, it is well known, proceeding upon the assumption that the membrane of the urethra was of muscular structure, conceived that a stricture arose from a spasmodic contraction of certain fibres in the passage. At first he imagined that these fibres were circular in their direction, and that a stricture was formed by their closing, after the method of a sphincter muscle; but afterwards, having more narrowly inspected the course of the fibres by the aid of Mr. Bauer's microscope, he discovered that they traversed the canal in a longitudinal course. Accordingly, he substituted for his former explanation, which he had held for sixteen years, the following, which we venture to think is a most improbable one—that the muscular fibres, contracting in their whole length, act with greater force at one part than at all the others, and thus give rise to stricture. We confess, that even if it were allowed that muscular fibres existed in the canal of the urethra, this shifting of opinion in regard to the

mode of their action would lessen our faith in the soundness of the theory; and we should ask the eminent author for some further proofs that muscles could act as he has described in one part of their course more forcibly than in others, and so as to remain permanently contracted in this single part, while the rest of the tube, of the same muscular texture, relaxed passively, or even became preternaturally dilated posterior to the stricture. When spasms occur in the course of the intestinal canal, from irritation, we have every reason to believe that the contractions vary their situations. At all events, we know that they are removed effectually by the slightest mechanical force after death, and disappear almost instantaneously by the portion of gut being placed in water; but this, it would appear, is contrary to what takes place in strictures of the urethra, where it is maintained that a "spasmodic" stricture can be preserved after death, and be exhibited in preparations, the muscles continuing contracted even after soaking and maceration. Mr. Clement has presented many weighty arguments against the supposition—that the canal of the urethra is muscular. There is one, however, which has been used by Sir Charles Bell in his latest publication upon Strictures, that tends more strongly than any other which the author has quoted, to confirm his views; and as it has been omitted by him, we may be allowed to extract it here. That gentleman, in a lecture formerly delivered before the College of Surgeons, while speaking of the dilatation which takes place in the portion of the canal posterior to the stricture, thus expresses himself. "You will perceive that whilst the bladder is singularly contracted and thickened in its walls, the whole extent of the canal, from the sphincter of the bladder to the back of the stricture, is enormously dilated. This, by the by, is another proof of the want of muscularity in the passage, and that the supposed muscular texture around the urethra has nothing in common with the *detensor urinæ*, or muscular coat of the bladder, in the discharge of urine; otherwise those fibres, if they existed, would have partaken of the excitement and development which has produced so singular a change on the bladder, and they would have resisted the dilatation." So far, indeed, is the supposed muscular texture of the urethra from becoming magnified in correspondence with the muscular coat of the bladder, that the part posterior to the stricture is peculiarly thin, and liable to be ruptured, giving rise to fistula in perineo.

The following observations, in reference to the treatment of strictures at their early stage, are, we think, of much value. "The fears of patients are frequently excited by the stream of urine appearing smaller than usual, after their recovery from the acute symptoms of gonorrhœa; and they actually may experience some difficulty in voiding it. This they of course attribute to the formation of a stricture; whereas it more frequently depends upon the want of elasticity in the canal, produced by the previous inflammation; and some degree of thickening may probably exist as the consequence of it, but which will gradually subside if the canal be not interfered with, or irritated by the passing of instruments."

"When obliged, for the satisfaction of my patient, to sound the urethra, in order to discover the situation of a supposed stricture, I have frequently observed, that after the bougie was passed three or four inches from the orifice, its point touching or pressing a particular spot, the patient has shrunk back and expressed a feeling of pain. By a gentle pressure, however, the bougie may be passed onwards, and it afterwards proceeds with facility towards the neck of the bladder, without producing any unusual pain. On withdrawing the instrument, and pressing with the finger upon the urethra externally, at the part where the bougie produced the pain when in the passage, I have always found that the patient complained of soreness, and a sensation different from what was caused by pressure upon any other part of the whole canal. This is what some surgeons would term an incipient spasmodic stricture, requiring the daily use of the bougie to overcome its tendency to permanent contraction; but I have considered it as a remnant of the gonorrhœal inflammation; that this particular part of the membrane of the urethra continues inflamed, is consequently irritable, and requires a plan of treatment directly contrary to that which might be proper in a case of spasmodic stricture, supposing such an affection

of the urethra to exist." . . . "The fact that certain portions of the membrane of the urethra continue in a state of chronic inflammation after the total suppression of a gonorrhœa, has hitherto been but little attended to, if not entirely overlooked: but, from repeated observation, and the numerous cases which have presented themselves to my notice, I am convinced that it is of very frequent occurrence; particularly in persons of a scrofulous constitution. The opinion is not founded merely on speculative grounds, for in three instances I have had an opportunity of dissecting the urethra, the patients having been carried off by acute diseases shortly after their recovering from attacks of gonorrhœa. In each of these cases the membrane of the urethra presented nothing unusual in appearance, excepting at one spot about three inches from the orifice, which looked much redder than any other part of the canal, felt firmer under the finger, and, upon a minute inspection, a congeries of small vessels was seen ramifying on the surface, evidently denoting an increase of vascularity. The examination of these cases first led me to suppose that this local inflammation, if not subdued, might most frequently be the cause of the formation of strictures in the canal; and acting upon this supposition, I have since directed such a plan of treatment as appeared to me most rational for the cure of all cases of incipient strictures, where any tenderness on pressing the urethra was present. Instead of irritating the canal by the introduction of instruments, I have advised, in the manner already stated, the application of leeches, and afterwards friction with the mercurial liniment."

We have left ourselves only space to enumerate shortly what are the remaining contents of the volume. There is a case of spina bifida, in which the operation of puncturing the tumor was repeatedly tried, but ultimately proved fatal. A case is next given of aneurism by anastomosis, in which the ligature of the vessels belonging to the tumor did not retard its growth, and there was a necessity for extirpating the whole substance. The next in succession is a case of lithotomy in a female child. This is followed by the description of a tumor of an uncommon nature, which was removed from the biceps flexor cubiti in a man. The last is an account of a formidable operation (which seems, however, to have been both skilfully and expertly performed), of extirpating an immense tumor, which was attached to the lower jaw, and extended nearly to the clavicle. The removal of this tumor was followed by paralysis of the side of the face; and the author takes the opportunity of adding several remarks on Sir Charles Bell's important discovery of the functions of the nerves, and their classification.

In conclusion, we have only to hope, for our own parts, that the author will be encouraged, by our mode of noticing his present work, to fulfil the intentions which he has expressed, of adding, at a future opportunity, further cases and observations to those contained in this volume. He may be assured, that cases well recorded form a constant and valuable treasure to the medical profession; and that there is still ample scope for a cultivated mind to extract new circumstances, and to give fresh interest to the daily occurring events of practice, however beaten and common the path thus selected may appear to be.

CUTANEOUS ERUPTION IN CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

In Russell and Barry's Official Reports on Cholera Spasmodica, there is an extract from a "Report on Cholera as it prevailed in the city of Moscow, and in other parts of Russia, in autumn 1832, and winter 1831, by Dr. Keir;" at the close of which we find the following paragraph. "The more rapid and violent attacks of the disease were sometimes terminated by convulsions; and in a few cases, where biliary and intestinal irritation had prevailed, a cutaneous eruption, resembling the nettle-rash or the measles, but with a larger spot, appeared on different parts of the body, and continued a few days. The patients under my care who were so affected recovered."

I am desirous, through the medium of your periodical, to direct the attention of the profession to this affection, of which I have now seen several instances.

This eruption I have found to make its appearance in protracted cases of cholera, where the disorder has assumed that typhoid character which is marked by extreme debility, drowsiness, not amounting to stupor, injected conjunctiva, and general indications of cerebral congestion.

After this state has existed several days, some red spots are observed about the wrists and hands, and the face becomes tumid, as on the approach of erysipelas. If this occur in the evening, on the following morning, the arms, the forehead, up to the roots of the hair, and the face generally, will be covered with large elevated patches, of a bright red colour, more vivid than measles, and more defined than scarlatina, much resembling nettle-rash, especially in the circumstance of their disappearance on pressure, and instant recurrence when that pressure is removed. They vary in hue at different times, being occasionally purplish, especially on the hands, and again resuming their bright tint without any apparent cause. On the second day the efflorescence is found over the whole trunk and limbs, and the sensation produced by it is not that of tingling and heat alone, but of intense itching. To allay this, the patient scratches himself incessantly until he has removed the cuticle in numerous places over his body and extremities, leaving patches of incrusted blood, and subsequent scabs, wherever this abrasion has taken place. The eruption is very well marked about the third and fourth days, its outline around the edge of each patch being then more distinct than before, and bearing a greater contrast to the central portion. It declines about the sixth day, and terminates by a general desquamation of the cuticle. In this respect, as well as in its definite duration, it is needless to remark that it differs altogether from nettle-rash. The constitutional disturbance is very great for a day or two before its occurrence, and continues to increase until it is at its acme. It is not attended by sore throat, enlarged glands, or acrid discharges from the eyes or nose. In one case which I witnessed, the powers of the patient, who had been previously much debilitated, were unable to sustain this new cause of irritation, and he sunk on the third day of the eruption. From a few cases it is not possible to draw up an accurate history of a new train of symptoms or appearances, which may vary in different instances; nor do I consider it as certain that this sequela of cholera is wholly dependent on the disease itself, since various remedies occasionally give rise to cutaneous affections. My belief, however, is, that the eruption in question does not arise out of the employment of remedial means. Three of the cases which I have seen, were under the immediate care of a gentleman whose treatment principally consisted in the exhibition of calomel, rhubarb, ginger, and salines. In other cases, opium and astringents had been freely used: the eruption was alike in all. I cannot, with Dr. Keir, consider it as connected *especially* with biliary and intestinal irritation, since I have seen no case of cholera which has terminated without such irritation. Perhaps this very imperfect notice may serve to call forth the testimony of others regarding this affection; which, whether corroborating or correcting what, from limited observation, I have been enabled to state, will not fail to enlarge our knowledge of a disease even yet so imperfectly understood.—I have the honour to be, sir,

Your very obedient servant,
B. G. Babington, M.D.

16, Aldermanbury, August 1, 1832.

CASES OF MALIGNANT CHOLERA,

Treated principally by Corrosive Sublimate and Acetate of Lead.

By F. J. Hart, Esq., Surgeon*.

Case I.—James Snell, aged 33, a leather-dresser, of rather a spare habit of body, had been labouring under diarrhœa for three or four days, but on the morning of Thursday, the 12th instant, was seized with violent cramps in the stomach and legs, and excessive vomiting and purging, which obliged him to apply for assistance.

At 12 o'clock I saw him. At this time there was no pulse to be felt at the wrist or ankles, the feet and hands were cold, contracted, and of a leaden hue; the eyes very much sunk and ghastly; the vomiting and purging still

* Communicated by the Central Board of Health.

continued, and the cramps in every part of the body very distressing; but the skin, except the extremities, was quite warm; the pulse in the upper part of the arms and legs, and the heart, was at 90, small and hard. Ordered mustard cataplasms to the feet, mercurial friction over the abdomen, to take brandy and water in small quantities, and a teaspoonful of the following mixture, in brandy and water, every quarter of an hour, until the sickness ceased, with one pill every half hour.

℞ Acid Nitrici, gtt. xxx.; Tinct. Opii, gtt. xl.; Aquæ Puræ, ℥iss. M. ft.
℞ Hydrarg. Oxymur. gr. iv.; Plumbi s. Acetas, gr. iv.; Pulv. Opii. gr. vj; Ol. Caryoph. q. s. M. ft. pil. xii.

At 6 P.M. very little change had taken place, except that the sickness had somewhat abated, and he complained of his head being light. At 10, the sickness had entirely ceased, the purging was become less frequent, the pulse could now be felt at the wrist, though very feeble, and the cramps were wholly confined to the body; the hands and feet were regaining their natural state; he complained of great thirst, and had passed no water.

To continue the pills every hour.

On the following day (Friday 15th), he was much better, had been in a violent perspiration all night, the purging had ceased, no return of sickness, the cramps much abated, the hands and feet perfectly recovered, and the pulse 120, soft and full; complains of his head, and great thirst. Ordered to drink lemonade; the abdomen to be bathed with hot water; to discontinue the cataplasms, and to take two tablespoonsful every three hours of the following mixture:—

℞ Quininæ Sulph. gr. vj.; Acid. Sulph. Dil. ʒj.; Tr. Card. cc. ʒiij.; Aquæ ad ℥vj.

In the evening no return of the violent symptoms; head much better; perspiration still continues; pulse 100, and soft.

On Saturday he was gaining ground very fast; and on Tuesday I met him, when he only complained of great weakness.

CASE II.—John Hubbard, aged 30, the captain of a vessel, of good health and sober habits, was seized on Tuesday, the 10th inst. with a violent purging and vomiting, which obliged him to go home. At six o'clock in the evening I saw him, when the vomiting and purging had increased; the pulse hard, small, and feeble; cramps in the legs and stomach; great prostration; still, both the hands and feet were warm, and the eyes were not sunk. Ordered warm bottles to the stomach and legs; to take nothing but a little brandy and water, or gruel with brandy in it, and one of the pills every half hour. At nine o'clock he was much altered for the worse; the pulse quite gone at the wrist; hands and feet cold and blue; the eyes sunk, and countenance ghastly; great oppression at the chest, and the voice nearly gone; the cramps were so severe that the poor fellow writhed from one side of the bed to the other with agony, and the sickness and purging occurred every five minutes. I considered the case hopeless, and ordered him a teaspoonful of the mixture in brandy and water every five minutes until the sickness ceased, and then to take in addition to one of the pills as above, one containing two grains of P. Opii every half hour, until the violence of the symptoms was abated. I saw him in the morning, and never did I see so great a change produced in so short a time. The sickness had ceased in half an hour, and the purging about four o'clock; the cramps had entirely subsided; the feet and hands were quite warm and natural; pulse very full, 120, and hard; the tongue from white had become of a vivid red, and the difficulty of breathing was gone; but his head became exceedingly light. I bled him to ℥xx.; ordered to continue the mixture every hour, and drink lemonade. In the evening every unpleasant symptom had subsided; he had passed water freely, and been in a violent perspiration all day; his mouth rather sore, and complained of weakness. Ordered the quinine mixture, a repetition of which completed his cure.

PHOSPHORUS IN CHOLERA.

To the Editor of the London Medical Gazette.

Epsom, July 30, 1832.

SIR,

IN one case of cholera I had practical evidence of the good likely to result

from a careful use of phosphorus. I gave but one grain, in quarter of a grain doses, every four hours. Had it been steadily persevered in, I have reason to think it would have tended materially, if not quite, to cure the patient, who was at the time in the collapsed stage. It will not be thinking too sanguinely to agree with Roi, that it is capable of producing "great effects." Its action being principally exerted on the urinary organs, and in renewing in an extraordinary degree impaired nervous power, it may be well if some of your readers possessing the opportunity will give it a regulated and steady trial. The method of preparing it for exhibition is, to shake a piece of phosphorus in a phial of warm water, by which it is, like oil, separated into minute globules, which, on the addition of cold water, become solid; these are to be reduced to powder, being rubbed up with white sugar. Treacle will be a good vehicle to give it in. What would be the effects of injecting phosphoric ether, largely diluted, into the urinary bladder, if in its contracted state such could be done? Having but few proofs of the inflammatory nature of the action set up in the mucous coat of the alimentary canal, and seeing as we do the depression of vital energy following the serous discharges, could we effectually constringe the relaxed and open exhalants, (no matter whether they are so from a specific affection of their extremities, or as a consequence of the *vis a tergo*) by a solution of alum in decoction of oak bark, thrown up by an enema syringe? The coagulating power of alum is a reason why I prefer it. Could any benefit be derived from the use of ox-gall? As I have not now an opportunity of trying these means, possibly you will excuse my begging publicity for the suggestions; and remain, sir,

Your obedient servant,

M. B. Barry.

EFFECT OF HEAT ON IODINE AND MERCURY.

To the Editor of the London Medical Gazette.

Sir,

Should you deem the following curious circumstance (connected with the change of colour produced by the application of heat to the combinations of iodine and mercury) worthy of notice, you will much oblige by inserting the annexed account of it in your highly-interesting journal.

The per-iodide of mercury, when recently prepared by mixing equal parts of hydriodate of potash and perchloride of mercury, both in solution, is of a fine *scarlet* colour, and easily reducible to an impalpable powder. The application of a very gentle heat to this powder speedily changes it to a sulphur *yellow*, and, by sublimation, fine acicular crystals of that colour are obtained; these, however, on becoming cold, and being allowed to stand for a few minutes, reassume the red colour, and this change is repeated upon every alternation of heat and cold. The crystals possess more lustre than the powder; but this lustre, after the second application of heat, is much diminished.

The proto-iodide of mercury—for so I conceive it to be from analogy—is made by boiling proto-chloride of mercury in a solution of hydriodate of potash. A precipitate is the result, which, in this instance, is at first *yellow*, but which, on the application of a gentle heat, becomes *red*, with a tint of purple; and after being allowed to cool, speedily resumes its yellow colour. This salt, like the former, may be changed from yellow to red at pleasure, on the application of heat; always returning to yellow upon cooling. These effects are not produced by boiling either salt in water, although a much less heat than that of boiling water appears to be sufficient to produce the change; the mere application of the heat of a candle to a piece of paper containing the substance, being adequate to the purpose.

The restoration to the original red, in the case of the per-iodide, and to the original yellow in that of the proto-iodide, is accelerated in each case by triture. The effects I have noticed take place, as far as I have been able to observe, whether the experiment be performed in atmospheric air, oxygen, nitrogen, or carbonic acid gas.

Should this communication be the means of drawing the attention of scientific observers to the investigation of a class of remarkable compounds, there can be little doubt but that the labour

they may bestow will be amply repaid by interesting discoveries in this branch of the science of chemistry.

I am, sir,
Your obedient servant,
WM. E. HEATHFIELD.

Hampstead, July 30, 1832.

MEDICAL GAZETTE.

Saturday, August 4, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

HOMŒOPATHIC TREATMENT OF CHOLERA.

THE application of the homœopathic system to the treatment (we are not yet far enough advanced in the faith to call it cure) of cholera, is one of the general topics of the day, both in the fashionable world and among medical men: it has formed the subject of a communication from one of our ambassadors abroad, and was actually put in practice in the case of a distinguished individual at the west end of the town, who lately died of this disease. We have ourselves, it will be remembered, endeavoured to make our readers acquainted with the leading principles adopted by Hahnemann, the founder of the infinitessimal school of therapeutics; and they are therefore prepared to hear, with understanding, if not altogether believing, minds, of exhibiting the ten thousandth part of a drop of laudanum, or the decillionth of a grain of hellebore.

In chronic affections—where time frequently does so much and medicine so little, and where the gradual and almost imperceptible manner in which effects are produced renders it often extremely difficult or impossible to trace these up to their causes—in such cases, we say, it might have been expected that the disciples of Hahnemann would have preferred to deal. But it is not so: the most rapid and fatal diseases are selected, and they do not refuse to grapple with cholera itself.

Till lately we have not heard much of the system on this side of the Channel, and though some obscure echoes of its fame reached us from time to time, they were looked upon as emanating from the heated imagination of a German enthusiast. Indeed we will not venture to affirm that they are even now looked upon as entirely free from this suspicion by those who are aware that the ten thousandth part of a grain of opium is regarded by Hahnemann as an enormous dose ("*dose enorme,*") and he generally limits his patients to the DECILLIONTH *of a drop of a drop of the tincture.*

Some might imagine that the effects of the medicine diminished in the same proportion as the quantity; but one of the great principles of the homœopathic system is directly opposed to this notion; and we are expressly informed, that the ten-thousandth part of a grain has nearly the same action as the hundredth part, and the hundred-thousandth part as the ten-thousandth part; from which we presume we may infer, that in administering a dose of rhubarb, or any similar purgative, the effects will be nearly as powerful, if we give the hundred-thousandth part of a grain, as if we had ventured to administer so much as the hundredth part—an inference which we are by no means disposed to call in question. This principle, however, does not always hold good, because we are elsewhere expressly told, that the ten-thousandth part of a grain of sulphate of quina is occasionally much too large a dose to venture upon in ague.

The statements in favour of the homœopathic method of treating cholera are so strong, that were we novices in

estimating the value of medical evidence, we should probably be borne away by the torrent of "facts" which sweep by us in the periodicals of Germany and France. But, alas! we have long found medical facts to be so unlike all others, that we are accustomed to weigh them in a different balance; and, with regard to the homœopathic asseverations, we are disposed to receive them with even more than our usual caution—probably as requiring as many of them to constitute one solid unit of indisputable truth, as it needs of their own fractional doses of medicine to make up a ponderable particle of matter.

Howbeit, as faithful chroniclers of the times, we shall record the passing events. The principal remedy, then, employed by the homœopaths against cholera is camphor, and the alledged rate of mortality does not exceed one in ten*; and even this small proportion would not perish (such is the consolatory language of Hahnemann) were all other accessary medicines laid aside. In order, however, to secure these happy results, the camphor must be exhibited at the very commencement, and of the most absolute genuineness. And here we must observe that two very wide doors are opened to the advocates of this doctrine for escape, if need be: first, the medicine may not be of the requisite purity; if there be mixed with it a portion of any thing else, though it be neither sensible to taste nor smell, nor touch, nor sight, nor come within the infinitest degree of subdivision that imagination can conceive, yet will this particle, which is all but a nonentity, wholly subvert whatever good effects the medicine might otherwise have produced. Again: there is a moment not always to be detected even by the initiated, after which camphor ceases to be of avail. "*Alors* (says Hahnemann) *si le médecin a neanmoins recours à ce remede, son emploi laisse mourir le malade.*" When, however, this period is not passed, we are to administer on a morsel of sugar, or in a spoonful of water, from one to two drops of an alcoholic solution of camphor, (one part camphor in twelve of spirit of wine) and to repeat this every five minutes for several successive times. The skin of the chest, arms, and legs, is to be rubbed with the camphorated spirits, while two teaspoonsful of it are to be thrown up as a clyster, with half a pint of hot water, and the air impregnated by placing camphor on heated iron. Two hours are assigned as a proper period, under ordinary circumstances, for this treatment to be persevered in during the first stage; but if any suspicion exist that the patient may already have passed into the second stage, recourse is to be immediately had to copper, in the minute doses which we formerly described, and repeated every half hour till the vomiting and diarrhœa shall have ceased, and heat been restored. During the use of the copper every thing else must be religiously abstained from: aromatics, baths, blisters, bleeding, are all equally pernicious: in short, it must be the copper, and nothing but the copper—"*sans cela le cuivre n'agira pas.*" It is farther stated by M. Hahnemann, that about two decillionths of a grain of copper, taken every morning fasting, the patient avoiding to drink immediately after the dose, is the most certain preventive of cholera which is known. Some, however, are too powerfully affected by these doses, and they are then advised to wear next the skin a piece of the metal as large as a sous. Indeed it sometimes happens that even this is too

* In a table before us, of 1073 cases, treated by ten different medical men on the above plan, the number of deaths is only 95; the remaining 978 having all been cured.

much for them, and they are attacked with all the symptoms of cholera; but then it is only a copper cholera, and, though they may die of it, their friends have the satisfaction of knowing that they have escaped the epidemic.

In this country, the principal, if not the only champion of this doctrine, is Dr. Quin, and the novelty of the practice has led to some notoriety in a country where, supported by the breath of fashion, every bubble floats its little hour. This gentleman, it seems, had long been convinced of the truth of the maxim —*similia similibus*; and, nevertheless, he did not venture to hope that the infinitessimal doses of his master, however they answered beyond the Rhine, would have proved "German to the matter" here: he was sceptical, in short, but experience has removed all his doubts; and the same result, we question not, will follow with all who put the practice to the same test. According to Dr. Quin, cholera is to be divided into six different forms, and the treatment is laid down with an affectation of greater precision than occurs in any paper of Hahnemann's that has fallen under our notice. We have the cholera *acuta, dysenterica, vomitoria, spasmodica, asphyxia vel sicca, et inflammatoria:* but we hasten to inform the reader, in order to relieve him from the apprehension that his patient may expire before he has ascertained which kind of cholera he labours under, that all are to be treated alike, at least at the commencement; and by this means time is afforded for a more deliberate consideration of the particular name to be applied. Camphor dissolved in spirit of wine is the great panacea to be first administered; and we are informed that a marked improvement often becomes apparent after the third dose; or at all events, that 10 or 12 drops arrest the disease with *certainty*, if administered within the first hour, and probably will do so even later. Iced water, in small quantity, but often repeated, is allowed for drink, and sometimes a morsel of ice is placed in the mouth.

But when the disease has already made a certain progress, the camphor is no longer capable of effecting a cure, and immediate recourse must then be had to other homœopathic means, to the absolute exclusion of camphor, as camphor, we are told, would interfere with them. Thus in the acute cholera, white hellebore (*veratrum album*) in the minute doses so often alluded to, is to be given every half-hour, hour, or hour and a half, provided the disease remain stationary, but if there be the slightest improvement, the remedies must be omitted while this continues progressive. If in this form of the malady there is a sense of heat in the epigastrium, intestines, &c. with ardent thirst, great prostration, and fear of death, recourse is to be had to arsenic; if the cramps be violent in the muscles of the chest, with continual vomiting and but little diarrhœa, the *lactuca virosa* must be put in requisition. In the dysenteric form, the diarrhœa is the most prominent symptom, and the chief remedies now are phosphorus and phosphoric acid, remedies of which it is somewhat equivocally said, that it is seldom necessary to repeat the dose. In the variety attended with much vomiting, ipecacuanha in infinitely small doses, and repeated in the same manner as the hellebore in the preceding case, is to be employed in the first place; but if the vomiting cease while the other symptoms continue, recourse must be had to *nux vomica, veratrum*, &c. Chamomile is also sometimes given, both in this and the dysenteric form of the disease. In the spasmodic form of cholera, metallic copper, or the *cupri acetas*, is to be administered; and in the cholera asphyxia, after camphor we are to repose our hopes on hellebore; or if the asphyxia be complete,

on vegetable charcoal and prussic acid! In the inflammatory variety some caution is required in respect to the use of camphor; and it is to be followed by the veratrum, ipecacuanha, and copper. After the vomiting has ceased, aconite is required to counteract the phlogistic disposition.

Such is an outline of the practice recommended by Dr. Quin, and by which, we are informed, he succeeded in curing 27 out of 29 cases of genuine cholera during his residence in Moravia; while of 19 who fell under his care in Paris, nine were cured by spirit of camphor alone; seven by this, followed by other remedies; and the three remaining by other remedies, not preceded by camphor. *All these patients recovered!*

Behold the homœopathic treatment of cholera, and its effects! We give the statements as they are given to us—we nothing extenuate, nor set down aught in malice. The reader may use his own discretion as to the degree of credit he may attach to them, but let him keep this in mind—that the asseverations are as strong, the witnesses as numerous, the details as complete, with respect to this as to any other method of treatment which has been proposed. Let him also answer this question—have not many patients as much medicine poured down their throats as would kill ten men who had no cholera; and if so, how can we be sure that some of them do not die of the remedies? If, notwithstanding all we have said, he will yet be sceptical, let him look on the homœopathic doctrine as a satire upon the system of over-treatment at present so much in vogue—and, in this way at least, Hahnemann may be found to do the state some service.

PROGRESS OF CHOLERA IN LONDON.

We have the satisfaction to state, that up to Thursday the number of deaths from cholera had undergone a very marked diminution, not exceeding one-half what they were a fortnight ago. This is apparently dependent upon the cases having assumed a somewhat milder aspect, as their number (though also on the decline) has not diminished in the same proportion. On the day above mentioned, the temperature suddenly rose very nearly twenty degrees, and became accompanied by a damp state of atmosphere. Some temporary increase may therefore be reasonably expected in the epidemic, but we have not been able to ascertain that such has actually been the case. Upon the whole, there seems good grounds for believing that we have seen the worst of the present visitation; and if so, the metropolis has been mercifully dealt with. At the present moment, the deaths each day from cholera *do not exceed one in every fifty thousand* of the inhabitants—a consideration calculated to allay the apprehensions even of the most timid.

THE CHOLERA AND THE LIFE INSURANCE COMPANIES.

At a late meeting of the various Life Insurance Companies in London, it was stated that, since the first appearance of Cholera in the United Kingdom, up to Friday, the 27th ult. there had been but 36 claims on the score of deaths occasioned by this disease;—a strong proof that its ravages have been chiefly confined to the lower orders of society.

CHAIR OF MEDICAL JURISPRUDENCE, IN THE UNIVERSITY OF EDINBURGH.

The Crown has appointed Dr. Traill, of Liverpool, to the chair of Medical Jurisprudence, in the University of Edinburgh, vacant by the appointment of Dr. Christison to that of Materia Medica. This selection was apparently unexpected by the profession in Edinburgh, eleven of whom employed in lecturing had presented a petition to the House of Commons, evidently directed against Dr. Maclagan, who it was supposed would be chosen in consequence

of his exertions at some of the political meetings in the North. We believe that Dr. Traill was supported by the Lord Chancellor.

CHAPTAL AND ORFILA.

We are sorry to have to record this week also another remarkable death that has occurred in the French capital. The Count Chaptal, the celebrated chemist, expired on Sunday last, in the 77th year of his age. He was a Peer of France, and under Bonaparte's dynasty was for a time Minister of the Interior. In the annals of chemical science his name will long be remembered with praise.

Another very eminent chemist, M. Orfila, was, by the latest accounts from Paris, in a most alarming state of health: having been seized with the prevailing disorder, the early symptoms of it were combatted apparently with success: but a relapse ensued, for the result of which much apprehension was entertained.

THE LATE BARON PORTAL.

There are some circumstances connected with the history of the late veteran Portal, which seem to be well worth mentioning. It was to him that Voltaire applied the complimentary expression, *Il consulte la mort pour prolonger la vie.* Antoine Portal was born in January 1742—thus being at the time of his death seven years older than the venerable Goethe. He studied at Montpellier, and applied himself principally to anatomy. At an early age he came to Paris, and became attached to Senac, whom he assisted in his work on the Heart. Portal's name soon became distinguished, and he reckoned among his friends Franklin, Buffon, and D'Alembert. In 1769 (the year in which Cuvier was born) he became a member of the Academie des Sciences, and took his seat in that assembly, which was then adorned by the presence of Condorcet, Bailly, Lalande, Laplace, Lagrange, and Lavoisier. For sixty-four years he held the Professorship of Medicine in the College of France; and in 1820 was mainly instrumental in organizing the Academy of Medicine, of which he was chosen Honorary President for life.

LONDON HOSPITAL.

Dr. A. Frampton has been elected Physician to the London Hospital, vacant by the resignation of Dr. Macbraire.

MADAME DERUBIGNY BARRÉ.

We have been favoured with the following extract of a letter from the distinguished Professor of Anatomy in Dublin, with leave to make it public if we thought proper. It is too remarkable an example of the absence of prejudice respecting dissection, not to call for our immediate attention.

* * * *

"You were rightly informed, my dear sir, about Madame Derubigny Barré. She did bequeath her body to me for the purpose of dissection, and also 10*l.* for the expense of preparing her skeleton, which she directed should be placed in a glass case in my museum, with a suitable inscription, setting forth that her motives for doing so were to benefit the living, and to give an example for other females to follow. She was a very remarkable person, and had passed through a most eventful life. She was a native of France, and born of a good family. During the "reign of terror," she was kept in prison for two years, under the daily expectation of being led to the guillotine, in consequence of her having concealed her father, who was a reputed royalist. On her trial, she made her own defence, with so much dexterity and eloquence that she was liberated by acclamation. She afterwards joined the republican party, and was a leading person in forming the Band of Amazons, which consisted of 200 of the most beautiful women clothed in armour. She was Secretary to the Female Club, and as such corresponded with the other clubs of Paris at the time that Bonaparte was coming into notice. She was also made Commissary of the prisons, in which capacity she ministered to the comforts of those who were destined to suffer death. She afterwards travelled over many parts of Europe, and finally settled in London and Dublin as a teacher of her native language. In 1808, she published, in London an Elementary French Gram-

mar; and in 1822, in Dublin, a book in the form of letters, in which she gave an account of her own sufferings in France, and a number of interesting anecdotes of her fellow-prisoners, and likewise of the French king and queen during their captivity.

"I was not acquainted with her before she sent me her will; but from those with whom she had lived in habits of intimacy in Dublin, I learned that she was a very benevolent woman, and possessed a very strong and independent mind. She died at the age of 72, of the most extensive cancer of the stomach I ever saw. The stomach was adherent to the parietes of the abdomen, and the ulcer had formed a communication with the transverse arch of the colon, by which the fæces passed into the stomach. I understand she bore her sufferings with great fortitude. She had been a very handsome woman when young: her skull is finely formed, with a very conspicuous development (as the phrenologists would say) of the organs of benevolence and veneration. The remains of her body are buried in the ornamented grave-yard which I have constructed in the neighbourhood of the Anatomy-house, where I propose erecting a monument to her memory, as being *the first* FEMALE *who bequeathed her body for dissection.*

* * * * *

"Most truly yours,
"J. MACARTNEY.

"Dublin, July 27, 1832."

GRANVILLE'S "CATECHISM OF HEALTH."

In a notice of this ridiculous book in the new number of the "Quarterly," the reviewer points out the "good joke" of making *four* editions, "with additions," out of a *single* one, by the simple transformation of the title-page: but he has his doubts whether a "second" edition of the said title-page ever appeared. We happen to be able to assure him that a second *did* appear: we have a copy of it even now before us, it being the identical one which we reviewed last Christmas*: but we are bound in justice to add, that it does not contain the *rom*, in p. vii.—the typographical accident of the omission of an *f*—which is so acutely noticed as being repeated in the "third" and "fourth" *editions!*

The account, it seems, stands thus between Dr. Granville and Dr. Faustus. Faust's Catechism was first published in the original German 40 years ago: it was translated and frequently reprinted in this country within the last 38 years: and we now have it hashed up once more, and offered to the public in "four editions" (of the title-page), under the cookery of Dr. Granville.

We had not room in *our* notice for a tenth part of the good things contained in the Doctor's *réchauffée*, but perhaps the reader may not be displeased to see how they are served up by our contemporary:—

"The Doctor's interrogator soon rises into higher regions, and becomes inquisitive as to the arcana of science. 'What,' says he, 'what have you to observe with regard to *beef?*' The doctor replies, with admirable caution and discrimination,—

'To persons in health, beef—that is, the flesh of the ox—is a nourishing and wholesome food.'

"After some equally profound observations on mutton and lamb, the querist propounds the following theme:—'What *rank* does poultry hold as an article of food *after* meat?' The question implies that the flesh of fowls is not only not *meat*, but that it is food of an inferior and suspicious *rank*. The Doctor answers this question with startling originality,—

'The flesh of the common fowl affords very excellent nourishment, when properly cooked.'

"But the most difficult question of all is that which surprises us at the beginning of the sixth chapter of the second part:—'Ought *bread* to form a proper addition to the other articles of food of which a dinner is composed?' The matter, we see, is deep and difficult, and might involve a doubt as to the mode in which large classes of mankind—the Indians and the Irish, for instance—dine; but Dr. Granville solves the problem with admirable dexterity. 'Yes,' he says, '*bread*, or some other wholesome *farinaceous matter*, should

* See our last vol. (ix.) p. 551.

always constitute a part of this meal; and thus all mankind may safely indulge in bread, rice, or potatoes, as a part of their dinners: Prodigious! Then there is a certain polished civility of language, which, except in Dr. Darwin's 'Loves of the Plants,' we have seldom heard applied to the vegetable kingdom.

'Are onions to be *ranked* amongst the articles of vegetable diet to be *recommended*?'

'Yes,' responds the oracle, IF WELL BOILED.'

'What is the *character* of peas and beans?'

"Their *character* is, we regret to find, rather bad: 'they contain little nourishment, and should *therefore* be refrained from by all except the strong and *laborious*;' the laborious, it seems, being a class which may be content with little nourishment."

Noticing the apparatus of wire-work, recommended by Dr. Granville for keeping a smelling-bottle constantly attached to the nose, for fear of cholera, the reviewer very properly observes, that "if Dr. G. will only exhibit it on his own person—great as he tells us his present practice is—he will be more *followed* than any doctor since the days of Van Butchell!"

CHRONIC HYDROCEPHALUS TREATED BY PUNCTURE.

Christian Littlejohn, whose age is eight months, was affected with chronic hydrocephalus. Her mother observed a few days after birth a greater separation of the bones of the head than natural, after which its size began to increase very rapidly. Eleven weeks after birth, I was requested to see her along with my friend Mr. Moir, Lecturer on Anatomy in this place. By that time the head had acquired an enormous size; it measured in circumference 23 inches, and from the meatus of one side to that of the other, across the vertex, 15½. There was a constant rolling of the eyes and squinting, but there was no unusual dilatation of the pupil, which contracted readily on the application of light. The bowels were irregular, and she was affected with slight startings during sleep. Various methods of treatment had been adopted, viz. compression, blisters, mercury, diuretics, &c. but in spite of these measures the head continued to increase. As the general state of her health appeared good, I resolved upon trying the operation which had been recommended, of gradually discharging the water by puncture. The operation was accordingly performed on the 25th of August, six days after my first visit. The instrument which I employed was a trocar, such as is used in hydrocele. I introduced it about half an inch in depth on the right side of the anterior fontanelle, and three ounces of serous fluid were discharged through the canula. A piece of adhesive plaister was placed over the puncture, and a roller applied around the head. She slept well that night, but next day she was slightly feverish, and continued so for two days afterwards, when she appeared as well as before the operation.

On the 4th day of September, the puncture was repeated in the same manner on the opposite side, and five and a half ounces of turbid serum were evacuated, containing several flakes of lymph. No unfavourable symptom followed. On the 15th September, the size of the head appeared much lessened, and was found to have diminished 2½ inches in circumference, and 2¼ across the vertex. Ossification had made considerable progress. A large opening in the frontal bone, which extended from the bregma to the nose, was completely filled up, while those in other parts were much diminished. In again using the trocar, only an ounce of fluid was discharged. On the 5th of October, I inserted the trocar near to the part I first punctured, and introduced it as far as the meninges, but only a half ounce of fluid passed through the canula; I therefore reintroduced it, and entered it obliquely, about an inch and a half in the direction of the ventricle, and upon withdrawing it, nine ounces of serum were discharged in a continued stream. The wound was closed, and a roller applied tightly around the head. Immediately after the water was discharged, the pulse became feeble, and she was faint and weak; but during the evening she fell asleep, and awoke an hour afterwards apparently much refreshed. To my great surprise, not one unfavourable symptom followed. The pulse, indeed, became more regular than it had hitherto been, the startings during sleep were not so frequent, and she appeared in other respects better, with the exception of her bowels, which continued to discharge stools of a dark green colour. She continued to improve for nearly three weeks afterwards, when her former symptoms gradually returned, and an obscure fluctuation could be perceived by pressing with the fingers above the anterior bregma. Small doses of calomel were administered till the mouth was affected, which shortly produced an absorption of the fluid, and a removal of all the hydrocephalic symptoms. Since then she has had no relapse, and has enjoyed almost uninterrupted good health. She is a stout and lusty child, and her size uncommonly large for her age. The bones of her head

are now complete, excepting the anterior opening, which is closing. The size of the head is less by four inches in circumference, and two and a half across the vertex, than it was previously to the first operation.

With the exception of Dr. Conquest's two cases, I am not acquainted with another in which the ventricle has been punctured for the relief of water in the head. In the cases of Rossi, and Dr. Vose, the water between the membranes only was evacuated. An opinion is entertained by several, that this operation is not only a very dangerous, but an extremely doubtful one. I trust, however, that the result of these cases will prove that such fears are in a great measure groundless, and that, under favourable circumstances, the chance of cure is such as to justify its performance.—*Mr. Russel in Edin. Med. and Surg. Jour.*

LIGATURE OF THE COMMON CAROTID, IN EPILEPSY.

Michael Cox, pensioner, ætat. 25, sanguineous temperament, and muscular. This man has for the last five years been subject to very severe epileptic fits, recurring generally about once a fortnight. He was first attacked whilst on duty, at Burmah, but without any previous warning, and without having experienced any attacks of illness. He had, however, for some time been much exposed to the sun, and undergone great fatigue. He had lived generally temperately, and has now in his possession a certificate of good conduct received from his commanding officer. Since the first seizure, the epileptic fits have generally recurred without any assignable exciting cause, but have been also occasionally induced by intemperance. He has not been able, however, to take by any means the same quantity of ardent spirits, or other intoxicating liquor, as European soldiers generally are; a very small quantity, comparatively, completely overpowering him, inducing extreme giddiness, and violent throbbing headache. He had been frequently bled during the paroxysms, but had not, by his own account, been subjected to any other mode of treatment. The first time the fits were particularly brought to my notice, in this individual, was whilst he was attending at Hospital as orderly over one of the sick. The attack was extremely violent, and his efforts so powerful that it was with difficulty he was restrained by several persons. There was, in this case, great cerebral congestion; a feature which I conceived essential to every attack of epilepsy, and by preventing which (an object I believed attainable by tying one or both carotid arteries) I hoped to cure the disease. Still my expectations of success, did not rest entirely upon the correctness of this view of its nature, which might, I was aware, be erroneous, but yet the epileptic tendency be removed by the operation, although in a manner different from that expected by me.

The operation was performed on the 4th of February. It took up a considerable time, owing to the quantity of blood that flowed upon the slightest incision, obscuring the parts, as well as from the livid colour the patient's face assumed, and the feeling of extreme giddiness, induced by the recumbent position in which he remained during the operation, which obliged me to raise him up repeatedly, as I feared, from these symptoms, the recurrence of another attack. I found it necessary, at last, to bleed him largely from the arm, after which no farther cause of delay occurred. He had been bled freely the preceding day also, in consequence of one of these fits. I divided the sheath as near the larynx as possible, that there might be less chance of wounding the external jugular vein. A small vein that crossed the artery was divided, and there was a considerable flow of blood, which alarmed me greatly for the moment; it soon, however, subsided.

The artery was very little disturbed from its lateral connexions, and was secured with a single ligature; the edges of the wound were brought together, and kept in contact with three sutures, over which strips of adhesive plaister and simple dressing were applied: above these a pad of folded cotton was placed, to keep the skin, as much as possible, in contact with the parts beneath; and finally a roller.

The patient had taken a dose of salts the day before, which had operated moderately.

On the 4th, 5th, and 6th, there were no constitutional symptoms whatever; there was slight uneasiness in swallowing his spittle, or any thing of very small bulk, as it required a greater degree of contraction in the muscles concerned in deglutition.

On the 7th, he complained of pain over the left temple; tongue white and dry; skin warm; pulse 72; bowels open freely: the wound was dressed for the second time, and was nearly healed, but there was some swelling and hardness about the cervical glands. He had been drinking the previous night.

Seven leeches were applied to the temples, and he took six drachms of sulphate of magnesia, as a precautionary measure. He was discharged from Hospital on the 24th, the wound having been for some time completely healed, except at the single point where the ligature hung out. He attended daily to have it dressed.

The ligature came away on the 5th of March.

April 13th.—Since the operation was performed, there has been no return of the epileptic attacks, nor any tendency to them. He has experienced also, since that time, a great improvement in his general health and feelings. His spirits have been good; be-

fore they were almost constantly depressed, and he could not stoop, for any time, without giddiness, and consequent danger of falling. On this account he was unable to work at his trade—that of a shoemaker—and was obliged to abandon it. Since the operation, he has again resumed his work, and has not experienced the least return of these disagreeable feelings. He has also proved the efficacy of the remedy that has been employed, by very hard drinking, which he and his friends considered a test: it required a very large quantity of spirits to make him drunk, and he did not afterwards experience the headache and gloomy and even horrible feelings, which had previously always followed such an excess. He suffered, as he told me, scarcely at all. His whole feelings have undergone a complete revolution, and he is now as happy as he was before miserable and wretched.—*Mr. Preston, in Calcutta Medical and Physical Transactions.*

GENERAL DISPENSARY.

Medullary Sarcoma of the right Lung, and of the right side of the Chest.

Ann Crutchfield, aged four years, was admitted, under the care of Mr. Coulson, April 18th, for a swelling of the right side. The mother states, that about a month ago the child fell against the bannisters of the stairs, to which fall she attributes the origin of the complaint.

On examination there was discovered a firm unyielding swelling of the seventh and eighth ribs, near to their angles, somewhat resembling an egg in shape. The part was not at all painful on pressure. Both sides of the chest were equally acted upon during respiration; the child could walk and run about as well as ever, and the child's health was apparently not in the least affected. No decided opinion was at first given respecting the nature of the tumor; the treatment which was ordered consisted in the application of leeches to the part every other night, and the use of the hydriodate of potass ointment. In less than a month the swelling had much increased in size, retaining however its firmness, and then suspicion was entertained of its malignant character. The general health continued good until the middle of June, when cough and dyspnœa supervened, and one spot of the swelling felt softer than the rest. A consultation of the medical officers was held on the case, who decided that it was not empyema, the only complaint to which, from its external character, this had any resemblance. The general impression was, that the child laboured under some malignant disease. The symptoms of the affection of the chest continued to increase, and on July 5th the child expired.

Beneath the integuments covering the swelling, the cellular tissue was considerably thickened; the posterior edge of the swelling was covered by the latissimus dorsi, the remaining part by the serratus magnus. These muscles were not altered in character. The chest was then opened, when the whole of the right lung, with the exception of two or three spots only, was found converted into one large white mass, adherent to the pleura covering the anterior and posterior parietes of the chest, but not to the part corresponding to the external swelling. This mass, when cut into, resembled the medullary substance of a soft brain; some parts in the centre were quite in a liquid state, like thick pus, and from almost every portion this liquid could be squeezed out. There was no appearance of blood in any spot, and the pulmonary artery was nearly obliterated, but several parts had a delicate reddish tint, or hue. On the right side of the thorax, extending from the sides of the bodies of the vertebræ along the hollow of the sixth, seventh, eighth, and ninth ribs to their cartilages, there was a thick mass, similar in character to that of the right lung. The ribs were not destroyed, but the seventh and eighth could be easily cut with a pair of scissars. The diseased mass on the outside of the chest did not correspond in extent to that internally; it formed, however, part with it, as the intercostal muscles were completely destroyed. The pleura was very much thickened. The left lung was in a state of high inflammation, and there was considerable effusion of serum into this side of the thorax. The heart was healthy, as were also the abdominal viscera. The head was not examined.

Contraction of the Fingers of the left Hand—Cured.

Lucy Anderson, aged 20, of a good constitution, was admitted under the care of Mr. Coulson, for an incipient contraction of the fingers of the left hand. Six weeks before, whilst cutting a shoulder of mutton across, the knife slipped, and penetrated the dorsal surface of the articulation of the first, with the second phalanx of the middle finger of the left hand. The wound was healed in a week; after which the finger became contracted, and was painful on every effort to extend it. The contraction gradually increased, and extended to the other fingers. The joint was swollen, and great pain was felt when any attempt was made to straighten either the injured finger or the others, and the pain extended up the arm. The affection was treated on the same plan as chronic inflammation of any other joint: repeated blisters were applied, and internally small doses of blue pill and rhubarb were given, so as to affect the mouth; and under this plan of treatment she gradually recovered.

Disease of the Shoulder-Joint.

John Bigsteer, aged 24, applied at the General Dispensary for an affection of the right shoulder, of six months' standing. On examining the patient, the right shoulder was found enlarged, particularly in the direction of its transverse diameter; the muscles of the arm were wasted, and the arm itself longer than that of the opposite side. The arm was quite useless, and the shoulder at the upper part was painful; the pain was particularly acute when the head of the bone was pressed on, or when it was rotated against the glenoid cavity. There was also a pain in the course of the biceps and just above the olecranon, near to the insertion of the triceps. The man's general health was good. Mr. Coulson ordered him to be cupped to sixteen ounces on the shoulder, to take some aperient medicine, and to keep the arm quite still. The cupping was repeated five times, to ten ounces each time, and a moxa was afterwards applied over the joint. At the end of five weeks the patient could raise the arm to a right angle with the shoulder, and after two months he was quite cured.

MIDDLESEX HOSPITAL.

Cases of Aneurism of the descending Aorta.

Case 1.—Jas. Hopkins, a lath-vender, æt. 47, admited October 18, 1831. He complained of constant pain in the left iliac region, with paroxysms of increased severity, and of numbness, and at the same time tenderness, of the ilium and upper part of the left thigh; which he could not (when sitting) raise up without the assistance of his hands, though he was able to move it in walking.

He said he had been subject to this pain, "off and on," for seven years, and had never been free from it for the last three years; that, when most acute, it was attended with perspiration, but no sickness; was somewhat relieved by fomentations, and had of late been much allayed by opiates. He was most easy when on his right side, but at one time he could not lie in that position, on account of the sensation of dragging which it produced in the pained part. Formerly he had some difficulty in making water, but this symptom had left him. The urine was cloudy, and faintly acid. His bowels had been quite regular, and his stools large and cylindrical, before he began to take opium. He had no fever.

No fulness, nor hardness, nor tension, could be detected by the touch in the left iliac region; the pain, however, was increased by pressure made just within the rim of the pelvis and above Poupart's ligament, in the situation of the left kidney, and in the left side intermediate between these parts.

Nothing gave him any relief but opiates. He took a third of a grain of the acetate of morphia twice a-day, and by degrees the paroxysms of severe pain left him, and the constant sense of uneasiness diminished, and he thought himself better. After some time he left the hospital. Dr. Watson, in a clinical lecture, mentioned the case as one of great obscurity; at first he had suspected disease of the left kidney—afterwards, that it might be in some part of the descending colon, or in the upper portion of the rectum; but no suspicion was entertained of its being aneurismal.

After leaving the hospital he became worse, the general character of the symptoms remaining the same, except that latterly he had again complained of a sensation of dragging above the left groin, and of an increase of pain whenever his bowels were moved.

He died about the middle of last month, and the body was examined by Mr. Bullen (under whose observation the patient had been both before and after he was in the hospital), on the 17th June, 1832.

A considerable quantity of blood was found extravasated in the cavity of the thorax. On further examination this was found to have proceeded from the bursting of an aneurism of the descending aorta, which commenced at the sixth dorsal vertebra and extended to the second lumbar, involving the left kidney, a small portion only of which remained. The bodies and transverse processes of the vertebræ within the sac were completely denuded, and in a great measure destroyed by the pressure of the aneurism. The walls of the tumor were about two inches and a half in thickness, and composed of a fibrous substance. The aorta was enlarged at its arch, and depositions of ossific matter were found in various parts of its inner coat. The heart and lungs were healthy. The right kidney and ureter were larger than common. In other respects the abdominal viscera were free from disease.

Case 2.—Jno. Burkitt, a police constable, æt. 28, having a dusky complexion, and a countenance expressing much distress, was admitted by Dr. Watson, May 31, 1832.

He complained chiefly of pain in the epigastrium, where there was a large, solid, irregular projection, pulsating at the same time with the radial artery, and a little later than the heart. Each pulsation was attended with a forcible elevation of the projecting part, and a distinct whiz, audible through the stethoscope. A circumscribed tumor was felt, apparently near the surface, and extending obliquely from the convex edge of the false ribs on the left side into the right hypochondrium. A space of about an inch intervened between the tumor and the edge of the ribs on the right side. The action of the heart was natural. Both the fe-

moral arteries beat strongly. Pulse at the wrist soft and small; tongue white on its edge—dry and red in its centre.

He declared that he had felt no pain in the epigastrium, and was not aware of any swelling there till within the last fortnight; but that for several months he had been subject to aching and uneasiness in the back and in the lower part of the belly, and that he then suffered in the same parts, as well as in the situation of the tumor, a constant gnawing pain, which was occasionally much aggravated, especially towards night. The swelling had rapidly enlarged since he first noticed it.

He did not recollect any strain or injury, and knew of no circumstance which might account for the appearance of the tumor. He had a trifling cough, and said that he was short-breathed; and there was some inequality in the sound of his respiration in different parts of the lungs.

He remained in the hospital till the 12th of July, when he died. He was once or twice bled from the arm, and leeches were several times applied over the tumor, with but little benefit; and the general circulation became so feeble that it was not thought adviseable to push the bleeding further. His sufferings were greatly mitigated by moderate doses of the acetate of morphia. Nausea and vomiting of bilious fluid occurred on one or two occasions. The tumor enlarged somewhat, and its shape varied a little from time to time—probably with the varying condition of the stomach and intestines which lay above it. The pain felt in the back extended upwards between the shoulders, and considerable tenderness came on over the lumbar and dorsal vertebræ. Subsequently he complained of much pain in the left groin, and became unable to void his urine without the assistance of the catheter. The pain in the groin was always increased when he lay on his left side. His usual position was on his right side, with his knees drawn up towards his chest. For the last week of his life he had continual pain in the left iliac region, and numbness of the left thigh. He sunk gradually. The nurse who laid him out observed that the swelling had entirely disappeared.

The body was examined twenty-four hours after death. The abdomen was flat. A dark spot on the epigastrium marked the former situation of the tumor. When the abdominal muscles were divided, a stratum of coagulated blood was found between them and the peritoneum, increasing in thickness from above downwards as far as the pubes. There was no fluid in the cavity of the abdomen, and no apparent displacement of the viscera. Upon turning the intestines aside, two separate aneurisms of the ventral aorta became visible, situated nearly opposite to each other, immediately below the cæliac artery. Each was about the size of an orange. One, which was rather the larger of the two, projected forwards and to the right side; this had not given way. The other extended backwards and to the left side, and pressed upon the second lumbar vertebra, the body of which was excavated by absorption, but not carious, being covered with a thin layer of ligamentous substance. This second aneurism had given way on its left side; the peritoneum, however, had not been ruptured; the blood which had escaped, amounting probably to eight or nine ounces, had diffused itself in the filamentous tissue behind it; the greatest quantity was by the left kidney, around which it formed a large grumous clot.

The heart and remaining portions of the aorta were free from disease, as were all the other viscera.

MR. ARNOTT TO DR. JOHNSON.

To the Editor of the London Medical Gazette.

Sir,

Had I been aware that articles are admitted into Dr. Johnson's journal without his cognizance, as appears from his statement in the Gazette of this day, I should not have thought it requisite to notice that which was the subject of my letter to you of the 5th inst.—I am, sir,

Your obedient servant,

James M. Arnott.

Saturday, July 28th, 1832.

METEOROLOGICAL JOURNAL,

July 1832.	Thermometer.		Barometer.	
Thursday . 26	from 50 to	71	30·12	Stat.
Friday. . . 27	49	71	30·08 to	30·14
Saturday . 28	50	70	30·17	30·30
Sunday . . 29	51	71	30·35	30·29
Monday . . 30	43	73	30·30	30 34
Tuesday . 31	42	73	30.22	30·14
August				
Wednesday 1	44	71	30·07	30·01

Wind, N. and N.E.

Except the first and two last days, generally clear; rain in the evening of the 1st inst.

On Monday evening, about 5 minutes before 9, a Meteor, of a rather extraordinary kind, made its appearance in the northern part of our hemisphere: it was first seen in Camelopardalis, from whence it slowly proceeded in a direction towards Coma Berenices, near to which constellation the meteor disappeared. Though scarcely dark enough to render even the pointers of Ursa Major visible, yet the meteor shone with great splendour, and by one more fortunate than myself, having observed its transit complete, its light was thought sufficient to have caused a shadow. But the more remarkable fact is, that the train, which was about 8° in length, remained visible, and clearly defined, for more than 3 minutes. The evening was remarkably calm.

Charles Henry Adams.

NOTICE.

We did not receive Dr. Allsop's paper till the No. was actually in the press. It shall appear next week.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 11, 1832.

ESSAYS ON DIAGNOSIS.

BY

MARSHALL HALL, M.D. F.R.S. L. & E. &c.

ESSAY I.

I. ON THE SOURCES OF DIAGNOSIS.

IT appears quite unnecessary to insist upon the importance of diagnosis. The distinction and identification of the disease are now universally acknowledged to be the first part of the office of the physician in his actual visits to the sick.

With the diagnosis must be associated a knowledge of the nature of the disease, or *pathology*, and a due appreciation of the powers and condition of the patient, or, to use a neglected phrase of great practical utility, the "*constitution.*"

We are, in this manner, led to the ultimate object of the physician, the appropriation of the remedies, or *therapeutics.*

Having distinguished the disease from all others, we have still many an arduous duty to perform: we have to consider of what nature it may be; in what stage it may be; what are the peculiar changes of action in which it consists; what organic changes it may have already induced; what effects it may have had on the vital functions and actions; what devastations it may have produced upon the powers of the system.

These points are to be reconsidered at each visit. The actions of the disease; the effects on the structure of the part affected, and on the system at large, vary from day to day. They must be watched and traced.

But even this is a simple view of the case, compared with that which actually occurs in practice. Very few cases of disease are *simple.* Generally more than one organ is affected, and the principal disease is, either from the beginning, or in the course of the affection, complicated with others, seated more or less remotely. Having detected and distinguished the principal disease, we have continually to ask, what others—what *complications* there may be.

Another cause of complication which operates in the course of the disease, is, the very remedies employed; and we have also continually to ask, whether such and such a phenomenon belongs to the disease, or to the treatment,—whether it be an effect of the morbid actions, or of the remedies.

Complicated as this view of the practice of physic may be, it is rendered easy and familiar to the student by proper arrangements, and to the practitioner by the force of habit. It will be our object in the present instance to offer such an arrangement of the sources and objects of diagnosis as may be useful to the young and the inexperienced, for whom these essays are entirely destined.

The sources of diagnosis may be arranged under the following heads:—

I. THE HISTORY.
II. THE SYMPTOMS, OR CHANGES OF FUNCTION.
III. THE EFFECTS OF REMEDIES.
IV. THE MORBID ANATOMY, OR CHANGES OF STRUCTURE.

I. The first of these, or THE HISTORY OF DISEASES comprises

i. THE CAUSES. These are

1. *Constitutional.*
2. *External.*

ii. THE COURSE, which is

1. *Acute.*
2. *Chronic.*
3. *Insidious.*
4. *Sudden; &c.*

II. THE SYMPTOMS, OR CHANGES OF FUNCTION, are observed in

1. *The Countenance.*
2. *The Attitude.*
3. *The Tongue.*
4. *The General Surface.*
5. *The General System.*
6. *The Functions of the Brain, the Spinal Marrow, and the Nerves.*
7. *The Respiration.*
8. *The Circulation.*
9. *The Stethoscopic Signs.*
10. *The Functions of the Alimentary Canal.*
11. *The Functions of the Urinary Organs.*
12. *The Functions of the Uterine System.*
13. *Examinations of the Abdomen, Rectum, Vagina, &c.*

III. THE EFFECTS OF REMEDIES are

i. IMMEDIATE.
ii. REMOTE.
iii. DIAGNOSTIC.
iv. MORBID.

They are seen chiefly in the administration of

1. *Bloodletting.*
2. *Emetics.*
3. *Purgatives.*
4. *Opiates.*
5. *Antimony.*
6. *Mercury.*
7. *Digitalis.*
8. *Alcohol.*
9. *Quinine; &c.*

IV. CHANGES OF STRUCTURE are

1. *Febrile.*
2. *Exanthematous—Eruptive.*
3. *Inflammatory.*
4. *Arthritic.*
5. *Rheumatic.*
6. *Scrofulous—Tuberculous.*
7. *Scirrhous.*
8. *Fungous—Tuberous.*
9. *Melanotic.*
10. *Dropsical.*
11. *Hæmorrhagic; &c.*

Such is an imperfect enumeration of the sources of diagnosis. The first of these, or the history, teaches us much of the probable progress of the disease, in inducing local changes in structure or devastations of the powers of the general system. The symptoms point to the organ principally affected. The effect of remedies, carefully considered, throws an important ray of light upon the nature and violence of the disease, and upon the energies of the system. The examination of the changes of structure proves an important corrective of our opinions.

It must be acknowledged that it is to the study of morbid anatomy that we are principally indebted for the recent progress, and, indeed, for almost all that is solid in medical science. It is by the investigation of morbid anatomy that we are principally enabled to establish correct species of disease; but it is equally true, that all the advantages which spring from our knowledge of changes of structure must flow through that of the history and symptoms, as the channel, to our individual patients. The progress of medicine as a science—might we not say, as an *abstract* science?—may be considered as greatly dependent on that of our knowledge of morbid anatomy; but the advancement of physic a sa practical art, is intimately linked with our knowledge of the history, symptoms, and the effects of remedies—with the diagnosis of the disease in the living patient.

On the other hand, our knowledge of the "constitution" of our patient—a part of medicine which has been altogether neglected lately—depends entirely upon the history, symptoms, and effects of remedies. It is to this department of knowledge that Celsus alludes in the following paragraph:—"Ob quæ conjicio, eum qui propria non novit, communia tantum intueri debere; eumque qui propria nosse potest, ea quidem non oportere negligere, sed his quoque insistere. Ideoque, cum par scientia sit, utiliorem tamen medicum esse amicum, quam extraneum." It is to this department of medical

knowledge that I would particularly call the renewed attention of the profession. Every physician feels how much easier it is to prescribe for a patient for whom he has frequently prescribed before, than for a stranger. The habit of such a patient in regard to the kind and severity of the disease, and in regard to the power of supporting important remedies, is familiarly known to him. There is in every one a certain *idiosyncrasy*, to which it is highly important to attend with scrupulous care. This notion is become antiquated of late; it is nevertheless founded in truth, and will meet with acceptance, as an old friend, by all practical physicians.

One department of knowledge, to which the reader's attention will be repeatedly called, may be termed new; it is that of the effect of remedies, and especially of blood-letting, as a diagnostic of diseases, and as a criterion of the powers of the patient.

The *morbid* effects of remedies, as illustrated in the cases of excessive reaction from loss of blood, of the crethismus mercurialis, &c. will also be brought before the reader in a subsequent part of these essays. Other instances of the morbid effects of remedies are afforded by purgatives, opium, digitalis, &c. It is frequently an interesting question, involving the safety of the patient, whether a given symptom belong to the disease or to the remedy—whether the former be unsubdued, or the latter be already given in excess. Mr. Brodie observes, in reference to the treatment of injuries of the head*,—"Where bleeding has been carried to a great extent, symptoms frequently occur which in reality arise from the loss of blood, but which a superficial observer will be led to attribute to the injury itself, and concerning which indeed it is sometimes difficult, even for the most experienced surgeon, to pronounce in the first instance to which of these two causes they are to be referred." Dr. Bateman† remarks on that effect of mercury, termed *erethismus mercurialis*, in his own case:—"It is evident that the features of the malady are not sufficiently known, even to the most enlightened members of the profession; for the failure on the part of the medical advisers, in the instance about to be related, to recognise its first symptoms, and the consequent repetition of the dose of the poison, after its commencement, had nearly proved fatal." I shall never forget a tragical instance of the effects of digitalis, under precisely parallel circumstances.

The study of morbid changes of structure must be pursued in a new manner, before it can reflect all the advantages which it may do upon the practice of physic—that is, in connexion with the previous morbid actions; otherwise, how can they be anticipated—prevented? The very nomenclature must be changed. Certain morbid structures are found in febrile, in eruptive, in inflammatory, in scrofulous, in hæmorrhagic, diseases; they have all been designated as inflammatory, and there have been interminable disputes whether they are causes or effects. The true mode of pursuing this subject is, to associate the morbid change with the previous disease, and, as much as possible, with its symptoms, its periods, its degrees of severity, &c. just as, during our attendance on the sick, we should each day inquire—what is, at this precise period, the probable state of the structures?—of the constitutional powers?

This is the more essential, because any given morbid change of structure is seldom or never met with in patients, as they are in books, distinct and isolated. It seems probable, indeed, that the solids, the fluids, and the nervous system, are variously but simultaneously involved in all diseases. The morbid change is seldom confined to a part—an organ. Certain morbid appearances, and certain associations of morbid appearances, are met with in fevers, in the eruptive diseases, in inflammations, in scrofulous or tuberculous affections, in dropsies, in hæmorrhages, &c. to which my attention has been forcibly drawn, and to which I wish to draw the attention of the profession. Such forms and such *associations* of morbid changes constitute *the* disease. Each of such forms is *peculiar*. The same change of structure observed in different diseases, according to our usual phraseology, is not, in fact, *the same*. The inflammatory affections of the skin which occur in scarlatina, in rubeola, in variola, are not the same. In the same manner morbid changes of structure, observed in febrile, inflammatory, and other diseases, although designated by the same term, are not in truth the

* Medico-Chirurgical Transactions, vol. xiv. page 382.

† Ibid, vol. ix. page 220.

same. It is on this account that I have rather chosen to speak of morbid changes as febrile, exanthematous, inflammatory, &c. for in this manner alone we identify such morbid changes, and associate them with the individual disease. I have no doubt, too, that, by a diligent and careful scrutiny, such changes of internal structure will, like the appearances on the skin, be found to be peculiar in each disease. We should not be satisfied in speaking of the cutaneous affection in variola, rubeola, and scarlatina, as mere inflammation. On the contrary, we carefully preserve the idea of difference, of peculiarity. The same observation applies to the internal changes of structure. The situation, as well as the character, of these affections, is peculiar. In fever, we look for an affection of Peyer's glands; in rubeola, for bronchitis; in scarlatina, for affections of the throat, &c. . But it still remains to shew, that in each and every disease, the very morbid change of structure itself is peculiar.

There is another view of this subject. Such changes may occur in a given *series*, or with a given course of the disease; and this series may flow from the original causes, as successive local causes and effects, as the effects of remedies, or from the condition of the system: some are even cadaveric. In fever, we may first have ulcerations of the intestines, then hæmorrhage, as a consequence; then the sinking state, and its effects upon various organs. Of what value is abstract morbid anatomy, undetected during life, unassociated with the history, the symptoms, and the effects of remedies, untraced to the individual disease?

TWO CASES
ILLUSTRATIVE OF THE
EFFICACY OF IODINE IN THE CURE OF ULCERS,
Arising from Venereal Buboes.

To the Editor of the London Medical Gazette.

12, Dock-Street, Hull,
Aug. 2, 1832.

Sir,

If you think the following communication worthy of insertion in your valuable publication, be pleased to insert it at your earliest convenience.

I am, sir,
Your obedient servant,
Isaac Lyon,
M.R.C.S L.

Having been baffled for a long time in the cure of a large venereal ulcer, situated in the groin, I was led to the trial of iodine, both externally and internally, from reading Lugol's Observations on Iodine in the Treatment of Scrofulous Affections; and should it upon further trial be found that iodine will cure ulcers of this nature, it will be a discovery of great value to public institutions in the saving of sarsaparilla, which in most cases of this kind answers well; but even here I have sometimes seen it fail.

M. F. æt. 20, of a lymphatic temperament, applied to me on October 8, 1831, for a large ulcer in the right groin. On examination I found it to be three inches in length, and one and a half in breadth, with extremely irregular and jagged edges, and an unhealthy blush of redness surrounding them. She stated that she had been ill eleven weeks, but that she had not perceived any thing the matter with her previous to finding a swelling in the groin, a point I very much doubt, as her husband, who put himself under my care at the same time with herself, had sores upon the glans penis. She had during this time been under the management of a druggist, who had given her very large quantities of mercury both externally and internally, so as to affect the mouth two or three times very much. She had very great pain in the sore, and her health was very much disturbed. I began the treatment by applying the nitrate of silver to the surface of the ulcer, adjoining the margin, and gave her sarsaparilla in the form of pill, made from the compound extract of sarsaparilla, and applied simple cerate, spread upon lint, as a dressing to cover the wound. No amendment having taken place after a lapse of three weeks, I determined upon trying iodine, both externally and internally. I gave her three minims of the tincture of iodine (made by dissolving twenty-four grains of iodine in an ounce of rectified spirits of wine), three times a day, in a little water; and applied an ointment (made by rubbing up 3j. of the hydriodate of potass, very finely powdered, with an

ounce of lard), to the wound. This irritated it so much that I was under the necessity of reducing its strength to ℈j. to the ounce. In the course of three months the ulcer was perfectly healed, without any other medicines being given either externally or internally. Emboldened by the successful issue of this case, I determined in a future case of a similar nature to put the same plan into practice; and this I did in the case of a man.

J. R. æt 21 (May 7, 1832), of a pale unhealthy appearance. He had had a sore upon the glans penis eleven weeks previously, which continued four weeks, and then healed. This was followed by a bubo in the right groin, for which he had been under the care of a druggist for seven weeks, who had given him large quantities of mercury, both externally and internally, without affecting the mouth. The bubo suppurated, and was followed by an ulcer about half the size of the one in the former case, and having the same appearance. I began by giving him thirty minims of the tincture of iodine in three ounces of water, a table-spoonful to be taken three times a day; and added five minims to the mixture every four or five days, as he could bear it; so that, towards the close of the treatment, he took twelve and a half minims for a dose, beyond which he could not bear it, as it produced nausea. On 29th June the ulcer was healed, exactly seven weeks from the commencement of the treatment. The only other remedy I applied was, that I twice touched the edges of the sore with the nitrate of silver; but this appeared to me to have very little effect in the cure. In any future cases which may present themselves to me, I shall use no other remedies but the iodine, so as to be able to speak still more decidedly than I can at present upon its value in cases of this description; but still I think it is well worthy of trial, and shall be happy if others find it of the same benefit that I have done. Should I meet with any more cases of the same kind, I will detail to you the result.

STATE OF THE MUCOUS MEMBRANE IN CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

The very general complaints of the inefficiency of all remedies in severe cases of cholera, pointed out to us, in the commencement of this inquiry, that our time would be better employed in a careful study of the nature of this disease than in an experimental application of any new remedies which conjecture might suggest. At the same time we considered it our duty to the sick carefully to select, out of the various plans of treatment recommended by sufficient authority, such as, in our judgment, appeared most suitable to the circumstances of different cases, and to give them a cautious, fair, and sufficient trial.

Partly for the purpose of shewing the exact value of the remedies we employed, by accurately delineating the circumstances in which they were used, as well as the effect they produced; and partly from a wish to present the facts from which our pathological conclusions are drawn in a form open to strict examination; we claim the indulgence of being allowed to recite several cases, well knowing that such details are in general dry, and little interesting.

To any person contemplating this disease at the bed-side of a patient, the following objects of inquiry (necessary to answer the important question, how shall we cure this disease?) at once present themselves.

What is the nature of this peculiar disorder of the alimentary canal?

What is the nature of this remarkable affection of the organs of circulation, and of the blood itself? — Which of them takes precedence in the order of seizure?

Is one dependent on the other, or are they distinct and separate affections?

Under what difference in circumstances do their different modifications occur?

Will the answer to the last question throw any light on the nature, treatment, prevention, or mitigation of the severity of the malady?

1st. What is the nature of this peculiar disorder of the alimentary canal?

We naturally first ask, what are its appearances on dissection?

Case I.—Thomas Mansall, æt. 18 months, was seized about six o'clock P.M. July 20th, with violent vomiting and purging, screaming out from pain, which occurred in paroxysms. The dejections very soon became a pale limpid fluid, depositing white flocculent particles. In the course of two hours the violence of the vomiting, purging, and the fits of pain, abated; but prostration rapidly increased. He was seized with cramps, and his hands were spasmodically contracted. About ten o'clock his colour had become very livid, and extreme coldness of the body had taken place, without sweat. Difficulty of breathing was now observed; this increased along with the prostration, until almost the only signs of life were deep and slowly-drawn sighs, at long intervals. He died about 4 A.M., 21st, ten hours after seizure. To particular inquiries as to the first appearance of illness, his mother stated that he had screamed out several times about 2 P.M. but afterwards played about as usual, until seized with the vomiting and purging.

Inspection about 12 *hours after Death; D. Booth and W. Schofield being present with us.*—The body did not present any appearance of previous deficient nutrition; the nails were livid; the extremities contracted and shrivelled; the limbs rigid; the eyes sunken; the livor of the surface in general was very slight.

The appearance of the alimentary canal, although examined after the thorax, deserves first notice. Its colour externally was natural. A quantity of pale limpid fluid, in which floated white flocculent particles, was contained in the stomach and intestines, but without any trace of bile or fæculent matter. The colour of the mucous membrane was pale throughout its entire extent, except in the cardiac portion of the stomach, where slight injection of the capillary vessels with bright-coloured blood was apparent.

The mucous follicles from the cardia to the rectum, both solitary and aggregate, presented a remarkable appearance. The solitary follicles projected above the surface of the mucous membrane, like small round bodies of a whitish opaque colour, from the size of a pin's head to that of a mustard-seed, and somewhat larger; almost entirely covering the cardiac portion of the stomach, here being most opaque. They were more thinly scattered in its pyloric portion; still scarcer in the small intestines, and again numerous in the colon and rectum. The aggregate follicles of the small intestines were diseased in the same manner. This opacity diminished in the small intestines, still more in the arch of the colon, until they became quite transparent in its sigmoid flexure and in the rectum; both of which were closely contracted. Dr. Allsop, who performed the dissection, struck with their appearance, which instantly suggested to him that the nature of their contents might explain the origin of some part of the peculiar secretions of cholera, drew his scalpel over the mucous membrane of the stomach; and observing that a substance, of the colour and consistency of thick unboiled paste, was expressed from them, exactly similar to the flocculent matter floating in the limpid fluid contained in the canal—a depression occupying the place where the follicles had been prominent before—immediately said, "these are the parts by which the flocculent portion of these secretions are formed." The follicles throughout the entire extent of the canal were afterwards examined in the same manner, with the same result. The substance expressed from them varied a little in appearance: that from the stomach was more opaque than that from the small intestines, and that of the sigmoid flexure of the colon and rectum was entirely transparent, but very tenacious, and resembling that gelatinous substance often found in the evacuations of children when labouring under other disorders of the bowels.

The glands of the mesentery were generally enlarged and hardened, of a pale transparent colour, not yet presenting that livid appearance studded with opaque points, which is common in the further advanced stages of this scrofulous affection. The urinary bladder was closely contracted and empty. The gall-bladder was distended, of a dark-green colour; its duct pervious, and gorged to its termination, the inner membrane of both being lined with dark-green and very tenacious mucous. Not the least unhealthy appearance could be discovered in the semilunar ganglia, in the solar, mesen-

teric, and renal plexus, nor in the par vagum, and branches of the sympathetic nerve, either in the thorax or abdomen.

The lungs were collapsed to an extent unusual in death from other diseases. Their anterior part was of a pale colour, but they were dark purple towards their root posteriorly, from the settling of fluid blood, which oozed from them freely on section. The internal mammary veins, the cavæ, and other veins of the thorax, were distended. The right auricle of the heart extremely turgid; the right ventricle contained a very small quantity of fluid blood, its walls were thin and flabby; the left ventricle was entirely empty, its cavity exceedingly reduced in capacity, and its walls, in consequence of an unusual thickness, giving an appearance of hypertrophy to this side, and of dilatation to the opposite; the left auricle contracted, contained a very little; the roots of the pulmonary veins some blood, but insufficient to render them turgid. Not a trace of coagulum could any where be perceived, nor any difference of colour in the blood contained in any part of the circulating system in the thorax.

In the head, the turgescence of the veins of the brain (even to their minute origins) was extreme; their blood fluid, and of a very dark colour. Numerous dark points appeared on section throughout the substance of the brain, both cineritious and medullary, as well as in the cerebellum, the medulla oblongata, and upper parts of the spinal column; they were, however, largest, and most numerous, in the medullary substance of the upper part of the hemispheres. About half an ounce of serum was found in the lateral ventricles, and a similar quantity at the base of the brain.

It is probable that in this case the appearance of the mucous follicles of the intestines was more remarkable than common, or such a striking diseased state could not have escaped notice in the many dissections performed by others*. The diseased action peculiar to cholera was probably here modified by a scrofulous constitution, yet turgescence of follicles with this peculiar secretion, having never been before found by us in numerous dissections of scrofula in every stage, we felt the greatest desire minutely to examine the condition of these follicles, and of the remainder of the mucous membrane, in as many cases as possible. After being refused the examination of some fatal cases, we obtained permission to dissect two; but one of us being prevented attending by indisposition, the precaution was taken of removing portions of the diseased structure, and sending them to Dr. Booth, that he might, if agreeable to him, bear witness to the degree of fidelity observed in the description, and as to their correspondence with the appearances in the above case.

Cases II. & III.—Thomas Page, aged 7, was seized about eight o'clock on 25th July with vomiting, purging, and violent pain of the bowels. These symptoms had not continued more than an hour, when he sank into a state of exhaustion. Pallor had accompanied the first accession of the purging, and it became shortly exchanged for a livid tint of the surface. After this he lay in a state of heavy dosing, roused at times by pain, restlessness, thirst, and complaining of a load and sense of constriction at the pit of the stomach, making breathing difficult. His surface was cold, hands contracted, pulse imperceptible, respiration deep and heavy, pupil dilated, eye squinting; but the purging and vomiting almost entirely ceased soon after the accession of this prostration. Cold sweat and cramps did not at any time come on. The heat of his body returned in a slight degree about seven o'clock in the evening, but without any other improvement. He died at midnight.

The body was examined about thirteen hours after death, together with that of his father, aged 42, who was seized a little later on the same day, and died about seven o'clock in the evening. It is unnecessary to detail his symptoms now; the case was remarkable for the severity of the spasms, and its rapid progress. Blood-letting was used with the effect of removing the cramps, and apparently of retarding death. This will be described when treating of the effects of remedies.

The two bodies lying side by side,

* The authors will find some interesting information connected with this subject in Dr. Chambers's account of a disease resembling cholera, which attacked a school at Clapham in August, 1829,—see Gazette, vol. iv. pages 375 and 410; and in several papers in this journal, particularly those by Dr. Hope, see vol. ix. pages 883 and 905; also M. Dupuytren's views, same volume, page 874.—Ed. Gaz.

presented a favourable opportunity for comparing their appearances. The habit of the son was spare,—that of deficient nutrition; that of the father robust, the fat lying two inches thick on the abdominal muscles. In the thorax of both, very similar appearances were found. The veins of the walls of the chest were, however, more extensively engorged in the child, even to their minute branches; which ramified in the cellular substance connecting the nervous filaments of the par vagum and sympathetic;—an appearance often remarked along with congestion in hooping-cough, but entirely without any discoloration of the pure white tint of the nervous filaments themselves.

In both, the venæ cavæ, likewise, and the veins of the surface of the heart, were much distended. The right auricle was remarkably turgid. The right ventricle contained a little blood; its walls thin and flabby. The left ventricle contained scarcely any; its walls, as in the former case, thick, and very firm, and the capacity of its cavity reduced. The left auricle presented a remarkable contraction contrasted with the right; yet it, as well as the pulmonary veins, contained some blood. The blood on both sides of the heart was equally dark. The right ventricle contained, in the father, a small, soft, ill-defined, dark coagulum; that of the son, one layer, and firmer, of the usual yellow colour. A small, dark, soft coagulum was found likewise in the left auricle. In the remaining vessels it was fluid. The lungs of both were greatly collapsed and pale anteriorly, but darkly congested posteriorly, to an extent of at least two-thirds of their substance; crepitous, and the fluid blood flowing freely from them on section, chiefly from the branches of veins of a considerable size. In the father particularly, minute vessels were seen congested anteriorly, between the divisions of the lobuli, which were unusually distinct.

In both, the gall-bladder was distended, and although of a dark green colour externally, the bile seen on the scalpel was of the usual orange tint. The urinary bladder contracted and empty.

The nerves of the abdomen could be examined in the child only, from the quantity of fat with which the mesentery of the father was loaded. Nothing unusual was discovered. The veins of the stomach and mesentery, although they contained some dark blood, were not more engorged than is general in death from other diseases.

As to external appearance:—In both, the colour of the large intestines was not unnatural; but the stomach and duodenum, jejunum, and upper part of the ileum, were of a pink colour, from capillary injection, of a bright colour, beneath the peritoneum. The muscular coat of the stomach of the father was remarkably distinct, and its colour of a pale red. The viscus was greatly distended, and the intestines of both, as well as the stomach of the son, were loaded (in places turgid) with a large quantity of watery fluid; in which floated white particles of a flocculent appearance. In the father, these particles were, in the small intestines, more broken down and mixed with fluid, like flour and water, giving the whole a thicker appearance than in either the stomach and large intestines.

Internally:—In both, the mucous membrane of the stomach, especially its cardiac portion, that of the duodenum, jejunum, and part of the ileum, was injected minutely with blood of a bright colour: the remainder of the small intestines and the colon presented here and there aborescences of injected vessels, which terminated in some places in small patches of capillary turgescence.

In the child, follicles, prominent, and turgid with an opaque secretion, were scattered over the surface of the stomach; most numerous in its pyloric portion, but not to such a remarkable extent as in the former case; they were most distinct in the ileum: the solitary follicles there scattered about the distance of half an inch from each other, were considerably larger than mustard seeds. The aggregate follicles appeared like round, whitish, opaque bodies, set close together, covered with a transparent membrane, tinged with blood, minutely injected vessels ramifying on their interstices.

At the commencement of the duodenum and at the termination of the ileum, these were of larger size; in the former place, a few strings of coagulated blood adhered to their orifice; and, in the latter, they had a greenish semitransparent colour, and were remarkably distended. Those of the colon were of a darker green, and quite transparent, many of them being surrounded with a

faint zone of injected capillaries, and their centre having a dark point. They were most numerous at the orifice, and in the course of the vermiform appendage, which contained a gooseberry-seed and some small portions of feculent matter, the only traces found in the whole alimentary canal*.

The contents of these follicles varied with their colour and opacity. Where opaque, they contained opaque secretion; where transparent, it was likewise transparent, and of great tenacity. The mesenteric glands were generally enlarged, hardened, and pale, as in the former case.

The stomach of the father was injected to a greater extent than that of the child; its mucous membrane along the greater curvature was softened, and could be readily scraped up with the nail. The appearance of engorged follicles was very indistinct, except at the commencement of the duodenum and the termination of the ileum: yet on removing a very tenacious, semitransparent mucus, which adhered to the stomach, as well as to the colon, follicles of the size of a pin's head on the pyloric portion of the stomach, and larger in the colon, were observed, turgid and prominent. The patches in the duodenum and ileum were of great size, but more coalesced, the appearance of each separate gland being less distinct. Their contents could be expressed the same as those of the others.

The pancreas was injected in both cases, and the mucous membrane of the stomach and duodenum appeared rather thickened. The villi, of a reddish colour, were unusually distinct, especially in the duodenum and jejunum.

Such being the appearances in these, the only cases which we have yet had an opportunity of examining, for the present we must conclude that the part of the secretion so peculiarly characteristic of cholera is formed in the follicles of the mucous membrane; and that the effect of the diseased action of cholera was modified by the previous constitutional state of the patient, occasioning the remarkable turgescence of these follicles in the cases of these children.

The next question is, of what part is the limpid fluid a production? Of the action of the rest of the mucous membrane, whether entirely devoted to absorption, or partly to secretion, we are not sufficiently informed. The case of the first child, in which there might be said to exist not a single follicle over the whole extent of the stomach and bowels unfilled with this secretion, seems to prove that it must have had some other source; but on the other hand, in expressing the contents of some of the follicles from John Page, particularly those of the colon, we thought that we observed a small quantity of thinner fluid exude, as well as the tenacious secretion,—but on this point we must wait for further opportunity of observation. At present it appears most rational to attribute the secretion of the limpid fluid to the rest of the mucous surface. Whether any part of it is formed by the pancreas, we have not yet had opportunity to determine.

It is necessary to be thus (almost trivially) minute and particular, in tracing the parts by which the secretions in question are produced, that we may advance on a sure foundation to consider the more important question—what is the nature of the diseased action which produces them?

It is certain that in two of these cases it was accompanied by injection of the capillaries; and the pallor of the membrane after death in some cases, does not prove that augmented determination of blood has not at any period formed part of the morbid action, and that it is accompanied in all cases with a remarkable degree of exaltation in the sensibility of the mucous, and irritability of the muscular, tunics; but further than this the present state of physiology and pathology does not permit us to advance. We know little more of the nature of inflammation, the simplest of all diseased actions; and how happy should we be if the influence of remedies on this diseased state was ascertained as accurately in the former as in the latter case!

There is indeed one theory which here may demand cursory notice—the supposition that this secretion is the simple consequence of transudation. Secretion being the formation of a substance not previously contained in the

* Here we may take the opportunity of remarking, the enteritis appears to have been often erroneously attributed to portions of extraneous matter accidentally finding their way into the vermiform appendage, for such things are by no means rare in post-mortem examinations, where no disease of the bowels existed.

blood, and transudation the separation of one of its component parts, the argument for this hypothesis is, the asserted identity between these evacuations and the serum of the blood. This we have not yet had leisure to test by experiment; but we may now observe, that healthy mucous differing so little in its chemical characters from the serum of the blood, may in its diseased state put on an appearance not to be distinguished from serum by analysis, and yet be not identically the same, or separated by the simple process of transudation.

Leaving discussions like this for others of more practical importance, we shall enter upon the consideration of the nature of the peculiar affection of the organs of circulation, and of the blood, and their relation to each other, before detailing the effects of remedies on any of the varieties of the disease. This part of our subject must be deferred to another opportunity.

LEONARD LEDBROOK, Surgeon.
JOSIAH ALLSOP, M.D.

Septon, August 1, 1832.

CHOLERA — PRACTICAL REMARKS ON VARIOUS REMEDIES.

(Being a Report transmitted to the Army Medical Board.)

Chief Cholera Hospital,
Grange Gorman Lane,
July 28th, 1832.

As the cases of cholera sent to this hospital are generally in a state of collapse on admission, but few opportunities of treating the premonitory symptoms occur in proportion to the number of cases presenting themselves.

Upon mature reflection, I am of opinion that the usual practice of blood-letting in the *commencement* of the attack is dangerous, and that it should be preceded by some stimulant, otherwise collapse is very likely to be produced. I prefer an emetic of mustard, or common salt, for this purpose; and when the system is sufficiently aroused by such means, together with the use of friction to the extremities, sinapism to the epigastrium, and hot salt to the feet and back, the *cautious* use of the lancet is followed by the best effects, and frequently cuts short the malady. I am further convinced, that whenever the pulse does not improve in strength during the abstraction of blood, it will be decidedly injurious in this disease, although in inflammatory complaints it is desirable to produce an effect of an opposite nature. This practice, followed by the use of calomel in moderate doses, to promote the biliary secretions, appears to be the most rational.

In the cases of collapse the indication seems to be to give an impulse to the arterial system, which I conceive is best effected by producing energetic vomiting. With this view the mustard emetic is given in all cases where re-action has not been already established; and at the same time the external stimulants mentioned above are employed. After the operation of the emetic, the carbonate of ammonia in solution is given in doses of a scruple, at the intervals of a quarter or half an hour, or an hour, according to the intensity of the collapse. Calomel is now used in ten grain doses, the *first* being *sometimes* combined with a grain, or a grain and a half, of opium. This plan is followed up by three grain doses of calomel every second hour, which are persevered in until biliary evacuations are established. Should the ammonia not be rejected, it is continued until re-action takes place, and then omitted. Great circumspection is required in the use of this stimulant, as, if too long persevered in, it will induce cerebral congestion.

When re-action is violent, or the head becomes affected, both local and general bleeding must be employed; but great discrimination is necessary with regard to blood-letting, as it is most useful if judiciously had recourse to, and equally pernicious when improperly used. In fact, extensive experience alone can supply the mature judgment requisite in this case.

When great irritability of the stomach exists, leeches *freely* applied to the epigastrium, and followed by warm fomentations, and sometimes by blister, are highly beneficial. The effervescing mixture also, with tincture of opium, is frequently successful in checking vomiting. Enemata of mustard also are sometimes useful in arresting it, in addition to their stimulating effects in collapse.

Opium is rarely used in this hospital, and never in large quantity, in consequence of its increasing the tendency to determination to the head, and coma, so general in this disease.

With regard to the drink of the pa-

tients during collapse, I may observe that this is regulated by their feelings. Bottled ale, cider, soda water, wine, and brandy and water, are all found beneficial in different cases. The ale is highly useful, frequently allaying the vomiting when nothing else can be retained. Soda water also, as well as spring water, is often successful; the latter is constantly preferred to all other drinks.

In the consecutive fever, the chief indication is that of moderating the violence of re-action, and obviating the tendency to cerebral affection invariably accompanying this disease.

The saline treatment has been fully tried, and found to be unsuccessful.

Two cases have been treated with hot water, *strictly* according to the plan recommended by Mr. Smith, of the Coldstream Guards, both of which have been unfortunate.

The injection of the veins with saline fluid, as practised in Edinburgh, has also been tried in two cases, and failed.

It is worthy of remark, that all the cases of fever lately occurring appear to be nearly allied to cholera, and have a strong tendency to run into collapse, a fact which it is *very important* to bear in mind in the treatment of such cases.

It is hoped that the great pressure of business will furnish sufficient excuse for these hurried and imperfect remarks.

Owen Lindsey, M.D.

CHOLERA — PROHIBITION OF FLUIDS—RECOVERY.

To the Editors of the London Medical Gazette.

New-Street, Bishopsgate,
August 3, 1832.

Gentlemen,

If you think the following statement of cases which have come under my care worthy of your notice, you will oblige me by inserting them in your useful publication.

I am, Gentlemen,
Your obedient servant,
John Wilson.

Case I.—John Bramley, Angel-Alley, Bishopsgate-Street. July 6th, 4 p.m. I found this man labouring under a violent attack of spasmodic cholera: no pulse at the radius, axilla, carotids, or even at the heart; incessant vomiting and purging of a thin gruel-like fluid; universal cramps; tossing about of the body and limbs; extreme coldness. It was evident that this man, if not speedily relieved, must die. All possible external means were employed to restore warmth and circulation—fomentations, frictions, &c.—without effect.

℞ Pulv. Opii, gr. ij.; Potass. Carbon. ʒss.; Pulv. Zing. ℈ss. statim. sum. Vomited.

7 p.m.—℞ Liq. Potass, ʒiij.; Aq. ℥iv. M. A dessert spoonful to be taken every ten minutes, most strictly prohibiting drink of any kind excepting the medicine.

Hor. 8va et 10ma p.m.—Contin. Mist.—Pil. ex Opii, gr. ⅔; Sapon. gr. ij. 2dâ q. h. s.

July 7th.—After three in the morning the cramps had lessened.

11 a.m.—Complained that the medicine had made him sick, and whilst I was with him he vomited nearly a pint of fluid. Upon questioning him, he confessed that he had drank water, contrary to my express orders. I now laboured to convince him that his life depended on his own conduct and command of himself.

Cont. Mist.—Pil. Anod.; Pil. ex Pulv. Rh. Sap. a. gr. ij. intervallis pil. anod. sum.

8th.—Vomiting diminished; pulse felt.

Cont. Mist.—Pil. Anod. et Pil. ex Rhæo.

9th.—Vomiting ceased; pulse and warmth restored.

Mist. ex Pulv. Rhæi.—Sod. Subcarb. a. ʒss.; Aq. Cinnam. ℥j.; Aq. ℥iij. ¼ 4tâ q. h. s.; Pil. ex P. Jalap. Aloes, Sapon. a. p. æ. 4tâ q. h. s.

10th.—Feculent stools.

Mist. ex P. Rh. ℈i.; Magn. Carb. a. ℈ij.; Aq. ℥vi. ¼ 4tâ q. h. s.

11th.—Repet. Mist.

8 p.m.—No urine passed until this evening, when, after experiencing some uneasiness from the desire to void it, he passed as much as would fill two or three pots, of turbid dark-coloured urine.

12th.—Continuing to improve.

13th.—Mist. ex Pulv. Rhæi. Sod. Subc. a. ℈ij.; Aq. Pim. ℥j.; Aq. ℥iiij.

From the continuing this medicine until the 17th he became perfectly convalescent, and is now able to work at his business (silk weaving).

Case II. — —— Duncan, Smith's Buildings, Angel-Alley.

July 12th.—Symptoms nearly alike to the former. A dose of opiate powder administered, and vomited.

Mist. ex Liq. Potass, ut antea eodem modo sum. No drink allowed.

13th.—Vomiting ceased.

Mist. ex P. Rh. Sod. Subc. a. ℈ij.; Aq. Pim. et Aq.

14th.—Repet. Mist.

15th.—Convalescent.

Case III. — —— Fry, Bishopsgate Workhouse.

July 12th.—Symptoms similar to the former. No pulse; cramps; universal coldness; thirst.

Mist. ex Liq. Potass. ut antea eodem modosum.

13th.—Mist. ex P. Rh. Sod. Subc a. ℈ij.

14th.—Better.

Repet. Mist.

15th.—Convalescent.

In these two cases, no urine was passed during the severity of their suffering.

Case IV.—Elizabeth Myers, Half-moon-Alley, Bishopsgate-Street.

July 18th, 7 p.m.—Seized with violent purging and vomiting of thin gruel-like fluid. Extreme pain; cramps all over her body and limbs; no pulse; universal death-like coldness. Her feet and legs were put into hot water.

Mist. ex Liq. Potass et Aq. to be taken as above.

9 to 10 p.m.—Contin. Mist.

Pulsation felt.

19th.—Better, but vomiting produced by taking a cup of tea. Strict abstinence from drink enjoined.

Rep. Mist.

20th.—Still better.

Rep. Mist.

21st.—Improving.

Rep. Mist. Pil. ex P. Rh. Zing. a. gr. ij. 2dâ q. h. s.

22d.—Still improving.

Mist. ex Sod. Subc. Magn. Carbon. a. ʒss. Potass. Sulph. ʒiss. Aq. ℥v. ¼ 4tâ q. h. s.

23d.—Rep. Mist.

24th.—Convalescent.

I am satisfied that much of the benefit obtained by these patients arose from their being totally prohibited from drinking water, or any other fluid. Their suffering from thirst was dreadful, and it was grievous to hear the woman, Myers, when she did in some degree recover her voice, cry out feebly for *a drop of water—a drop of water!*—but I repeat that I am satisfied, that had she been indulged in her wish, she would have died.

Within the same period I have had three other cases—one male and two female; all old persons. In all of these the symptoms were relieved and removed by the above treatment, but they afterwards sunk, from previous disease brought on by excess of intemperance. I have had also a great number of cases of colic, or bilious vomiting and purging, similar to those which occurred last year, but which have all yielded to the ordinary remedies.

PATHOLOGY AND TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

After all that has been said and written respecting the nature of the collapse which constitutes the pathognomic character of Asiatic cholera, we are as far as ever from any satisfactory conclusion. The treatment, whatever be its success, is consequently empirical, and must remain so until we are in possession of such data as will furnish us with means of explaining the rationale of the symptoms.

The suspension of the circulation has been attributed to a direct depression of the cerebral energy—to the want of those changes which the blood undergoes in the bronchiæ, and which fit it for the purposes of life; either proceeding from a want of nervous power, or from some impediment in the pulmonary system itself. In all diseases affecting the vascular and cerebral systems, when the chain is once formed, their operation being reciprocal, it is impossible to say which forms the first link in the morbid cycle; but this we do know, that in cholera the blood does not undergo the necessary change in the respiratory organ, and therefore it not only cannot supply the cerebral system, but it is incapable of even exciting the heart's action. The consequence

is a state of collapse; and this I am induced to explain upon a principle which I do not think has been as yet fairly stated.

From the excessive quantity of fluid matter discharged from the whole surface of the alimentary canal, there would appear to be a completely inverted action of the lymphatic vessels and of the venous capillaries; but we all know that the volume of blood may be diminished to a still larger amount, without injuring the vital functions. The whole train of symptoms of cholera induces me, therefore, to believe that the collapse is not produced by the absolute loss sustained; but that, from the peculiar nature of the discharge, the relative proportions of the constituent parts of what remains, becomes wholly unfitted for the purposes of life. The serum being almost wholly abstracted, what remains becomes unfit for circulation, and loses all arterial character, so that it is incapable of stimulating the heart, and of supplying the cerebral system,—consequently all secretion is at an end: I am therefore led to think, that the affection of the stomach and intestines will admit of direct efforts being made to close the mouths of the vessels pouring out the more watery constituents of the blood.

Against this view it will be said, that the symptoms of collapse have occurred without any very copious discharge either by stool or by vomiting. Nevertheless it will in such cases be found, that the previous diarrhœa has been of considerable duration; and we are farther to consider, that the blood in some individuals will bear a diminution of its watery parts with more impunity than in others. In inflammatory action, and in the plethoric and robust, we endeavour to increase the relative quantity of the thinner parts of blood; but in the aged and infirm, and these are precisely the cases where the discharge is most speedily followed by collapse, the reverse holds good.

What treatment others may be induced to adopt, should this view appear worthy of attention, I leave to their judgment. The indications which I keep in view are—

1st, The diminution of the entire volume of the blood, by venesection.

2d, The saline treatment, as assisting to restore the arterial character of the blood.

3d, The treatment of the relaxed state of the alimentary canal as a local affection, by astringents, both vegetable and mineral, as catechu or kino, combined with aromatic confection; weak doses of a solution of sulphate of copper, nitric acid, acetate of lead, the pulv. cretæ comp., and ice in very small quantities.

4th, The use of external heat, and other stimulating applications.

JOHN VETCH, M.D.

Charter-House, Aug. 4, 1832.

P.S.—I may add, that the best effects have followed the use of kino before collapse had taken place, but where the rice-water stools were very copious.

CASE OF

MALIGNANT CHOLERA.

Injection of Saline Solution into the Veins—Accouchement on the following day—Ulceration of the Cornea—Recovery.

To the Editor of the London Medical Gazette.

SIR,

I OUGHT to have sent you this very interesting case for insertion in the Medical Gazette some time ago, when suggested to me by Dr. Abercrombie, of Edinburgh, to do so; but the unceasing labour arising from cholera has prevented me.

Your obedient servant,

DAVID C. CARRUTHERS, M.D.

Ordinary Physician to the Dundee Cholera Hospital, Member of the Royal Medical and Royal Physical Societies of Edinburgh.

Mary Cunningham, æt. 36, residing in King-Street, was admitted into the Cholera Hospital along with her husband William Cunningham, about half-past six in the morning of the 20th June, 1832.

She was in a very hopeless condition; her features were collapsed, and her eyes sunk, and surrounded by livid areolæ; her hands and feet were shrivelled and cold; the pulse at the wrist was imperceptible, except a slight vibration at intervals; tongue cool, and of a dirty white appearance; violent and constant pain at the epigastrium; laborious breathing; severe cramps in

both limbs; great prostration of strength; feeble jactitations of her body, and continual tossing of her arms. The profuse watery purging had ceased an hour before, but the vomiting continued unabated.

She was seized rather suddenly about three o'clock in the afternoon of the 19th, with violent purging and vomiting of a gruelly-like fluid, soon followed by agonizing cramps in her limbs and other parts of her body. About two o'clock A.M. a medical gentleman was sent for, who prescribed medicines, frictions, sinapisms, &c. but considered both her and her husband too far gone for removal to the hospital. His prescriptions were not followed up, and her symptoms continued with unabated violence throughout the night; and for which she took only frequent and copious draughts of cold water. Early in the morning the neighbours became alarmed; and their landlord came to the hospital, and insisted on having them removed; he was deaf to every reasonable remonstrance, and allowed it was the same to him whether they should die on the way to the hospital or not, provided they did not die on his property. They were admitted, and Mary Cunningham was in the condition already described.

Treatment.—A large sinapism, made with equal parts of flour of mustard, oatmeal, and boiling vinegar, with a small quantity of aqua ammoniæ, was applied over the epigastrium, and along the course of the spine; warm sand bags were placed at her feet, and her limbs constantly rubbed with the ordinary stimulating embrocation, spread on coarse worsted gloves.

℞ Ol. Palid.; Ol. Terebinth.; Aq. Ammon. aa. ℥j.; Pulv. Lyttæ ʒj. M.

Her abdomen was rubbed with the anodyne liniment, and tightly wrapt in a broad flannel bandage.

℞ Calomel grs. v.; Opii gr. iss. M. ft. Pil. stat. sumend.

Let her be kept quiet in the horizontal position, and let her refrain from drinking as far as possible.

Noon.—The sinapisms were removed about eight A.M.; the surface of her body is now covered with the cold clammy exudation, which gives out a peculiar odour.

8 o'clock P.M.—On consultation it was resolved to try the transfusion, and I attempted to inject into a vein at the bend of the arm a solution of the muriate and carbonate of soda, by means of the common stomach-pump, with a pipe belonging to the apparatus for injecting subjects affixed to the extremity of its flexible tube; but I could not succeed, owing to the unmanageable form of such apparatus.

At half-past ten P.M. she remained much the same, and I left by the mail for Perth, in order, if possible, to obtain an apparatus.

Midnight.—Getting very low.

June 21st, 1 o'clock A.M.—Appears to be sinking fast.

6 o'clock A.M.—Continues to get lower, and to every appearance will not survive much longer.

10 o'clock A.M.—I returned from Perth, bringing with me a very excellent set of transfusion instruments, and found our patient still in life, but in the following condition:—She lay motionless on her back; her pulse was imperceptible; respiration slow and heavy; eyes sunk, turned upwards, and the eyelids half-closed and surrounded by livid areolæ; mouth half open; tongue cold, and still of a dirty white appearance; breath chill; face collapsed and dusky coloured; the surface of the body cold and clammy; hands cold, livid, and sodden, as if they had been soaked in water for a length of time; feet cold and livid; voice low and husky.

I made an incision alongside the median basilic vein, introduced a probe underneath it, and made an aperture into the vein itself sufficient to admit the tube of the instrument; and injected, in the space of half an hour, the following solution:—

℞ Mur. Sodæ, ʒj.; Carb. Sodæ, ℈j.; Aq. Pluv. Calid. ℔iij. Solve.

The rain-water had passed through a filtering-stone, and was heated to 212°; the salts were then dissolved in it, and the solution filtered and allowed to cool down to 110°. Towards the conclusion of the injecting, the pulse began to return, the eye-balls to lose their death-like appearance, the areolæ to get of a lighter shade; her whole countenance to brighten up, her tongue to get warmer, and respiration quicker; the surface of her body gradually became warmer; her hands lost somewhat of their livid and sodden appearance, and

grew warmer, as also did her feet; and her voice became more natural, and in a short time she spoke and expressed herself as being easier. She continued in this flattering state for nearly three hours, when the effects began to wear off, and the symptoms of collapse rapidly to return. The probe was again passed under the vein, the tube introduced into the former opening, and other 5lb. of the solution were slowly injected. During the injecting of the fluid her countenance again brightened up, and she rallied with amazing rapidity. After three pounds were injected she complained of pain in the middle of her chest, and respiration became embarrassed. The operation was suspended for a few minutes; her breathing became calmer, the pain wore away, and the remaining two pounds were injected without causing any uneasiness.

Her countenance had now lost the choleric aspect; eyes suffused; tongue warm and moist; respiration natural; pulse 90, and good; hands and feet, and the whole surface of the body, warm and dry. Her voice became of a natural tone, she spoke distinctly, and she felt very easy and comfortable.

Four o'clock P.M.—Vomiting and purging of gruelly fluid.

Midnight.—Vomiting and purging less severe.

June 22d, 3 o'clock A.M.—Passed urine copiously, which she has not before done since the attack.

8 o'clock A.M.—Vomiting gone, but purging still continues.

℞ Ext. Catech. grs. xv.; Tinct. Opii, ʒss. Sol. Ichth. ℔ss. M. pro Enemate.

6 o'clock P.M.—Has passed more urine. She has taken a little small-beer, for which she longed very much. She has had several loose stools of yellow tinge.

Noon.—Complains of pains in her loins and abdomen.

1 o'clock P.M.—She continues to complain of the pains, which return at intervals. On first examining her abdomen I thought she had got ascites, but found it to be the uterus; and on inquiry, discovered she was six months gone with child, and I found the os uteri expanded to the size of a sixpenny-piece.

3 o'clock P.M.—Labour has made rapid progress: at a quarter past three I delivered her safely of a dead girl. The placenta had a clayish appearance, and contained dark viscid blood; the child was of a very livid colour. She desired a little whisky, which was given her.

4 o'clock P.M.—She continues to do well.

June 24th, 6 o'clock A.M.—She vomits almost every thing she partakes of. Her left eye, ever since the first injection, has been very much inflamed, and there is now a small ulceration on the cornea, a little below the centre. She seems rather comatose.

Let a large blister be applied behind the left ear.

25th, 4 o'clock A.M.—Has been very restless, and complained bitterly on the pain of the blister, which was removed, and has acted pretty well.

10 o'clock A.M.—Has slept since the blister was taken off, but now complains of severe pain in her chest, directly behind the sternum.

Noon.—The pain in her chest continues, and she is now troubled with a cough, and she spits a thick purulent matter, and vomits frequently small quantities of a very green fluid.

Midnight.—The pain is easier, and she has turned very peevish—quite the reverse of what she has hitherto been.

26th, Noon.—Has slept a little this morning; her breathing is now natural, and the comatose symptoms have entirely disappeared since the application of the blister. She has had chicken-tea for dinner, with a little port wine after it.

Midnight.—She has had no motion in her bowels since the morning of the 24th. Let her have a domestic enema.

27th, 4 o'clock A.M.—She has slept none all night, and has been very uneasy concerning her bed, which she fancied was about to be heated by steam, which she said she had observed filling the ward during the day. At last we succeeded in convincing her that it had been a little smoke that had deceived her.

10 A.M.—Has taken her breakfast heartily, and expresses herself greatly better: she got out of bed in search of cold water to wash her mouth. She takes little drink of any kind, knowing well the bad effects of it; and she says the great thirst has abated considerably since she ceased to gratify it.

28th.—She was this day removed to the convalescent ward: she still vomits

a little greenish fluid, and spits the thick purulent matter.

29th.—Feels herself gaining strength daily, and she sits up in her bed and converses cheerfully with the other patients. She recollects perfectly well of the second injection, or, as she terms it, "the pumping of her veins."

30th.—Vomiting is less frequent, but the spitting continues much the same. Her breast has been rubbed for several nights with the tartar emetic ointment.

1st July.—Vomiting has ceased, and the spitting is less troublesome. She now sits up in bed the greater part of the day.

2d.—The spitting has nearly ceased.

3d.—She is no longer teased with the cough, and the spitting has ceased completely. She has been allowed to get out of bed and walk about the ward.

4th.—She feels so well in every respect, that we consider it unnecessary to report her case any longer.

24th.—Is now quite well and of a healthy appearance, and has been reported to the Board of Health as completely cured.

Since the above successful case, I have repeated transfusion in four other cases, with temporary relief only.

The operation is simple, and easily performed. After having introduced the tube *fairly* into the vein, the piston must be worked with very great caution at first, else, from the collapsed state of the vein, the fluid will regurgitate into the loose cellular substance. Throughout the whole operation, the careful working of the piston is of paramount importance: the body of the syringe is to be kept perpendicular to the bason containing the solution, and the piston-rod is to be moved slowly up and down to the extent of one inch only. If longer strokes are made, the fluid will be thrown in with too great rapidity; and the patients, in several instances, have died within a very short time after the operation.

Before concluding, I beg to direct the attention of your readers to the remarkable disparity of the pulses at the wrist in cholera morbus. If in the left wrist the pulse is barely perceptible, in the right it cannot be felt; whilst if it is at all perceptible in the right wrist, in the left it is much more so: indeed, this difference has been so uniform in a very great number of cases, that I consider it one of the characteristic symptoms of this disease.

Dundee Cholera Hospital,
July 30, 1832.

MORTALITY FROM CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

In the sixth volume of the London Medical Gazette (page 691), I submitted to your readers an analysis of the London Bills of Mortality for 1829; wherein it appeared that there died in the metropolis that year, of what are commonly called *bowel complaints*, only forty-one persons—viz.:

Of Diarrhœa	31
Of Dysentery	6
Of Flux	4
Total in 1829	41

In the year 1830, the number of deaths by the same disorders are stated as follows:—

By Diarrhœa	19
By Dysentery	24
By Flux	10
Total in 1830	53

The term *cholera morbus* appears in these bills, for the first time, on Tuesday, 21st December, 1830; and the first death by such a disease is recorded in the following week. The total number of deaths by bowel complaints, in the year ending December 20th, 1831, is thus given:—

By Cholera Morbus	48
By Diarrhœa	33
By Dysentery	11
Total in 1831	92

Of the total number of deaths by cholera morbus in the year 1831 (48), there took place,

Prior to August		12
In the autumnal months,		
August	10	33
September	13	
October	10	
Subsequent to October		3
Total in 1831		48

The following table exhibits the deaths by cholera morbus in the first six months of the present year:—

In January	0
February	25
March	250
April	273
May	52
June	67
Total in the first six months of 1832	667

The next table gives the deaths by cholera morbus in the month of July, as registered in the Bills of Mortality. The result will probably prove as great a surprise to your readers as it has done to me:—

In the week ending July 3, 1832,	55
——————10, ...	108
——————17, ...	158
——————24, ...	380
——————31, ...	305
	1006

The concluding table gives the total number of deaths by bowel complaints, in the first seven months of 1832:—

By Cholera Morbus	1673
By Diarrhœa	25
By Dysentery	12
Total in the first seven months of 1832	1710

These statements afford, I think, a pretty satisfactory proof that the precautions of government have not been unnecessary, and that the anxiety of the public mind has not overstepped the bounds of reason so far as some *philosophers* would induce us to think.

I remain, sir,
Your very obedient servant,
GEORGE GREGORY.

31, Weymouth-Street,
August 6, 1832.

CHOLERA—CASES IN WHICH THE SALINE TREATMENT FAILED.

[THE following cases, in which a trial was made of the saline treatment, occurred in an institution in London—the name of which we decline giving, lest any unnecessary alarm should be excited; but the details have been authenticated by the practitioner in charge. —ED. GAZ.]

June 17th.—Jane Cox, aged 35, admitted at 4 P.M. with all the formidable symptoms of cholera (had been ill about half an hour when first seen); coldness of extremities; violent cramps; pulseless; vomiting, and stools watery and colourless, ejected with great force, with violent burning sensation at scrob. cord. Ordered warm bed; mustard emetic, which seemed to give temporary relief to pain; after which gave muriate of soda, carbonate of ditto, of each 3ss. c. oxym. pot. gr. v. every half hour.

6 P.M.—Has taken three doses; no re-action.

10 P.M.—Eleven doses; much the same; ordered frictions with Ol. Tereb. and sinapisms; warm air bath was used, and produced some slight warmth of surface, but pulse still imperceptible, and no diminution in symptoms.

12.—Continues the same.

Cont. Pulv. Sal.

18th, 4 A.M.—Rapidly sinking; no re-action whatever has taken place.

Cont. the powders.

8 A.M.—In articulo mortis. Expired at half-past 8 A.M. Has taken about twenty-five powders, but without any marked benefit.

There was considerable blueness in this case; and the pulse never once indicated any sign of re-action.

Mary Hughes, aged 45, admitted June 22d, 4 P.M. Collapse in this case was very great; pulseless; insensible; coldness and clamminess of skin; vomiting, and dejections colourless; blueness of extremities, and violent cramps. Gave the mustard emetic, which produced some slight amelioration of symptoms, but very transient. At 5, ordered the powders consisting of the muriates and carbonates of soda, with the oxymuriate of potass, every half hour, in half a tumbler of cold water, together with sinapisms and frictions with Ol. Tereb. Although the powders were regularly given as directed, not the slightest amendment took place, and she expired at 8 A.M. of 23d.

The vomiting and purging in this case was almost without intermission. The pulse never rose, but cramps less severe toward the latter part.

Anne Jemmerson, aged 30, admitted June 23d, 8 P.M, of previous ill health,

with all the formidable symptoms of the preceding cases, but no blueness; cramps, vomiting, and purging, the same; pulse imperceptible.

℞ Sod. Mur. Sod. Carb. aa. ʒss. Pot. Oxym. gr. vij. omni semi horâ. Sinapisms and frictions with Ol. Tereb.

10 P.M.—Apparently some slight improvement; skin rather warmer, and less cramps; dejections and vomiting the same.

Cont. Pulv.

12 Noon.—Much the same.

Cont. Pulv.

24th, 2 A.M.—Skin becoming cold again; evident relapse; restless, and throws herself about continually in the bed. No pulse; vomiting, cramps, and dejections the same.

Cont. Pulv.

4 A.M.—Her cries are, at times, piercing and distressing in the extreme; not, as she says, from pain, but the violent spasms of her legs. Still continues to vomit, but purging less.

8 A.M.—Evidently sinking; restless, and breathing becoming shorter. Vomiting less; still no pulse; spasms still violent, but not frequent.

Cont. Pulv. ut antea.

10 A.M.—Refuses to take any thing, either of medicine or nutriment. No spasms or vomiting, but evidently sinking fast. Restless, throwing her arms out of bed, and moving from one side to the other frequently.

11 A.M.—Evidently expiring. Half-past 12, expired.

Some slight re-action appeared to take place about 10 P.M. 23d, but very transient. The pulse at no time was perceptible, and the tongue throughout exhibited a dark mottled appearance. The saline treatment in this case, as in the preceding ones, apparently produced no permanent effect.

Jemima Swan, aged 40, admitted June 29th, 10 P.M. Vomiting, purging, cramps, &c. Collapse greater than in any of the preceding cases. Had been seen, an hour previous, in apparently a good state of health: the attack was quite sudden. Spasms violent, and vomiting almost incessant, and ejected with great force. Pulse altogether imperceptible, and the skin of a death-like coldness; tongue of a darkish blue mottled appearance, and breath cold; breathing hurried. A mustard and salt emetic was administered, which in some measure relieved the pain and vomiting, but cramps still continue.

℞ Mur. Sod. ʒss.; Carb. Sod. ʒss.; Oxym. Pot. gr. vij. ft. Pulv. omni semi-horâ sumend. Injections of three pints of water, at 114° Fahr. with a wine-glassful of brandy every two hours; with sinapisms and frictions with Ol. Tereb.

12 o'clock.—Has continued the powders, and injection was repeated. Symptoms, if any thing, aggravated.

29th, 1 A.M.—Cramps without intermission, and cries out violently in the most piercing shrieks; vomiting the powders whenever given, and injections return. No pulse; eyes sunk deep, and surrounded by a livid areola.

Cont. Pulv.

2 A.M.—No amendment; every symptom apparently aggravated; pulseless, and of a death-like coldness.

Cont. Pulv. ut antea.

3 A.M.—Sinking fast; throws herself about the bed; distressing anxiety of countenance; voice sunk to a whisper; skin wet and cold; no pulse.

Cont. Pulv. ut antea.

4 A.M.—Rapidly sinking. Voice gone. No spasm or vomiting.

Expired at twenty minutes to 5 A.M. June 30th.

Not the slightest re-action whatever took place in this case. The powders were regularly given every half hour, but as instantly rejected with considerable force. The pulse was never at any one time perceptible, scarcely even in the axilla; and the failure of the above treatment in this and the preceding cases (the only ones in which it was ever tried) caused us to relinquish the plan altogether.

SALINE TREATMENT IN CHOLERA.

Audi alteram partem.

To the Editor of the London Medical Gazette.

SIR,

ON opening your Gazette, No. 16, July 21, 1832, page 512, I read "Mr. Richardson on the Saline Treatment of Cholera."

This gentleman gives you two cases,

"in one of which," (the lady on Ludgate-Hill) "the saline treatment," he says, "was employed without the slightest benefit; in the other opium was given, *combined with salines*, and with the best effects."

Forbearing to animadvert on the want of courtesy due to the family of the first patient, (for I am persuaded none of them were aware that the case of their mother was laid before the public till it fell under my observation) I trust a regard to truth, by which I mean an explicit and correct statement of facts, will ensure this communication a place in your pages.

When the servant of my much-esteemed and dear departed relative came in great alarm to me, at the House of Correction, between nine and ten o'clock on Monday morning, July 9th last, desiring that I would call on the family immediately, and bring with me some of our medical attendants, for her mistress was very ill, and they were afraid she was seized with cholera, I dispatched a messenger instantly to Dr. Stevens, and was myself soon at their dwelling, with a view to sooth their anguish, and as much as possible to dissipate their fears.

I beg here, once for all, unequivocally to declare, that what observations soever I may make in this letter, I have no intention of uttering even a syllable that may wound Mr. Richardson's feelings: on the contrary, I am constrained in justice to him to bear this testimony to his conduct,—that his anxiety for the welfare and recovery of his patient was, I believe, equal to my own. His attendance nearly through the whole of the day; his watching from hour to hour, that every favourable opportunity might be improved, was conduct creditable to the humanity of his character, and honourable to his profession. It is the unfavourable aspect which a certain mode of treatment—the subject matter of his letter, assumes, of which I complain; and knowing nothing of medical science, it is quite possible, that in correcting what I conceive to be mistakes—want of care and due consideration—whether what he publishes may tend to do good or evil—I may fall into error equally as censurable as I consider his to be.

It is on public grounds alone that I step out of my obscurity to venture an opinion founded on facts.

"If I had been called in sooner," says Mr. Richardson, "I have *no doubt* but the disease might have been cured without the slightest difficulty." I could have said the same, at ten o'clock, while the patient was alive; but after death I could say no such thing, as it involves a question in theology of more difficulty in the solution than any in the whole circle of medical science.

"All is in His hands whose praise I seek."

Having seen hundreds of cases of cholera, in all its stages—incipient and malignant, collapsed and relapsed—*many* within the walls of the House of Correction for the County of Middlesex, under saline treatment, with few exceptions, *surprisingly recovering and recovered*; and others, under other treatment, out of that house, in most instances *falling victims to the disease*, I was anxious to know the treatment under which my relative had been put since six in the morning, up to that hour, ten o'clock. Mr. Richardson was not at that moment present, so that this I could not learn. I ordered the saline ingredients to be procured, and hastened to his dwelling. I was there informed that he was attending his patients. Mr. Wakefield's residence, No. 1, Lansdowne-Place, was my next point. I met that gentleman on the way, and returned with him to Ludgate-Hill. We arrived about eleven o'clock. Mr. Richardson was also in attendance nearly at the same time. I then heard nothing of salines having been administered, but opium only to the amount of between sixty and seventy drops, *viz.* of laudanum.

Mr. Richardson writes thus to inform the public, that "at six o'clock the patient got out of bed, and was so exhausted that her daughters were obliged to lift her in again. Up to this period," (*i. e.* six o'clock, I presume,) "nothing had been done for her relief. I immediately prescribed saline draughts with opium, &c. Mr. Wakefield stated, that, at the Prison, no opium was administered in this disease; no calomel; no stimulants. They trusted to salines, and to salines alone."

The expression, "salines with opium," would lead to this conclusion, that salines had been administered with opium, from the time that Mr. Richardson first visited his patient,—"*immediately*" is the term he uses; immediately he prescribed, he prescribed saline draughts with opium. If this be not the meaning, the statement is calculated to mislead his reader, except by salines he means something else than the saline powders used in the cases of cholera, in the House of Correction, Cold-Bath-Fields; *viz.*

Carbonate of Soda,
Muriate of Soda, and
Oxymuriate of Potass.

I must ingenuously confess, that I heard something said about salt and water administered, but that was after I had caused the ingredients here named to be procured; and *after ten o'clock*; so that, in fact, opium *without* these salines, not *these salines with* opium, had been administered.

Notwithstanding Mr. Richardson's candid acknowledgment, that he is "not an uncompromising advocate for any exclusive plan of treatment;" yet, "he must say, the salines proved utterly useless, in the

present instance." He, as well as myself and the family know, that when the saline powders were first administered, *i. e.* about eleven o'clock, A.M., nothing was retained on the stomach. He himself, indeed, says, "the only fluid the patient was allowed to drink, was a solution of carbonate of soda; and as soon as it was rejected by vomiting, it was repeated:" and my firm persuasion is, that if I had taken from 60 to 80 drops of laudanum (opium it is here called) in the lapse of three or four hours, whether in health or in sickness, that an appetite for either food or medicine would not thereby have been created, but the contrary.

"I have sent you the history of this case," continues he, "in order that the plan adopted in Cold-Bath-Fields may become generally known." What? that salines proved utterly useless! I cannot discover that all the *candour* which, at the first transient view of this case, where "the patient became gradually worse, and died at eight o'clock the same evening," *is clearly manifest*; when it is compared with Case II. where "salines, with the addition of tincture of opium," was "a combination much *more effectual than salines alone.*" That "patient is now" (at the time this gentleman writes) "convalescent."

Besides, that patient, the second case, is said to be "attacked precisely in the same manner as in the first case: when the disease had made an equal progress, I was sent for to his assistance." Thus, a parallel nicely drawn is attempted to be established. But, besides constitutional differences and disparity of age, 54 and 28, may not the parallel in other respects fail? My relative, though apparently "a stout, healthy woman," as she is described, had been ailing for some time past, and had frequent occasions for medical aid. Was this likely to be the case in the other instance? The comparison is evidently calculated to throw discredit on the Cold-Bath-Fields practice by the salines ALONE. If the salines mentioned by Mr. Richardson be not the saline powders used at the prison, but *a common effervescing saline draught*, then there is an end of all candour, plain dealing, and fairness of statement.

"*One fact,*" it has been well said, "*is worth a thousand opinions,*" or speculations, warped by prejudice, or wrought up so as to produce a bias on the minds of others. I would beg with the utmost earnestness—warn with all seriousness—conjure with the deepest solemnity—all men, as they regard their reputation and honour (medical as well as others), when they take upon themselves to instruct the public, especially in matters of life and death, that they lay aside all prejudices, where the delineation of facts is the subject, and take especial care that there be no colouring—no attempts made to give any wrong bias to the mind of the inquirer—no withholding any thing essential—no addition of that which existed not—no use of equivocal expressions; but "the truth, the whole truth, and nothing but the truth."

What are the facts, in the reported cases, under all other modes of treatment of this disease, where the Cold-Bath Fields mode has not been adopted?

I answer, that about one-half of the patients are generally lost: as in Mr. Richardson's cases, one lost—one saved.

In the more favourable cases, one lost—$2\frac{1}{2}$ saved.

In the most favourable cases, one lost—about four saved.

In the reported cases before the public, to speak in round numbers, the average under all modes of treatment has been about one lost—$2\frac{3}{4}$ saved.

Under the saline treatment, as practised in Cold-Bath-Fields, in about 200 cases in one place reported, one lost—about 7 saved.

Having been an eye-witness of the cases which occurred at Cold-Bath Fields Prison, and having had daily opportunities of observing the almost magical effects of the saline treatment, often even *in the worst cases*, I do not hesitate to say, that one lost—thirteen saved.

Should Providence see fit to afflict me (which God forbid) with this *awful malady*, could I doubt a moment under what treatment I should choose to be placed?

I am, sir,
Your faithful and obedient servant,
JOHN OUSBY,
Chaplain of the House of Correction for the County of Middlesex.

Cold-Bath Fields, House of Correction, Middlesex, Aug. 1, 1832.

NECESSITY OF FIXING UPON SOME STANDARD IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

YOUR remarks on the homœopathic treatment of cholera are just, but the facts must be received, like many others, *cum grano salis*. I consider, however, the numerous varieties of treatment now recommended, require a more serious review from your able pen; and you will, perhaps, pardon this effort to direct your attention to the subject from one who has witnessed a great number of cases, without that success which others boast as attendant upon their measures: the consequent impression that

such success has been overrated, has led me to the following conclusions:—

1. That it is essentially necessary medical men should adopt some *standard* whereby they may judge of the intensity of a case of cholera by description, and the effect of treatment; and, since cases can scarcely be called malignant, until the stage of collapse arrive, let *that standard* be, the fact of the absence of pulse at the wrist: and the gentleman who can bring forward the greatest body of evidence of having recovered patients who had *no* pulse at the wrist, *together with* the other frightful symptoms of collapse, will be entitled to the most consideration from his professional brethren and the public.

2. That the much-vaunted success of different practices in this disease will be found to exist in cases described as having the *premonitory symptoms;* that these symptoms being partly owing to the prevailing epidemic, and partly to the season, will yield to almost any measures when combined with *restraint in diet,* whether those measures be anodyne, cretaceous, stimulant, or saline, or, may I not add, homœopathic? except a few aggravated cases, which will run into the stage of collapse, and die.

Trusting the above will be considered deserving a corner in your Gazette,

I am, sir,
Your obedient servant,
An Apothecary.

Lambeth, August 5, 1832.

DOUBLE AND SINGLE VISION.

To the Editor of the London Medical Gazette.

Sir,

In the observations you have made in your number of July 28th, on a paper of mine on Double and Single Vision, published in the third number of the Dublin Medical Journal, there are some points worthy of your reconsideration. You say,—" But we think the explanation defective, if not absolutely incorrect, for there is nothing more certain than that the lines of visible direction do in both cases intersect, and always will, as long as the axes of vision make any definite angle." Now, sir, nothing is more certain than that this assertion of yours is quite incorrect. If both eyes, as in the diagram attached to my paper, be directed to a certain point (*a*), then an object (*c*) beyond that point will have its lines of visible direction in both eyes thus related:—

1. If the point *c* be very close to the point *a*, it will be also of course very near the prolongation of the axis of each eye, and it will be outside each axis, consequently the image of the point *c* will in each eye be inside the optic axis, and very close to it. In this case, the lines of visible direction will meet at a point more distant than *a*.

2. If *c* be now supposed to recede from *a*, then, as at the same time its perpendicular distance from the prolongation of the optic axes must increase, its image in each eye must necessarily move further from the axis, and more inwards, until it arrives at a point (*f*), where the lines of visible direction become parallel: this point may be determined by joining the centres of the eyes, and drawing a diameter in each eye perpendicular to the line of junction.

3. If *c* recedes still further from *a*, its images will fall in each eye further from the axes than the point just mentioned, and consequently the lines of visible direction will now become divergent; as I have represented them in the plate. No demonstration can be clearer, therefore, Mr. Editor, than that which proves your chief assertion to be altogether erroneous. If time permitted, I could show satisfactorily that the first of the three cases above detailed does not take place in the experiment, for the experiment cannot be made with bodies very close to each other.

As Sir David Brewster and all modern optical writers lay it down as a well established principle, that the apparent direction of any object depends entirely upon the direction of the perpendicular to that part of the retina on which the object of that image is painted, it is obvious that the relative direction or position of any two objects must altogether depend on the relative direction of their respective perpendiculars, or lines of visible direction, and can have nothing to do with planes or projections external to the eye, as your hypothesis would require.

With a sincere desire to cultivate the

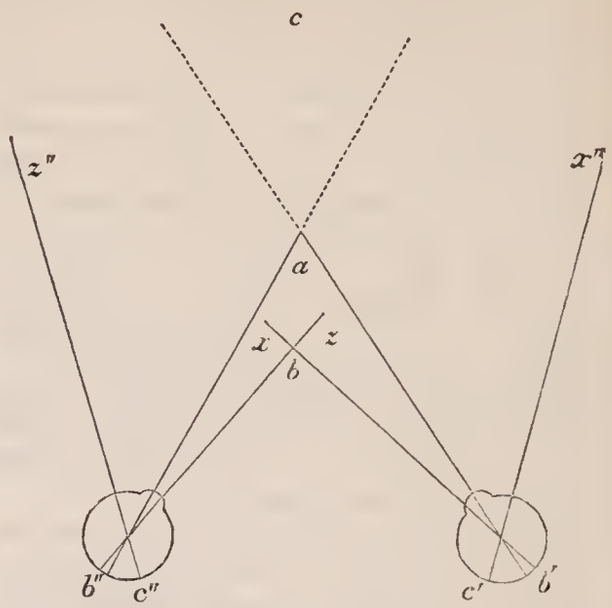
investigation of truth even in matters apparently unimportant, and a deep sense of your anxiety to promote the same object,—I remain, sir,

Yours very truly,
ROBERT J. GRAVES.

9, Harcourt-Street, Dublin,
August 2, 1832.

——

In reply to Dr. Graves, we beg leave to say, that so far from being convinced by his "demonstration," we are less satisfied than ever with the correctness of his explanation. We had our doubts before—when penning our former remarks—where exactly the weak point lay in the Doctor's argument: of the insufficiency of the whole we had no doubt: but now we think we can expedite the matter, and shew, without difficulty, in what the mistake into which Dr. Graves has fallen consists. He assumes that the line of visible direction—or the perpendicular on the retina at the place of the image—is not coincident with a line drawn from the object through the centre of the eye. Now we, with great respect for the opinions of Dr. Graves, affirm that it is. Let a line be drawn from any point in an object, through the pupil and centre of the eye, to the retina—its termination in the retina marks the place of the image of that point. But this line is the course of the middle ray of a pencil of light proceeding from the object; and the object will be seen in the direction of a perpendicular, raised from the retina at the corresponding point of the image. What, then, is the position of the perpendicular? Why, passing through the centre as it must do, it is necessarily coincident with the central ray: the line of visible direction, consequently, is virtually the same as the line drawn from the object through the centre of the eye to the retina. Dr. Graves, however, strange to say, not only assumes that they do not coincide, but that, under certain circumstances, they form a considerable angle; and this is to us the more unaccountable, inasmuch as in his diagram, where the secondary object *(b)* is more near than the primary, *(a)* he makes them coincide, or uses one and the same line for both. For the better understanding of the question at issue, we give a copy of the figure which is attached to Dr. Graves's paper.



But to answer more specifically each of his "relations" stated in the above letter:—

1. Dr. Graves allows that in this case the lines of visible direction will meet (and of course intersect:) we go farther, and maintain, from *our* demonstration just given, that they must meet *in the object*—at the point *c*: there, too, they will intersect, though for the reason which we before pointed out, their respective images will not cross.

2. With regard to the second case, it is clear from the principles just laid down, that as long as *c* is at a finite distance from the place of the primary object, the lines of visible direction will contain an angle, which angle will vanish only when the distance of *c* becomes infinite.

3. It is scarcely necessary to add that this third condition of the question is an impossible case: if the distance of *c* must be infinite in order that the lines of visible direction may be parallel, no case can exist in which those lines should become divergent.

As to our "hypothesis of planes and projections," since Dr. Graves happens to allude to it, we threw it out, we can assure him, without the least intention of display, but simply as the best stop-gap we could think of for the argument sake. Upon further reflection, however, we rather fancy that the said hypothesis is not to be despised, nor to be rashly given up, till we have found a better. What we said about it was to this

effect—that the eye, in viewing a number of objects at once, or embracing a given field of view, sees those objects as if they were set before it, *projected* into one and the same surface, the distance of which surface is determined by the intersection of the axes of vision, or the place of the principal object. This, we submit, is nothing more than what the painter does when he transfers objects from nature to his canvass; he first deliberately makes his air-drawn picture, and then fixes it in his mimic one: and every body, so far as our own experiments warrant us in the assertion, will find this to be the sort of process which he goes through in contemplating any given prospect. But whether our hypothesis be the true one or not, it affords us at least a simple explanation of the phenomena which Dr. Graves, on *his* hypothesis, does not seem to have yet succeeded in explaining.—Ed. Gaz.

ANALYSES & NOTICES OF BOOKS.

" L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

Observations on the Healthy and Diseased Properties of the Blood. By William Stevens, M.D.

The work which we have to introduce to our readers under the above title, is one the subject of which has already excited great interest. To those who do us the honour to peruse this journal, the principal doctrines maintained by the author before us have for some time been familiar; we were the first to lay them prominently before the public, as opening a new and untrodden path in the field both of pathology and therapeutics. That, in the words of the sacred writer, " the life of the flesh is in the blood," may be taken as the summary of Dr. Stevens's views; and when to this we add the proposition, that the blood itself owes its florid colour, and many of its essential properties, to the presence of certain salts, we have given a very " brief chronicle," but a tolerably correct one, of the leading principles it inculcates. Those principles we have so often endeavoured to illustrate, that it would be a work of supererogation to dilate upon them now, as if propounded for the first time to the readers of the Gazette; and we shall therefore do little more than observe, that all the opinions, whether right or wrong, which have been given, either from the pen of Dr. Stevens or by ourselves, as descriptive of his views, are here fully stated and discussed, at great length and with great minuteness. The work is unquestionably one containing very original and important views; but these do not always receive from their author the benefit of so favourable an introduction as a more practised writer might have afforded them. Dr. Stevens's style is desultory, and sometimes requires much attention in its perusal to attain the full meaning of the writer—a circumstance which places him at a disadvantage, as the advocate of opinions many of which are adverse to those generally admitted. Besides the speculative and practical points more immediately connected with medicine, we have what might well have constituted a separate dissertation, on a " latent power of attraction," by which " oxygen lifts up carbonic acid, diffuses it equally in the general atmosphere, and retains it there, in direct opposition to the laws of gravity." This property is regarded as analogous to that discovered in liquids by Dutrochet; and there is obviously a close affinity between it and the views taken by two American writers — Drs. Faust and Mitchell, to whom, however, Dr. Stevens informs us that his views were made known during his last visit to the United States, his own MS. having also been seen by Mr. Travers and others in this country anterior to that time. This doctrine is of great importance, because, if correct, it will afford a new explanation of what takes place in the lungs during respiration. The following extract will enable the reader to form clearer ideas on the subject:—

" A piece of moist bladder was tied firmly over a tumbler of carbonic acid, and when this was exposed to the air, a part of the acid was evidently removed by the oxygen, faster than the air could enter, for the volume of the acid in the tumbler was soon diminished, and the membrane became concave from the atmospheric pressure. But when I reversed this experiment, and tied a piece of white leather (such as apothecaries use for blisters) over a tumbler of com-

mon air, and then immersed it in the carbonic atmosphere of the High Rock, there was soon such an increase of volume in the air contained in the tumbler, that the membrane swelled up, and became so very tense, that I was obliged to remove it from the carbonic atmosphere almost immediately, for fear that the glass might burst in my hand.

"In the first experiment, the decrease of volume in the air contained in the tumbler was not produced by any decrease of temperature; neither did the increase of volume in the second arise from any increase of heat; and to ascertain whether the oxygen, or something else in the air contained in the tumbler, had, or had not, drawn into itself, and that too with force, a part of the carbonic acid, even through the dense membrane, this, when it became tense, was immediately punctured with the point of a lancet, and a part of the air was drawn up from the bottom of the glass by an ear-syringe, with a long pipe. This air was passed through lime-water, which I had ready for the purpose; and a small portion of the contained gas instantly whitened the whole of the water. From this it was evident that oxygen, or something else in the atmospheric air, possesses the power of attracting carbonic acid to itself, even through a membrane much denser than that in the lungs, which separates the air from the blood, or the blood from the air.

"In the above experiments, the oxygen was evidently the agent which acted on the acid, for nitrogen possesses no power of attracting carbonic acid; neither was it the carbonic acid of the air which attracted the acid from the tumbler; for one gas does not attract another of the same kind, consequently carbonic acid can have no power of attracting itself.

"It is also evidently the oxygen which removes the acid from the blood in the process of respiration; for animals cannot live in an atmosphere of either nitrogen or carbonic acid; but besides this, when the quantity of oxygen is considerably increased, the acid is removed in the lungs with great rapidity, and when the blood is healthy, and contains its full proportion of saline matter, there is more oxygen than usual attracted into the circulation, the blood becomes too stimulating for the vascular solids, it then circulates with great force, and becomes arterial even in the veins. On the other hand, when we diminish the quantity of oxygen in common air, the cause of the impurity is not attracted; the acid acts as a poison in the left side of the heart, &c. the animal breathes with difficulty, its blood is not purified, and it soon dies."

We may perhaps take another opportunity of adverting to this subject more at length, as well as to other points in the volume before us, which, meantime, we recommend to the attention of the profession as containing views which, if correct, will have great influence on the practice of the next, if not the present generation; and which, if wrong, cannot be too speedily refuted.

The History of the Contagious Cholera; with Remarks on its Character and Treatment in England. By James Kennedy, M.R.C.S. Third Edition. Moxon.

The former editions of this work were published previously to the arrival of cholera in England; and the author obtained considerable reputation by the well-timed summary which he then presented to the public. The work, however, in its present shape, enlarged to nearly double its size, with, we believe, but a very trifling addition to its cost, is rendered by many degrees more valuable than it was before by the additions which it contains relative to the progress of the epidemic in this country. Mr. Kennedy has had much practical experience of the nature of the disease since its first appearance at Sunderland; and the arrangements made under his superintendence, in the Hetton collieries especially, constitute a very interesting and valuable portion of the volume. Altogether, we think we may pronounce this to be the most complete history of the cholera that we have yet seen. As to the *opinions* which are advocated in the work, we must do the author the justice of allowing him much credit, at least for consistency. When his first edition appeared, we were not a little surprised by the boldness with which the *contagious* character of the epidemic was announced in the title-page: the time, however, which has since elapsed, while it has contributed so much to establish the propriety of retaining

the distinctive epithet, has diminished almost altogether the hazard which in the first instance attended its adoption. The proofs of the contagious nature of cholera are well stated in Mr. Kennedy's pages; and we must not omit to mention that we find in them also a large supply of original reports of cases which were treated in the northern parts of the kingdom under the author's inspection. His account of the leading characteristics of the disease, as witnessed in India, and as it has developed itself here, is concisely and clearly set forth: nor in noticing the modifications of cholera in this country has he omitted to point out the various modifications which the treatment has undergone amongst us. We observe, that in his concluding remarks, Mr. Kennedy canvasses the value of Dr. Stevens's remedial measures: to this part of his volume we may have occasion hereafter to recur: at present we can only extract a short passage:—

"The time in which the *saline remedies*, and those more particularly of the non-purgative character, may prove of peculiar advantage, is probably in the period of transition, when the acute symptoms are passing into the chronic. The returning vitality may then favour their absorption in the stomach; and, as a consequence, the thickened blood may regain the more rapidly its serous consistence. In the fever, moreover, if the secretions are suppressed, and the blood deficient in its saline ingredients, their use may assist in removing these symptoms."

The following passage, also, is worth quoting:—

"In the second stage of acute cholera, the pulse at the wrist is nearly, or it is completely, extinct; the body is deadly cold; the stomach and bowels have lost their irritability; and scarcely any benefit can be expected from the use of the ordinary internal medicine. The propriety of *injecting the veins* may then be entertained; or the patient, under the steady application of external heat, may be intrusted to the chance of the constitutional resources bringing about re-action. Should re-action begin to come round, the medical treatment must be cautiously regulated, both during the transition and the consecutive fever."

MEDICAL GAZETTE.

Saturday, August 11, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

REVIVAL OF THE GIBBET IN LIEU OF DISSECTION.

THE fact of a murderer having within the last week been actually hung in chains—that is to say, the man's dead body having been gibbeted after execution—has excited a mingled feeling of surprise and disgust in the public mind. It was a thing that people were not prepared for—a recurrence to the practices of the olden time anything but favourable to the wisdom of our progenitors: it is felt that we have suddenly retrograded to those famous times when it was deemed expedient to inflict all sorts of barbarous indignities on the corpses of malefactors, not merely *in terrorem* for the living, but for retribution sake—that the evil-doer should have his full measure of punishment even to overflowing.

But there is a small mistake in this view of the matter: we have *not* retrograded—we have only not advanced: we are still in the same savage condition in this country, with regard to penal arrangements, that we were in a hundred or two hundred years ago. The system has never changed—the mode has a little.

It was in the middle of the last century that the act was passed for changing the murderer's sentence from gibbeting to dissection. We can very well fancy how things then stood. People had grown familiar with the sight of the gibbet; and condemned criminals could hear themselves sentenced to that ignominy without a single additional pang. But when, under the Act, the murderer was ordered to be "dissected and ana-

tomized" as a part of his punishment, it was like the application of a new and ingenious torture, and probably for a time it was attended with effects as salutary as the impressions it produced were horrible and revolting. The fowls of the air were to be exchanged for the ruffian anatomist: swinging in the open sky for the private and clandestine mysteries of the dissecting table: and exposure to the gaze of pitying multitudes for the daring scrutiny of the unfeeling dissector. "Anatomizing" was, accordingly, for a time looked upon as a capital mode of punishing with terror: and, as if to secure it expressly for that noble end, our wise legislators persisted as long as possible in prohibiting the practice of anatomy with any other view: the ends of science they would by no means put in competition with the ends of "justice," as they were pleased to call it. They generously overlooked the interests of the medical art, though so intimately connected with the preservation of their own bodily welfare, and that of their families and the community, in order that the provisions of the penal law should be the more efficiently executed.

Unfortunately, however, for our legislators, anatomy, too, was at length found to have "no terror in its threats;" and the increasing intelligence of the age insisted that it should be applied to another purpose. It was loudly demanded that the practice of dissection should be permitted—recognized—legalized, for other uses than the noble one of finishing the law. The demand was reluctantly complied with; but to mark the sense which our modern Solons entertained of the real uses of anatomy, they insisted that, as it was no longer to be employed as a punishment, some equivalent degrading process should be adopted instead. As murderers, therefore, can no longer be consigned to the knife of the anatomist, they must be hung in chains:

> "For buried surely they must not be;
> They are to be left on the triple tree:
> That those who pass along may spy
> Where the murderers' bodies are hanging on high."

If their bones cannot be kept for inspection in a glass case, they may be arrayed at least in a suit of irons, and conspicuously posited.

> "It will be a comfortable sight,
> To see them there by day and by night."

But enough of allusion to this barbarous and demoralizing enactment. It is somewhat beside our province to consider and discuss it in its proper extent; to shew how and in what respect it is as injurious to the country as it is disgraceful; how it tends to generate new crime, and to create a moral condition of things as foul as the atmosphere that immediately surrounds the gibbet. Ours be it simply to mark the *animus* which dictated the change in the murderer's doom; and to shew that however we may boast collectively of having acquired improved views of legislatorial policy within the last hundred years, we are really and truly in as much darkness on certain points, and as practically deficient in wisdom, as any generation that has gone before us within that time. Individuals, no doubt, have been observed from time to time attempting to effect a reform in these matters, but, unfortunately, they have been few in number, and some of them characterized by a degree of eccentricity that neutralized much of their otherwise possible usefulness. From Jeremy Bentham, who long ago announced his intention (recently put in practice) of leaving his body for intellectual profit and example sake, down to the first female who did the same thing, Madame Derubigny Barré (an account of whom we gave last week in

the words of Dr. Macartney), the public have been but too much accustomed to find in the histories of such persons traits of singularity, which have rendered their actions liable to a questionable construction. Old Doctor Monsey, who left his "carcass" to the anatomists, and whose singular letter to that effect may be seen in the Hunterian Museum, is, we think, an instance of a man doing more harm than good by his bequest; for few would be ambitious of being thought to follow in that old gentleman's wake. The case of Dr. Sims, which is recorded in an early volume of the Gazette, had also some curious circumstances about it. He was a very worthy man, but considerably whimsical; and at the commencement of the illness which terminated his life, was convinced that he should recover, if he could but *catch an ague!* He visited the "fens;" but returned to London without having succeeded, complaining "that the country had been spoiled by draining, and that there were no agues to catch." He died at Bath in 1819, after bequeathing his body to Mr. Soden, to be made into a skeleton. The life of Dr. O'Connor, who left his body to Dr. Macartney, and whose remains are curiously preserved in that gentleman's museum, was full of vicissitudes, but more remarkable, as we recollect, for crosses and mischances, than for any peculiar or unusual trains of thinking in the individual. *Et sic de cæteris.* Truth to say, little could at the best be expected from the example of a small number of particular persons. The true source of the remedy—the remedy, we mean, not for a mere supply of bodies, but for the removal of legislatorial and vulgar obstacles—was to be sought in the superior enlightenment of the public mind. That some progress of this sort has been effected, the enactment of the new Anatomy bill is, to a certain extent, an indication; but the stain which that statute bears imprinted upon it, has yet to be expunged. The attention of the public, it is true, has been rather painfully called to it by the late transaction at Durham. Habit, however, can do extraordinary things: and there can be but little doubt, that shocked as people may have been by the occurrence of so absurd and revolting a spectacle occurring in these boasted times of improvement, we shall probably find that they will soon become accustomed to such occurrences, and acquiesce quietly in their continuance, if some effort be not made to keep their better feelings alive. It is almost proverbial the difficulty of rooting out customs that have obtained any standing; but it is the duty of all "public instructors" to see that no detriment of this description shall take place: familiarity with the gibbet must be proscribed.

If any one express surprise that we did not point out this obnoxious clause in the Bill while it was in progress through parliament, we are not unwilling to confess that we let it pass designedly and deliberately, through a firm persuasion, that if any efforts were made directly to remove it, a powerful handle would be given for throwing the Bill out altogether. Prejudices were strong against the whole measure in the highest quarters: and, perhaps, we ought rather to congratulate ourselves that any bill at all in favour of anatomy has been past, than regret that we did not attempt to remove certain of its imperfections during its progress. We have no doubt that had such an attempt been made, it would only have endangered the success of the whole. A more perfect legislative enactment for the furtherance of anatomy has yet to be carried; but, at all events, we hope the time is not distant when the foolish and mischievous practice of the gibbet shall be exploded.

PROGRESS OF CHOLERA IN LONDON.

As we anticipated last week, the rise in the atmospheric temperature was followed by some increase in the number of cases of cholera: we are happy, however, to be able to state that the augmentation was not very considerable, and has again, during the last two days (Thursday and Friday) subsided, to about the measure which we mentioned last week.

PROFESSORSHIP TO THE COLLEGE OF SURGEONS.

Mr. Henry Earle has been appointed Professor of Anatomy and Surgery to the College of Surgeons, in the room of Mr. Guthrie, resigned.

OFFICERS OF THE SOCIETY OF APOTHECARIES.

List of Examiners *for* 1832-33: *chosen the* 31*st July*, 1832.

John Bacot, *Chairman.*

Allen Williams.	Charles Shillito.
Henry Blatch.	T. Lowe Wheeler.
Samuel Merriman.	P. Johnson Hurlock.
Thomas Hardy.	John Hunter, Jun.
John Ridout.	Edward T. Gart.
H. Cromwell Field.	

LIBEL TRIAL.

Laughlin v. *Browne and Sheehan.*

The defendants, who are the Editors of the *Comet* newspaper, published a libel on the plaintiff, Mr. Corry Laughlin, apothecary of the Lock Hospital, and a Director of the Apothecaries' Hall, Dublin. In the libel it was asserted that the plaintiff was incompetently informed on professional matters, and that he had been rejected eight times in endeavouring to pass his examination for a diploma. The trial came on last week, in Dublin. The plaintiff's case was stated with great ability by Mr. Whitside: and the Jury, after a short consultation, found for the Plaintiff.—Damages, 125*l.*; and 6*d.* costs.

TRIAL OF MR. HEATH.

Law as regards Carelessness in Dispensing Medicines.

Mr. Heath, the apprentice of Mr. Moir, chemist, of Brighton, was tried at Lewes last Tuesday, for the manslaughter of the late Captain Burdett, by having sent a phial containing oil of tar in place of a black draught. After the facts of the case (already known to our readers) had been adduced, Mr. Heath stated that the unfortunate event had arisen entirely from accident. At the time Captain Burdett's prescription was brought into the shop, another person came in and requested ten drachms of oil of tar: this he put into a phial, and placed it *unlabelled* on the counter in a stand used for holding medicines already made up. After he had prepared Captain Burdett's prescription, he placed the phial in the same stand; and both liquids being of the same colour, by accident substituted the one phial for the other, that containing oil of tar being sent to Captain Burdett.

The Judge, (Lord Tenterden) in summing up, observed that there was no doubt of the lamentable event having been the result of mistake; but at the same time, if they were of opinion "that the death of Captain Burdett was occasioned by the oil of tar, they must consider whether or not the accused had used due and proper care in sending out the medicine. If they considered that he had, they must of course acquit him, but if they thought he had not, *they must find him guilty of manslaughter.*" The jury acquitted the prisoner.

We trust that this melancholy event will not be suffered to pass without producing a useful impression on those to whom the task of compounding prescriptions is committed. That "due and proper care" was not used, is quite clear; and therefore we must suppose that the jury doubted whether the oil of tar was really the cause of death. The charge of the Judge clearly establishes, that mere accident or mistake in sending a wrong medicine, does not shield the chemist from the consequences of "killing and slaying" the unfortunate subject of his carelessness.

ATTEMPTED ASSASSINATION OF M. DUPUYTREN.

An individual had been a few days in the Hôtel Dieu with a wound in the left leg. He walked there, and there was nothing at that period in his appearance to indicate insanity. The next morning, however, he was in a state of excitement, and delirium was manifest. The patient jumped from his bed, came out, and would have precipitated himself into the Seine, if he had not been prevented. His ideas took another course. He begged of the attendants to amputate his leg, and asked for a saw to saw the bones; and having made a second escape, he went to the attendant of the hospital, and begged him, in the most earnest manner, to cut off his leg. This conduct was at first attributed to a mere nervous fit of delirium, and in order to prevent him from doing mischief, a strait-waistcoat was put on him, and proper medicines administered. When M. Dupuytren visited the patient on Saturday, one of the attendants, it appears, had loosened the strings, although, when the former approached the bed, he kept his arms as if they were confined. Suddenly, however, the madman jumped out of his bed in his shirt, and throwing himself with great force upon his knees, begged of him to have the strait-waistcoat taken off, and only to tie his hands with cords. M. Dupuytren endeavoured to convince the man that the strait-waistcoat was the least inconvenient for him, but in the instant he started up in a furious manner, struck M. Dupuytren a violent blow with his left hand, on the chest, and then exhibited a large knife with his right hand, and would have wounded M. Dupuytren with it if assistance had not been near. The person who wrested the knife from the man received a severe wound on his finger. M. Dupuytren, in relating these facts, made some interesting remarks on the nature of nervous maladies, and the acts to which patients afflicted by them are likely to resort. The individual in question, it appears, had no motive of hatred towards M. Dupuytren; and the cunning with which he concealed the knife with the last fingers of the right hand, while the others, stretched out, gave him the appearance of a suppliant, shews the method of his madness.—*Nouvelliste.*

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

EMINENT EARLY RISERS*.

If the practice of early rising require any other recommendation than the simple fact of its being favourable to health, to study, and to business, it may be found, perhaps, in the circumstance of nearly all the individuals whose names have been handed down to us as illustrious in history, being early risers.

Sir Thomas More, in his preface to the Utopia, remarks that he completed the work by stealing time from his sleep and meals. He made it his invariable practice to rise at four; and he appeared so well convinced of the excellence of the habit, that he represents the Utopians as attending public lectures every morning before day-break.

The well-known Bishop Burnet was a habitual early riser. When at college his father aroused him to his studies every morning at four o'clock, and he continued the practice during the remainder of his life.

Bishop Horne, at the close of his very excellent version of the Psalms, declares that during its composition, "he arose, invariably, fresh as the morning to his task."

The celebrated Dr. Doddridge mentions, in his Family Expositor, that it is to his habit of early rising that the world is indebted for nearly the whole of his valuable works.

Fabricius, a student of Linnæus, in his notice of that celebrated naturalist, observes as follows:—"Our habitation, that of the writer, the late Dr. Kulin, of Philadelphia, and another student, was about one-eighth of a league distant from the residence of Linnæus, at Hammarby, in a farm-house where we kept our own furniture and other requisites for housekeeping. Linnæus arose very early in summer, mostly about four o'clock; at six he came and breakfasted with us, and gave lectures upon the natural orders of plants, which generally lasted until ten.

Dr. Tissot, in his life of Zimmerman, author of the Treatise on Solitude, states that the latter was accustomed to rise very early in the morning, and

* Condensed from Philadelphian Journal of Health.

wrote several hours before he began his professional visits.

Paley, who in the early part of his college career led an indolent life, and mixed much in society of an idle and expensive kind, was one morning awakened, at five, by one of his companions, who reproached him with the waste of his time and of his strong faculties of mind. Struck with the justice of the reproach, Paley, from that time forward, rose at five o'clock every morning, and continued ever after the practice. It is easy to imagine how much such a course must have contributed to the celebrity of the author of the Moral Philosophy, Horæ Paulinæ, and Evidences of Christianity.

A volume might, indeed, be filled with notices of early risers. Bishop Jewell rose regularly at four; Dr. Franklin was an early riser; Priestley was an early riser; the great and learned lawyer Sir Matthew Hale studied sixteen hours a day, and was an early riser; Dr. Parkhurst, the philologist, rose regularly at five in the summer and six in winter, and in the latter season always made his own fire. It is to the hours gained by early rising that the world is indebted for the numerous volumes which, within a few years, have issued from the pen of Sir Walter Scott.

Among the ancients, the names of Homer, Horace, Virgil, and of numerous other poets, may be inscribed upon the list of early risers.

It is recorded of Buffon, that wishing to acquire the habit of early rising, he promised to reward his servant with half-a-crown for every morning on which he should prevail on him to leave his bed by a certain hour. The servant went resolutely to work, under a commission that authorized him to drag his master out of bed rather than fail; and, notwithstanding he had often to endure abuse, and even threats, so powerfully did the Count's long continued habits of indulgence oppose his own desires to break through them, he, nevertheless, succeeded finally in rousing his master regularly by the stipulated hour. And Buffon informs us, that to the unwearied perseverance of his servant the world is indebted for his well-known work on Natural History.

COMPARATIVE FATIGUE OF DIFFERENT KINDS OF TROOPS.

Ordnance Department.—Recruits for this branch of the service ought to be powerful and athletic men. Artillery men are much employed in the laborious duties of the arsenal, although in some respects they undergo less fatigue than the infantry. They do not in general carry their knapsack on a march, and they are little exposed by night duties.

Cavalry.—Dragoons are almost constantly employed, but their duty is not excessively fatiguing. As they do not carry their necessaries, a march does not add much to their usual exertions. On service they are generally much better protected from the vicissitudes of weather than the infantry.

Infantry.—An infantry soldier on active duty is exposed to greater fatigue than either an artillery man or a dragoon. On a march he is obliged to carry his knapsack, accoutrements, &c. which commonly weigh above sixty pounds. He is also much exposed to night duty. Infantry soldiers look sooner old than persons in civil life, a circumstance which probably depends chiefly on night duties and disturbed sleep. This branch of the service has always a larger proportion of sickness and mortality than either the ordnance department or the cavalry. These circumstances are, no doubt, to be attributed to greater fatigue, more exposure to the vicissitudes of weather, and perhaps to less rigour in the choice of recruits. If my opinion be well founded, it will appear that recruits for infantry corps should, in as far as regards health and muscular capability, be selected with even more care than for the cavalry."—*Mr. Mashall on the Enlisting, &c. of Soldiers.*

HEAT AS A MEANS OF DISINFECTING.

[In consequence of numerous inquiries on the subject of Dr. Henry's Observations on the Disinfecting of Clothes, &c. we subjoin his paper in an abridged form.]

Dr. Henry, of Manchester, has lately described a series of experiments, which established the following conclusions * :—

I. That raw cotton, and various kinds of piece-goods, manufactured for clothing from that or other materials, sustain no injury whatsoever, either of colour or texture, by exposure for several hours to a dry temperature of nearly 212 degrees Fahrenheit.

II. That the infectious matter of cow-pock is rendered inert, by a temperature not below 140 degrees Fahrenheit; from whence it was inferred that more active contagions are probably destructible, at temperatures not exceeding 212 degrees. This proposition it was obviously within the reach of experiment to determine. But I had intended to have re-

* Philosophical Magazine.

signed the inquiry, to those who are engaged in the practice of medicine, as more within their province than my own; when the appearance of malignant cholera at Sunderland determined me immediately to extend the investigation. If that disease be communicable from one person to another, there appeared ground for hope that any new facts or principles, respecting contagion generally, might be brought to bear upon this particular emergency. If cholera should be proved not to be so communicable, there still would remain many infectious maladies, to which any newly-acquired knowledge of the laws of contagion might admit of beneficial application.

Of diseases generally allowed to be contagious, I could obtain access to two only, typhus and scarlatina. The former malady does not, however, answer to all those conditions which are required to render it a fit subject of experiment.

In Scarlatina, however (including both *scarl. simplex*, and *scarl. anginosa*), we have a disease admirably adapted for furnishing the necessary evidence. No one doubts of its being infectious. Perhaps, indeed, of all the diseases with which nosologists have arranged it (the *exanthemata*), it gives birth to the most active and durable contagion. The interval between exposure to infection and the commencement of the disease is unusually short, and may be stated at from two or three to six days. When the infection has been received, the malady produced by it begins to be contagious before the scarlet efflorescence appears; and it continues so even after the subsequent desquamation of the cuticle. Every medical practitioner, of much experience, must have been baffled in his attempts to dislodge it from families in which it had gained a footing. In such cases its revival at distant intervals of time has been sometimes traced to clothes or bedding, which had been carelessly laid by, without being sufficiently purified. In the state of *fomites*, this species of infection has lain dormant for many months. Dr. Hildenbrand, for example, relates that he carried the infection in a coat, which had not been worn since his attendance on a scarlatina patient a year and a half before, from Vienna into Podolia, where the disease had till then been almost unknown. Generally speaking, too, scarlatina is a distinct and well-characterized disease; and whenever it is otherwise, the doubts may commonly be removed, by comparing it with the prevailing epidemic.

These considerations rendered me extremely desirous to try the disinfecting powers of elevated temperatures over the contagion of scarlatina. It fortunately happened that in one of the wards of the House of Recovery a patient (a female, aged nineteen, of the name of Gerrard) was suffering under that form of the disease which has been termed *scarlatina anginosa*. The symptoms, in the judgment of the attendant physician, as well as in my own (taken in conjunction, too, with the previous history of the case), left no doubt of its nature. To make the most of this excellent example of the malady, a succession of flannel waistcoats were worn, each for several hours, in contact with the body of the patient, and were then put into dry bottles, which were well corked, tied over with bladder, and laid by for use. Other opportunities of obtaining waistcoats, similarly infected, soon occurred, in the cases of Sarah Gerrard, a younger sister of the first patient; of William Johnston, æt. eleven; and of Robert Green, æt. fifteen. In Johnston, not only were the appearances quite unequivocal, but he was the last of four children, (not all of one family,) who had been infected, in regular sequence, by communication with each other.

1. A waistcoat, which had been worn all night by the elder Gerrard, a day or two after the appearance of the scarlet eruption, was heated four hours and a half at 204 degrees Fahrenheit, and on the 8th of November was applied to the body of a boy, æt. six years. No symptom having shown itself on the 15th, a second waistcoat was then applied to him, which had been worn more than twelve hours by Johnston on the second day of the scarlet efflorescence, and then heated at temperatures varying from 200 to 204 degrees Fahrenheit, during two hours and three-quarters. After an interval of twenty-two days, the boy, who still continued to wear the same waistcoat, remained perfectly well.

2. A waistcoat, which had been worn twenty-two hours by the elder Gerrard on the fourth and fifth days after the appearance of the eruption, was, on the 19th of November, heated three hours at 204 degrees. It was, after this, worn by a girl, aged twelve years, till the 30th, without effect. Another waistcoat, which had been worn by Sarah Gerrard, was then substituted, but without any effect ensuing.

3. A waistcoat, put on by Sarah Gerrard on the second day of the efflorescence, and worn by her three days, was applied, November 19th, after it had been heated two hours at 200 degrees, to the body of a boy aged ten years. On the 30th, a second waistcoat, which had been worn by Robert Green during the first and second days of the eruption, and which had been kept in the disinfecting apparatus at 204 degrees during one hour only, was substituted; but no symptoms of infection have appeared.

4. A waistcoat, which had been worn by the elder Gerrard seventeen hours on the 7th and 8th of November, (the second and third days of the eruption,) was kept closely corked up in a bottle till the 25th, then heated four hours and a half, at temperatures varying from 200 to 206 degrees, and applied to a girl aged thirteen years. On

the 30th of November, no effect having been produced, another waistcoat was substituted, which had been worn eleven hours by Johnston on the third day of the efflorescence, and then disinfected by a temperature of 204 degrees applied during two hours. No symptoms of scarlatina have shown themselves in this case.

In all the foregoing instances it was ascertained, by the most careful inquiries, that the children, to whom the disinfected waistcoats were applied, had never been affected with scarlatina, and were therefore liable to that disease. The children were attentively examined every day, in order that no slight symptom might pass unobserved.

The experiments, which have been related, appear to me sufficiently numerous to prove, *that by exposure to a temperature not below 200 degrees Fahrenheit, during at least one hour, the contagious matter of scarlatina is either dissipated or destroyed.* To me it seems more probable that it is *decomposed*, than that it is merely *volatalized;* because cow-pock matter, though completely deprived of its volatile portion at 120 degrees, is not rendered inert by temperatures much below 140 degrees. I did not, however, consider it as either necessary to the proof, or justifiable, to determine, with respect to the contagion of scarlatina, either the lowest temperature, or the shortest time, adequate to the disinfecting agency; for these points, which are of no practical importance, could not have been decided without the actual communication of the malady. Still less necessary, and less justifiable, should I have thought it, to have proved, by exciting the disease, that the waistcoats, as taken from the patients, were impregnated with the contagion of scarlatina.

It may, I am aware, be urged that the induction would have been more satisfactory, if founded on a greater number of instances. But experiments, of the kind which have been related, are attended with so many difficulties, as to forbid their multiplication beyond what is absolutely necessary. Not to mention other obstacles, it is far from easy to find young persons in every respect unexceptionable for the purpose;—to insulate them, as was done in these instances, from all casual sources of infection;—and to keep them under the watchful care of observers, qualified to mark even indistinct symptoms that might arise, and to apply the proper remedies. It must be acknowledged also, that the inference from the destructible nature of the *fomites* of scarlatina, to that of other contagions, remains analogical; and that experiments are still wanting to extend the proof to other known species. The argument, however, in its nature cumulative, has acquired a great increase of probability by the step which has been made, in showing that the power of heat is not merely exerted over cow-pock infection, but extends to the active and virulent contagion of scarlatina.

The circumstances under which the experiments were conducted render it, I think, demonstrable *that the disinfecting agency belongs to heat alone;* for the receptacle in which the infected waistcoats were placed having in every instance been closed, change of air could have had no share in the effect. The phenomena, then, are reduced to their simplest form; and the results put us in possession of a disinfecting agent, the most searching that nature affords;—one that penetrates into the inmost recesses of matter in all its various states. As a disinfectant of articles which are capable of imbibing and retaining contagion, heat is greatly superior to the vapours or gases used for the same purpose; inasmuch as the transmission of the latter may be stopped by a few folds of compressed materials; while heat, if time enough be allowed, finds its way in spite of all obstacles. To avoid being misunderstood, I must however repeat, that it is to the destruction, by heat, of contagion existing in substances technically called "susceptible," that I limit the proposal:—for instance, to infected clothing of every description; to infected bedding and bed-furniture of every kind that would be spoiled by washing; to trunks and other packages brought by travellers from infected places; and to merchandise, whenever it can be shown, or rendered highly probable, that such merchandise has been in the way of imbibing contagious matter.

METEOROLOGICAL JOURNAL,

August 1832.	Thermometer.		Barometer.	
Thursday . 2	from 52 to	77	29·84 to	29·79
Friday. . . 3	56	70	29·84	29·80
Saturday . 4	46	67	29·82	29·86
Sunday . . 5	49	65	29 86	29·82
Monday . . 6	48	67	29·82	29·85
Tuesday . 7	53	73	29.86	29·91
Wednesday 8	45	75	29·96	30·02

Prevailing wind, S.W.

Except the 6th and 8th, cloudy; rain on the 2d and 5th.

On the afternoon of the 2d, a violent storm of thunder and lightning, accompanied by heavy rain; at 15 and 30 minutes after six, a vivid flash of lightning was followed, after an interval of about one second, by as violent a peal of thunder as I ever remember to have heard. The particularly heavy fall of rain which immediately succeeded each flash of lightning above alluded to, excited the attention of many. From the hour of six till half-past seven, ·6 of rain fell.

Small meteors are nightly seen, particularly near the Zenith.

Rain fallen, 1 inch and ·625 of an inch.

Charles Henry Adams.

NOTICE.

R. J. G.—Sir D. Brewster had returned to Scotland.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 18, 1832.

ESSAYS ON DIAGNOSIS.

BY

MARSHALL HALL, M.D. F.R.S. L. & E. &c.

ESSAY I. *continued.*

II. ON THE OBJECTS OF DIAGNOSIS.

IN the former part of this Essay I have briefly enumerated the principal *sources* of diagnosis. I now propose to apply those principles to actual practice.

It is essential, in the first place, to form distinct ideas of the nature and subject of our investigation. I would observe, therefore, that diseases are not, like the objects of natural history, capable of being divided into species, which are continually reproduced in nature. They consist, on the contrary, of mere individuals, or varieties, which never recur in precisely the same form. They do not even admit of being viewed in this degree of simplicity; they are, on the contrary, not only continually varying, but they are continually more or less complicated with each other, in combinations which induce varieties still more varied. The varieties of diseases become yet more numerous, by the conjunction of the same or different diseases concurring in several parts. And lastly, every case partakes of a peculiar and individual character, impressed upon it by the peculiarities of the constitution, age, and sex of the patient; the season of the year, the prevailing epidemics, the "constitution" of the atmosphere, &c.

With all these sources of variety in disease, it is essential that the physician should be familiar; but it is equally necessary that this extensive subject should be simplified for the sake of the student and the young practitioner, who must be led through its elementary portals to view the complicated structure of the temple of medical science; or, to employ simpler terms, we must first treat of diseases viewed simply and distinctly, before we proceed to detail the circumstances of multiplicity and complication under which they occur in nature. We must then lead the student on to contemplate diseases as they actually occur; as almost universally complicated; as involving the general system and its various parts; or as affecting one particular organ principally, but the rest also consecutively.

Every disease must be observed in regard to

1. *The History.*
2. *The Symptoms.*
3. *The Effects of Remedies.*
4. *The State of the System.*
5. *The Morbid Anatomy.*

Every disease must be traced in its *Complications,* which, in their turns, consist in

1. *Effects of the Disease itself.*
2. *Effects of the Remedies;* and each of these, in
   1. *Symptoms,* or *Changes in Function;* and in
   2. *The Anatomy,* or *Changes of Structure.*

In pursuing the diagnosis, I purpose, after every enumeration of a disease, to add these words. The paragraph will

then be continued by the further addition of what is known on that topic, in regard to that disease. How many blanks there will be I need scarcely mention; but whenever such a blank in our knowledge does occur, a note of interrogation will at once express that fact, and suggest many a subject of investigation for new inquirers.

As a first example, I will adduce the case of FEVER. This term, derived from *ferveo*, merely means heat of the general surface. Such a condition occurs in many diseases; but the term fever has been restricted by physicians, ancient and modern, to denote certain diseases practically and really distinct from all others.

It rarely happens that fever consists in mere febrile movements of the system. There are usually complications with the general febrile state of affections of the head, chest, and abdomen; and it has been long disputed whether these affections be primary causes or secondary effects of the fever; and much that is just been argued on both sides of the question. It is singular that no such dispute has been raised in regard to a class of fevers which we shall designate the eruptive. Yet it appears to me that the rash and the sore throat of scarlatina, and the rash and the catarrh of rubeola, and the other complications of these and of other febrile diseases, occupy the very same rank as the various local affections, whether of function or of anatomy, which we observe so constantly in other fevers. They occur alike in the *course* of these several diseases, and doubtless occupy the place both of effects and of causes in the entire disease. The object of greatest moment in regard to the dispute is to lead the young practitioner to observe accurately, and to watch carefully, in order that he may early detect these complications in their varied form and extent, and promptly apply the appropriate remedy.

This concluding remark may be applied to the ERUPTIVE FEVERS. In the first days they cannot always be distinguished from other forms of fever, and during their course, and after their more wonted course, the same watchful observation is necessary to detect topical complications. This is especially true of variola, and scarcely less so of rubeola, of scarlatina, of erysipelas, &c. These complications of febrile diseases are principally two: a peculiar inflammation and its consequences; and tubercles. The former are acute at first; the latter insidious.

Similar remarks apply to the next class of diseases to be mentioned, viz. INFLAMMATIONS. Inflammation is apt, although far less so than fever, to be complicated. We frequently on dissection find inflammation of more organs, of more cavities, than one. This is particularly true of protracted inflammation.

I regard the view of fever and of inflammation, which I am endeavouring to sketch, as applicable to all derangements of the system without exception, in various degrees, and as one of the most important, and least cultivated, to which the attention can be directed. The actions of the system cannot be deranged in any way without the danger, daily increasing, of topical disease, in one or more organs. Most observed in fever, least in inflammation, this tendency obtains in all diseases, and in all disturbances of the system, only in varied degrees, and in various modifications. To this important subject I shall have frequent occasion to recur in the following essays.

ARTHRITIS is another disease which involves an affection of the general system, of various organs or parts, with that principally affected. How much has been said, and how little is really well and truly known on this subject! Not being a disease of hospitals, the morbid anatomy has not been well cultivated. No branch of physic is still so much in need of a philosophical investigation as that of gout.

Very similar observations apply to another disease, somewhat better known, indeed, but still involved in much doubt and uncertainty, viz. RHEUMATISM. The connexion of rheumatism and hypertrophy of the heart is well known to the physicians of this country; but the ear of our neighbours in France is still deaf, and their eye still blind upon this topic. And how does confusion reign in regard to the other internal rheumatic affections!

There is a subject entirely neglected by the profession, which I must bring particularly before the reader in this place. It is that which I have designated IRRITATION. Much confusion exists in regard to the sense in which this term is to be employed in medi-

cine. The French pathologists appear to mean by it only a lower degree of inflammation than that to which the latter term is confined. Now a question of mere degree does not require a new designation, but merely an epithet. Besides, it must appear to all, that from the very signification of the word, the term irritation ought to be used in a sense totally different from that of inflammation.

Without entering further into this discussion, I shall briefly explain the manner in which I purpose to use this term, and I shall do so by an unequivocal example. I will suppose a calculus existing in the gall-duct, or in the ureter. It may prove the source of much suffering,—of pain,—of sickness. This is a case of irritation. The calculus is the cause; the pain the immediate, the vomiting the more remote, effect. All this is very simple and intelligible. There is an example of irritation less known, less acknowledged, but not less unequivocal. It consists in a morbid condition of the intestinal contents, which proves a source of varied suffering, chiefly in the abdomen or in the head,—and resembling acute inflammation of those parts.

Next to irritation, we must consider the case of Exhaustion or of Inanition, from loss of blood or other causes. As in so many other morbid affections, the general system and different organs, are affected in this disease, but especially the head and the heart. Like irritation, exhaustion has long been mistaken, in its effects, for some inflammatory diseases. Both these cases should be constituted, like fever, inflammation, &c. into *genera*, or *classes* of disease. The former are scarcely less frequent or less important than the latter, and the diagnosis is one of the utmost moment.

Nearly allied both to irritation and exhaustion are Acute Dyspepsia and Chlorosis. Conjoining a marked affection of their general system with equally marked topical affections, they, like so many other diseases, must be viewed in the light of classes rather than of individuals. It is highly important that the young physician should be familiar with both their general and their local forms.

The same observation applies to Hysteria. Consisting originally in intestinal, or uterine, or, as some say, spinal irritation, and inducing peculiar effects upon the nervous and muscular systems, hysteria is not only a class of disease, but a class involving almost as many forms as that of inflammation itself.

In like manner, Scrofula or Tubercles are rarely confined to one organ. There is a general affection of the whole system, — the cause? — the effect? of this disease, and an affection of one or various organs. In our despair of curing tubercle, we should not neglect the study of the disease in this peculiar aspect. Such a study leads to an early diagnosis, and this always redounds to the physician's reputation, and constitutes, indeed, his best, his most just title to it.

The disease, still so little known, designated Melanosis, affects various organs simultaneously.

That disease, known under the designations fungus hæmatodes, encephaloid tumor, and many others, and which, after Dr. Farre, I shall denominate Tubera, as at once the shortest and most distinctive. Like so many others, this disease is one of the whole system, and apt also to occur simultaneously in various organs.

Scirrhus, and its consequence, Carcinoma, is also apt to occur in different organs, especially organs of the same system, and it spreads its awful ravages along the absorbent vessels and glands. Dr. Farre detailed a case in his Lectures, from Sir Astley Cooper, of scirrhus occurring simultaneously in the uterus and the mamma.

These diseases must next be arranged accordingly as they affect the different important cavities and organs, — the Head, the Chest, the Abdomen, the Brain, the Lungs and the Heart, the organs of the Digestive, Urinary, and Generative systems.

Such is an imperfect, rapid sketch of the *objects* of diagnosis. It now remains for me to state equally briefly the plan which I propose to adopt in the further prosecution of this important subject.

In the first place, I wish as much as possible to arrange and bring before the young physician *every* case which can require his attention in actual practice. In doing this, I have been equally desirous to avoid surcharging these sketches with the names and descriptions of diseases which are more objects of curiosity and over-refinement, than of practical utility. I must be excused

for still thinking the terms fever, inflammation, rheumatism, scirrhus, &c. useful and practical designations of disease, just as rubeola, erysipelas, and gout, are so. They, like all other terms in all the sciences, require first to be accurately defined,—the sense in which they are used to be strictly determined; and then I know not that the science or the practice of medicine would gain by an exchange of these terms for others, such as gastro-entérite, hyperémies, &c. &c. &c. terms equally objectionable, as *inadequately* expressing the nature and phenomena of fever or of inflammation respectively, and not having the sanction of use for their employment.

I shall be guided by similar principles in the distinctions which I shall attempt to draw between different diseases. My aim will further be to separate really useful subjects from the curious and the fanciful,—and this, although the former may consist in changes of function, unattended by appreciable changes of structure, and the latter, sometimes in actual changes in the anatomy: I think the distinction between inflammation and irritation, for instance, of infinitely greater *vital* importance, than that between eccentric and concentric thickening of the heart. I by no means pretend, however, to depreciate the merit even of such discoveries; I only wish to state my own conviction of the comparative value of certain medical facts and investigations, and to express the principle which has guided me in the selection I have made, and the comparative importance I have attached to them respectively.

Having in this manner endeavoured to form a complete collection of diseases, the next question has been as to the mode of arrangement. I have adopted that which has appeared to me at once the simplest and the most practically useful: it has been that of classing them together in the order and manner of their external similarity. I have designated this mode of arrangement the *diagnostic arrangement*, as being that which immediately suggests the objects and the means of diagnosis. Two or three diseases placed closely together, for that very reason require to be distinguished with peculiar care. A disease placed alone, on the other hand, however difficult of cure, presents no difficulty in the diagnosis. Having formed a conjecture, rather than an opinion, of a given case, we have but to refer to this classification to discover what difficulties in the diagnosis do exist, what dangers beset our path—as the mariner learns from his chart the situation of rocks and sand-banks—and we are immediately led to look for the means of avoiding them.

The very first question which occurs in regard to the arrangement of diseases, is the distinction between symptoms and real diseases. The very same affection sometimes occupies both of these ranks. For example, how often is dropsy a mere symptom? how often is it a distinct disease? It will frequently happen that the very same affection will be found arranged amongst the *symptoms* and amongst the *diseases*.

I purpose presenting to the reader a Sketch of a Diagnostic Arrangement of Diseases in the ensuing Number. In a subsequent essay I shall bring before my reader some of the more interesting and important points in the diagnosis. This rapid sketch of the sources and objects of diagnosis, will, I trust, not prove unprofitable or uninteresting. The subject will be treated at greater length in a work which I have in the press, and which I hope to publish early in the ensuing winter.

14, Manchester Square,
Aug. 8, 1832.

VACCINATION AS INFLUENCED BY THE QUALITY OF THE SKIN.

To the Editor of the London Medical Gazette.

Sir,

A late number of your journal contains some remarks, by Dr. Howison, on "the practice of Vaccination, with reference to the comparative Thickness of the Skin in Children." No one can have been engaged extensively in a vaccine institution without noticing differences in the qualities of children's skins, requiring attention on the part of the vaccinator. You will find mention made of this in some remarks on the "Surgery of Vaccination," which I published in the London Medical and Physical Journal for November 1826 (page 411). On the importance of *attention* to the fact, I am quite of the same mind with your correspondent, but my experience does

not enable me to corroborate the practical reflections which a "knowledge of this fact" have suggested to him. His words are these:—"The vaccinator should be on his guard, lest the skin should prove thin; for if so, blood will instantly follow the slightest scratch of the lancet, washing away the virus, rendering the (future) vaccination useless, running down the arm, proving discreditable to himself and hurtful to the feelings of the mother. A thick skin is advantageous, as allowing of the *free* and *slow* absorption of the vaccine lymph."

This passage contains matter which I believe to be questionable both in point of theory and practice. I do not clearly understand what is meant by a *free* and *slow* absorption of the vaccine lymph, nor how it is to be judged of, except by the advance of the vesicle; nor how the thickness or thinness of the *skin* can influence the activity of the absorbents of the arm. When a child is in full health, well nourished, and plump (that is to say, when the absorbent system is active throughout the body), the vaccination will, for the most part, advance regularly, and reach its acmé on the tenth day. When the child is puny, thin, and ill nourished—when you have all the evidences of inactive lacteals—there, in very many cases, vaccination will be languid and slow. The areola will not form before the ninth or tenth day, will even then be small, and the disease will not have reached its crisis before the twelfth day. Dr. Howison will not contend that this *slowness* of the vaccine process is "*advantageous.*" In summer, or when the capillary circulation is unusually active, the areola will often form as early as the seventh day; but I never imagined, nor shall I easily be persuaded to think, that this *rapidity* of the vaccine process is in any way *disadvantageous*. So much for Dr. Howison's doctrine of the advantages of a *free* and *slow* absorption of vaccine lymph.

And now a few words on another point of doctrine maintained by your correspondent—the ill effects of a free flow of blood from the punctures. "*It washes away the virus, renders the vaccination useless, brings discredit on the practitioner, and hurts the feelings of the mother.*" After practising vaccination twelve years, I have met with no such results. On the contrary, I see daily, at the Small-Pox Hospital, the most perfect vesicles rising where the arm had bled freely. Indeed, to this circumstance I have repeatedly called the attention of gentlemen attending at the hospital, and pointed out to them that a free flow of blood cannot prevent vaccination taking effect, after active virus has once come in contact with the absorbing surface of the cutis vera. As to the feelings of the mother being hurt by the sight of the blood, I can only say that no such thing takes place among the four thousand mothers who annually attend at the Small-Pox Hospital for the vaccination of their infants, some of whom are not contented unless there be a *palpable* degree of bleeding from the punctures, and many of whom, aware of what will follow, bring with them a handkerchief to tie round the child's arm.

It is scarcely necessary to observe, that the quantity of blood which follows the punctures made by the vaccinating lancet, will be proportioned to the fullness of the child's habit, and the state of the circulation in the arm at the period of the operation. A child full of blood, with an active state of vessels circulating and absorbing, is avowedly in the best possible condition for successful vaccination; and the arm of such a child is, *cæteris paribus*, most likely to bleed freely.

So far from considering, with your correspondent, Dr. Howison, that in vaccination a thick skin is advantageous, I have always held, and continue to uphold, the opposite doctrine. A thin skin, by affording a ready entrance to the lancet, favours the application of the virus to the absorbing surface; whereas, a thick and tough skin, by throwing back the lymph upon the shoulder of the lancet, will often, in the hands of an inexperienced practitioner (particularly if the lancet be at all blunt), prove the source of disappointment.

From what has now been stated, it will be seen that the success of the operation of vaccination depends upon various circumstances connected with the condition of the child—viz. its general state of health at the time, and the peculiar structure of its skin: also upon the delicacy of manipulation on the part of the operator. But these points, though highly important, are far from exhausting the subject. Much depends

on the original constitution of the child, and its aptitude or inaptitude to receive and nourish the vaccine virus. On this subject I have descanted at length in the London Medical and Physical Journal for November 1827. Lastly, the greatest importance must be attached to the selection of lymph from which the vaccination is to be produced, especially with reference to its age. I have recently, however, had reason to believe that another circumstance merits attention, and that even at the same age (say the eighth day) the lymph of all vesicles is not equally efficient. It is, of course, impossible to anticipate accurately what shall prove active and what inactive lymph; but experience will do much towards determining the point.

I cannot conclude without offering to your indulgent readers some apology for the minute details into which I have been led, and the importance I attach to circumstances which, in the eyes of others, may probably appear trifling.

I am, sir,
Your very obedient servant,
GEORGE GREGORY.

31, Weymouth-Street,
August 6, 1832.

RENT OF THE UTERUS.

To the Editor of the London Medical Gazette.

SIR,

IF you think the following particulars of a case of ruptured uterus will prove interesting to any of your readers, you will oblige me by giving them insertion.

I am, your obedient servant,
JOHN CHATTO.

14, Hunter-Street, Brunswick-Square,
August 7, 1831.

E. B. æt. 28, a well-formed little woman, (a patient of the Finsbury Midwifery Institution) residing in this neighbourhood, was taken in labour with her sixth child on the morning of August 3d. I was engaged to attend her; but not being at home when she sent for me, Mr. Thorne saw her about 11 A.M. He found the os uteri not at all dilated; neither were the pains active; and not thinking it at all likely the labour was near, he left her. Some time after this, however, the pain came on with such vehemence as to lead her to believe her delivery would be almost immediate, especially as all her former labours had been remarkably speedy. She did not, however, obtain any further aid until about 1 P.M. when I saw her. I found her lying upon the bed, and was struck with the paleness of her face, and weakness of her pulse, joined to some peculiarities of her manner, so very different to any thing I had before seen of her. She was very restless, and yet very averse to move, or be moved, in order to be placed in a convenient posture for me to examine her. She said she could not move without much pain. She would answer no question unless it were repeated many times, and then in a very unsatisfactory manner. She seemed in the greatest alarm as to the result of her labour, declaring she could never get over it, and complaining, apparently in a childish manner, of its duration. She did not seem to suffer much pain, at least of a regular or effective kind, although she said she always had some, and that a short time before it was very vehement. From the weak state of her pulse, I was led to inquire whether she had lost any blood, and found she had flooded somewhat: she herself said very much indeed, and that it had not come on until after Mr. Thorne was gone. I could not ascertain the exact quantity; but it seemed to me to be considerable, although, perhaps, not enough to occasion much alarm on ordinary occasions; and I felt more inclined to attribute her present state to excitement of the nervous system (especially as I learned her head had once before been most seriously affected) than to loss of blood, and endeavoured to quiet her, and enliven her spirits, which were very despondent—but without avail.

On examination, I found the os uteri a little dilated, and becoming very soft, the membranes being tense, and the presentation quite natural. Her pains were infrequent, and not very strong, but every now and then some small quantity of blood came away. The os uteri having presently become much thinner and dilated, I ruptured the membranes, conceiving her ill able to bear any farther loss of blood. Subsequently to this, no blood of any consequence flowed. A few more pains, slowly following each other, fully dilated the parts, and brought the head into

the pelvis. The pains were now for a long time absent, and when present, very ineffective ones; so that all the soft parts being fully dilated, and the pelvis a very roomy one, I gave her a scruple of the secale cornutum, infused for a few minutes. This produced energetic uterine action, which continued until the child's head was born, and then entirely ceased. I irritated the uterus externally by grasping it firmly, which gave her pain, but I did not dare to accelerate the birth of the child by manual efforts, fearing the effect of too speedily emptying the uterus while in so torpid a state. I felt much surprised at this circumstance, having always found where the secale had been given, that its effect has not ceased until the uterus was completely evacuated. I dare say ten or fifteen minutes elapsed before the pains recurred, when they did so with great vigour, and continued until the child and placenta were expelled; and, indeed, for some time after. The uterus contracted completely, so as to be felt like a hard ball just above the pubes, and the bleeding did not recur; nor, indeed, did the uterus change at all from this desirable state. She was delivered about 3 P.M. For some time after delivery, she complained of pain and soreness on my pressing the uterus, though not much more than women ordinarily do. She seemed to be hardly aware that she was delivered, but drank some spirits which I gave her. I asked her several questions, which she sometimes would answer, and sometimes not. I soon found, that so far from rallying from this state of depression, she was sinking still more; her face became of a deadly pallor; and her pulse first very small, and then imperceptible. I gave her copious doses of brandy without the least effect. She was in a constant state of excessive jactitation, fetching her breath every now and then almost convulsively. She talked much, and very much at random, declaring she was not delivered, and wishing to get up, in order to bring on her pains. Yet again, if you addressed her in a loud tone, she would answer questions apparently rationally enough. The muscular exertion she used was very great in throwing herself about and rising up in bed, often requiring much force to restrain her. It was with the utmost difficulty I could make her swallow brandy, gruel, &c.; but no traveller in the parched desert ever drank cold water with more avidity and delight than she did, expressing her pleasure in vivid terms, and calling aloud for more. Notwithstanding she was pulseless for so long a time, she never became at all insensible. Finding the measures I employed of no avail, I requested Mr. Thorne to see her. He directed the application of warmth to the surface of the body, and a continuance of the brandy, but with no benefit whatever. She continued in this state until within an hour or so of her death, when she became much quieter. She died about half-past 8 P.M. nearly six hours after delivery.

Upon examining her body the next day, a large quantity of blood was found effused into the cavity of the abdomen. The uterus was firmly contracted; and posteriorly, near its fundus, was found ruptured to some considerable extent. The surface of the rent was covered with portions of coagulum; it occupied a space perhaps as large as a crown-piece, but of irregular margins, and surrounded by a reddened stain, giving it, at first view, the appearance of having resulted from ulceration. Near this large breach were three or four smaller cracks in its substance. Upon cutting into the cavity of the uterus, it was found that the rupture had not extended into it, the lining membrane being entire throughout; and, indeed, a considerable portion of muscular substance intervened between it and the external rent, which perhaps had not penetrated more than two-thirds through it. The uterus seemed sound in all other parts of its structure. The surface, whence the placenta had been detached, looked as usual, and was some slight distance removed from the situation of the rupture.

It is interesting to inquire when this rupture first occurred, and whether the ergot of rye was at all instrumental in producing it. From considering the above details, it appears to me most probable that it took place at the time the woman described her pains as being so exceedingly severe after Mr. Thorne had left her, especially as I did not find the os uteri in such a state as to lead me to believe that much uterine action had been expended upon it. The patient's peculiar state of mind and body when I first saw her, and which did not exist when Mr. Thorne was with her, also corroborates this opinion. I did not, moreover, exhibit the secale until

the parts were well dilated, and I found her pelvis was a capacious one; and although it produced very effective and continued pains until the head was expelled, yet they were not more energetic than I have often seen them where this medicine had not been exhibited. Moreover, I think it highly probable that the same occurrence which produced this rupture, also caused a partial detachment of the placenta, whence proceeded the hæmorrhage, which she dated from the same period as the severe pain.

EXHAUSTION FROM LOSS OF BLOOD —EJECTION OF A LARGE WORM.

To the Editor of the London Medical Gazette.

Sir,

In an age like the present, teeming with so many important medical and surgical facts, I am doubtful whether an observation, not bearing the stamp of extraordinary value, would be deemed worthy of a place in your Gazette. Perhaps, however, a case professing only to record a single fact, may interest some of your readers, and I therefore request an early insertion of the following case in your journal, and I am, sir,

Your most obedient servant,

Owen W. Williams,
Member of the Royal College of Surgeons in London.

Leamington Spa,
August 7, 1832.

In April 1830 I was requested to visit an Irishman, who was said to be sinking rapidly, owing to the frequent vomiting of blood. On my arrival, I found a person, whose cold and exhausted body betrayed great symptoms of danger: he had a tumid belly; his eyes were sunk, and his pulse feeble; in fact, he appeared to be suffering the consequences of extreme loss of blood. I had the satisfaction to find, that by the prompt application of warmth externally and cordials internally, his pulse began gradually to become firmer and more frequent, while heat became more generally diffused over his death-like frame; and he soon recovered so far as to enable him to give me his previous history. He informed me that, as he was proceeding into the country to the harvest, he was obliged to sleep in an out-house: he slept soundly all night, regardless of its discomforts, owing to his having walked many miles that day. On the following morning he was seized with a violent pain in the region of the stomach, which continued several hours. About evening, nausea, succeeded by vomiting, came on: the ejected liquid consisted of a considerable quantity of dark blood, with some clots of coagulum. For eight days did he continue in this state, voiding at intervals blood from the stomach and dark matter from the bowels, when he was conveyed in a common stage to Holyhead: he was taken to the churchyard, and considered by many as dead. It was at this time my assistance was required, as the parochial surgeon. When the poor man was a little recovered, he was ordered to take an aperient draught; and I now also put him on a plan of treatment to prevent a recurrence of the hemorrhage, which, as his powers became revived, had manifested an inclination to return. The mixture prescribed for him consisted of

Pulv. Ipecacuan. Comp. gr. xij. Tr. Catechu ʒiij. Aq. Puleg. ℥vj. fiat Mist. capeat cochlearia duo ampla 2da quaq. hora.

Having taken one dose, and the pain still continuing, he thought the quantity too small; and, in the absence of his hostess, he was induced to take the remainder of the contents of the bottle, after but an interval of one hour from the first dose. This over-dose soon began to operate, so as to cause violent vomiting and retching, threatening instant death. On my arrival, I found the vomiting had ceased; and he could feebly articulate, that he felt very easy. On examining the contents of the stomach, which had been received into a common vessel, I was astonished to find floating on the surface a *Worm*, of the centipede kind, an inch and half in length, with 100 feet, covered with hair; and considerably larger in circumference than the natural size of such animals.

In this case, what proved to be an accident, did most powerfully effect the dislodgement of an animal, which we have every reason to suppose would have secured its hold in opposition to a more mild remedy, until life would at last have been totally extinguished.

It must be evident from the large quantity of blood voided, that it had perforated the coats of the stomach deeply, and most probably in several places. The gastric juice is well known to have no effect on a living animal, however powerfully it may be invigorated with the vital principle. In this case, that vital fluid could never have had the slightest effect, owing to the naturally weak powers of the constitution, and their scanty supply of nourishment, which this poor man is said to have suffered from.

REMARKS

ON

DR. WILSON PHILIP'S THEORY OF THE CIRCULATION.

By David Badham, M.D. Oxon.

It seems extraordinary, so long after the discovery of the circulation, that physiologists are still occupied in investigating the number and nature of the forces by which it is accomplished; yet, among the whole of those agents, which a more recent physiology has pressed into this service, there is perhaps none which we should not be glad to accept, in order to the better intelligence of that great and primary function, provided that we can assure ourselves of their reality. Dr. Wilson Philip, however, holds a different opinion, and seems to think that we have more of such agents in our hands than the occasion requires, and that the conjoined systoles of the heart and blood-vessels should be alone sufficient. I certainly agree with him in opinion that we ought to exclude the motions of the lungs from our estimate of these forces; not only because the act in question cannot occur in fœtal life (and we know that without respiration the fœtal heart transmits the blood as perfectly, and much oftener than the maternal), but because the respiratory motions are interrupted ones, and not strictly uniform even when in operation; whereas the flow of blood to the heart is continuous and regular. As the number of respirations is much smaller than that of the pulsations of the heart—there being generally but one of the former to three or even four of the latter—and as each pulsation (or egress of blood) implies a corresponding influx from the veins into the right auricle, the blood must of necessity both enter and be expelled from the heart during those respiratory pauses, when the pressure of the atmosphere is equal every where. Hence I should infer that the circulation of the blood through the heart is wholly unassisted by the function of respiration; and the same reasoning which makes me reject this influence disposes me still more to put out of account the variable and infinitely complicated results of muscular contraction, upon blood-vessels distributed through them; for this species of pressure can be no more than the occasional cause of an increased velocity, and not among the agents of a regular circulation. Now, if we reject these supposed agents to assist the *venous* circulation, there only remains for the explanation of that difficult problem, 1st, a resiliency of the heart itself, by which it may suck in blood from the veins; 2dly, a *vis insita* in the veins themselves, by which they are enabled directly to carry forward the blood into the right auricle. The latter property is particularly advocated by Dr. Wilson Philip in his recent paper; the former, however, is a power in which one cannot but feel a very different degree of confidence.

"Whatever be the elasticity of the ventricles," says Dr. Wilson Philip, "it can have no effect on the blood in the veins; because the ventricles merely receive the blood from the inelastic auricles, which are *contracting* during the ventricular *diastole*." But this conclusion appears to me to be but slightly supported by an appeal to facts: the auricle, as we know, contracts *but feebly and very partially*; so that the ventricle may still, and, as I believe, does in fact, perform the office of a sucking-pump on the rising column of the blood sustained in the vena cava. Again: Dr. Wilson Philip propounds, that "the veins being tubes of so pliable a nature as, when empty, to collapse by their own weight, it is impossible that an ascending motion in the blood could be produced in them on any principle of suction; and that as far as the heart may possess such power, its tendency would be to cause the vessel to collapse, rather than to raise the fluid." But surely no "tendency to collapse" can take place where a system

of vessels remains constantly in nearly the same state of distention; which cannot be otherwise where an equal quantity of blood is received by one side of the heart and propelled by the other. An average equality in the contents of the two orders of vessels is of strict necessity, and any tendency to collapse is effectually prevented by the constant ingress of as much blood into the veins as is drawn out of them by the alledged or admitted elasticity of the heart. But as Dr. Wilson Philip objects to reasoning in physiology where nature may be interrogated by experiment, the following experiment was instituted by that gentleman:—

Experiment.—An inch and a half of the jugular vein in a rabbit is exposed; a ligature is placed on the extremity farthest from the heart; the head of the animal is left depending, by which position the blood, in order to reach the heart, will have to ascend against gravity. "The ligature being suddenly compressed, the blood contained between it and the heart was completely and instantly expelled, so that to a superficial view there seemed to be *no vessel* where a large dark-coloured vein had just before appeared. In the meantime, on the other side of the ligature the vein had become gorged with blood. Hence Dr. Wilson Philip deduces a power of independent action in the veins to forward the circulating blood."

But may I ask Dr. Wilson Philip whether it be indeed so certain that the ascending blood, in his experiment, did thus prevail against gravity by a power resident in the vein, and not rather by that action of the heart (the indraught or suction) against which he has been contending? Why was it that an *artery*, similarly exposed and similarly tied, got rid of its blood more slowly than the vein had done? Is, then, the vis insita (even if we allow it) in a vein—a vessel which, according to Dr. Wilson Philip, "collapses by its own weight,"—greater than that of an artery? The experiment appears to me to shew nothing more than that the power of the heart (as might naturally be supposed) to draw blood into its cavities by its own resiliency, exceeds the "vis propria" of an artery deprived by ligature of the "vis a tergo." From this experiment on the vein, Dr. Wilson Philip also draws the conclusion that there is no "vis a tergo" concerned in the venous circulation; because when such a power, if it ever existed, is taken off by ligature, the blood still finds its way into the heart. But may not an opposite inference be drawn from Dr. Wilson Philip's own experiment?—for if there was no vis à tergo, why was the vein *behind* the ligature, as we read, "gorged with blood?" Can that distention be explained by any vital action of the vein? If so, a vein has opposite actions, and may both distend and relieve itself; which is not pretended. On the contrary, Dr. Wilson Philip attempts to shew that the action of the veins is of the same kind (*contractile*) with that possessed by the heart and arteries; so that the tendency of the vein should be to contract its capacity only. On the whole, I remain as unconvinced that the veins have any active share in the reflux circulation as before these experiments were instituted; and must believe the heart, arteries, and capillaries, to be the agents mainly conducive to its completion: the heart, the principal power, acting by its own muscularity and elasticity (at once a propelling organ and a great suction valve); the arteries having, perhaps, a larger share than is usually allowed to them in assisting the active energy of the heart; and the capillaries being far from unimportant auxiliaries in promoting that reflux which is still principally effected by the expansion of the heart.

Among the forces of the circulation, shall we ever be authorized to insert *an expansive, penetrating energy of the blood itself?* If we admit the blood to be a vital fluid, a fluid having a *vita propria*, and exhibiting certain properties not conceivable but by allowing it a participation in qualities which, when attached to solids, we call *life*, the notion of the blood assisting to move itself is not an intuitive absurdity. Vitality cannot be conceived without the power of motion.

It is usual to suppose that the power of the heart and arteries in the circulation is lost in the capillaries: these, consequently, must require a power of their own to advance the blood into the veins—in short, an independent action. The powers of the heart, arteries, and capillaries, must, it should seem, be in one sense independent powers, but must all act together and assist each

other. In the words of Hippocrates, though not in the Hippocratic application of them, παντα ευροα, καὶ συνροα.

Dr. Wilson Philip, I observe, elsewhere labours to shew that the absence of fibrine in arteries is not a reason for our denying muscularity to these tubes; and instances the crystalline lens (as Dr. Young had done) in support of this his opinion. But as there exists no ground for concluding, *à priori*, that any particular tissues must contract upon the application of stimuli, merely because they are what we call muscular, in visible structure; so there is no antecedent reason why other tissues may not, under certain circumstances, contract, though not muscular, in the possession of, or disposition of fibres. I think we have abundant evidence that they do. "Muscularity" and "muscle," words used by every body, are not yet rigidly defined. The chemist looks for the evidence of muscle in its *analysis;* the physiologist calls *that* muscle which contracts on the application of a stimulus; and the definition of a microscopist differs from both. The iris, the canal of the urethra, the middle coat of the arteries (perhaps miscalled muscular, but capable, notwithstanding of strong contraction), the gall-bladder, and the contractile coat of the intestines (which Dr. Hodgkin tells me is essentially different in its molecular structure from any muscle), may all be cited in evidence on this question. If, then, I deny to the veins a power of their own, it is not that they are not muscular in the disposition of their fibres, but that I yet see no evidence of their possessing any such action as should result from that structure. That veins may occasionally pulsate I admit, for I have seen an instance of the kind; but then such instances are too rare to permit any inferences from this observation; and if such instances were more frequent, still they would not explain what occurs in the ordinary circulation.

ON DOUBLE VISION, AND CERTAIN OTHER OPTICAL PHENOMENA.

To the Editor of the London Medical Gazette.

Hackney, Aug. 9, 1832.

Sir,

In the last number but one of your journal, you notice Dr. Graves's paper on Vision, and ascribe a certain portion of originality to his views of the "crossing of images." Allow me to refer you to some of the older volumes of the Lancet, which, by the by, contain far more valuable professional matter than do the later ones, for some excellent papers on many peculiarities connected with vision, by Mr. Thomas Williams; for instance, to p. 344 of vol. ii. 1828-9, wherein this gentleman adduces the fact, and explains what he conceives to be the reason, of the crossing of the images, under certain circumstances—he calls it the changing of sides—and mentions an experiment, in the repetition of which this phenomenon must be detected.

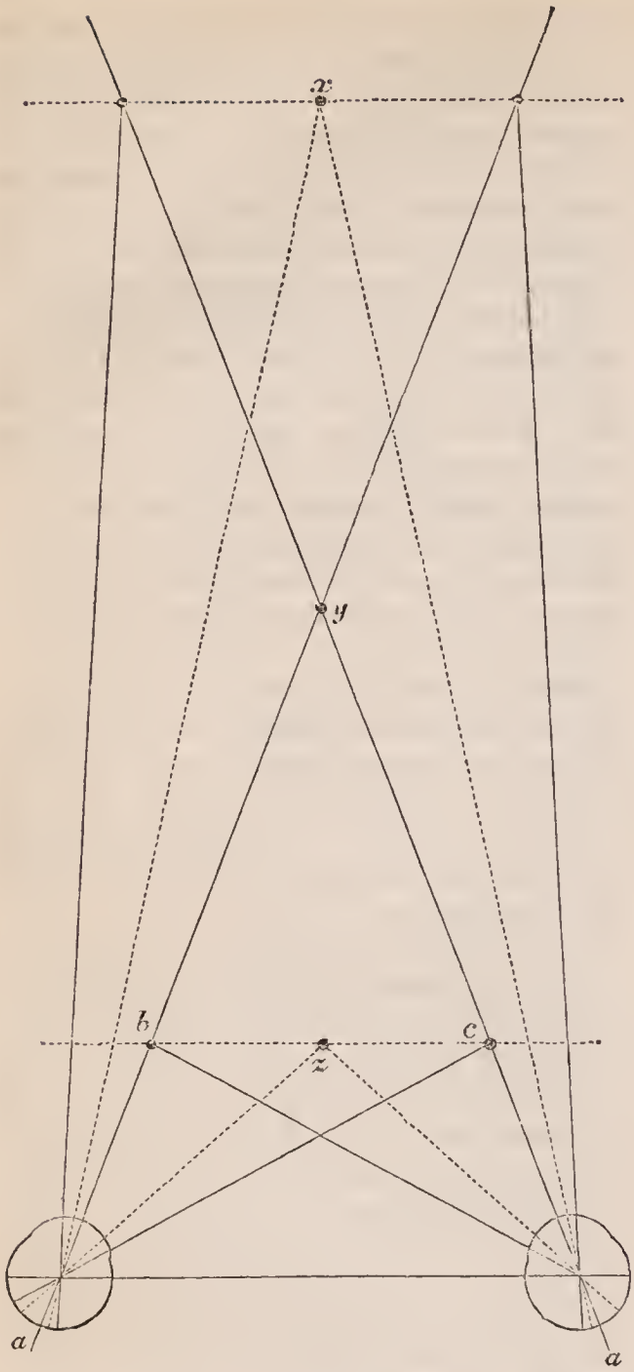
But he does not speak of the changing of sides in reference to the more *distant* of two objects, which you say must necessarily be the case*. From a repetition of Mr. Williams's experiment (the following), I cannot arrive at this conclusion.

A church, for instance, is at some distance directly before my eyes. I hold up my thumb at arm's length between my eyes and the church. If I look directly at the church, the thumb appears double; if I look directly at my thumb, the church appears double. But though both objects appear double under the varying circumstances, their images do not cross in both instances. If I look at the distant church, my *left* eye sees my thumb to the *right* of the church, and my right eye to the left, as may be proved by shutting either eye. But if I look directly at my thumb, which is the nearer object, my *right* eye sees the church to the right of my thumb, and my left eye sees it on the left of my thumb, as may be proved in the same way; and, if the following diagram shew the matter correctly, it will appear that the images of the distant object cannot cross. Moreover, intervening objects, in the experiment, do not appear to lose their proper relations as to distances or "planes by projection" or aught else.

The axes of the two eyes meet on the middle object, *y*, (see diagram, in next page,) the lines of the axes forming the angle, *a*, *y*, *a*.

The nearer object, *z*, being within

* We do not well understand Mr. Berry here; we have not said, nor did we mean to say, any thing relative to the changing of sides, at variance with his or Mr. Williams's conclusions.—Ed. Gaz.



the angle, and therefore to the right of the axis of one eye, and to the left of that of the other, is seen double; each image being removed from the line of the axis *inwards*, or towards the other eye, and the images decussate, because the left eye sees *z* to the right of its true position, and the right eye to the left; but the plane, or apparent distance of the images, must be the true plane of the object. It is seen double, because it has actually two apparent positions, when both eyes are engaged; for it is to the right of the axis of the left eye, and to the left of that of the right; and the degree of apparent removal of the image from the object, is in exact proportion to the actual distance between the object itself and the line of axis of the eye—that is, if the object be actually situated one inch to the right of the line of axis of the left eye, its image must appear just as much farther to the right of the object, or two inches in all from the line of axis—as in the diagram, from *b*, the line of axis, to *z*, the image, and from *z* to *c*, which is the same distance as from *b* to *z*. All will be reversed, of course, for the right eye.

But the images of the distant object, *x*, under the same circumstances, will be removed to the *outer* side of the axis of either eye, in proportion to the distance of the object from the lines of the axes. These lines are produced, yet form an angle, and their separation must increase with their length; these images, then, cannot cross. Moreover, the image on the *right* of the axis of the *right* eye, is that which the *right* eye receives, and the other that which the *left* receives. The actual position of the image will be, to the right eye, as much to the right of the object, as the true situation of the object is actually to the right of the line of axis of the *right* eye; and the apparent deviation will increase with the distance—that is, as the sides of the angle are prolonged.

The diagram can prove nothing with regard to "planes of projection;" I only mean by this that the experiment does not *appear* to shew any alteration of distances; yet this may certainly be owing to want of appreciation, from our actual consciousness of the distance and relations of objects.

While I have this opportunity, allow me to notice another optical fact, about which there seems to prevail some difficulty. In all works that I have met with, and in lectures, the *limbus luteus* and foramen of Soemmering are commonly said to correspond with the axis of vision; but this is the only insensible point of the retina; whereas, the accurate direction of the axis of the eye to an object, is supposed essential to distinctness of vision. If this be the case, its distance from the axis may be thus measured by means of its insensibility.

Let the arms be extended before the eyes, the thumbs being erect and close together; then shut, for instance, the *right* eye, the *left* being fixed on the *right* thumb. If the left thumb be now moved away from the right, it will soon be invisible, and will then, still moving on, presently be again seen. Now these distances may be measured. Suppose the right thumb just applying to the half-inch mark on a foot rule; the arms being extended, the thumbs are as

nearly as possible two feet from the eye. If the left thumb be now slid along the rule away from the right thumb (the left eye being fixed on this latter thumb), the left thumb will, when it has reached the $5\frac{3}{4}$ inch, begin to be invisible, first on its outer edge, then completely, and continue to be so till it has reached the $7\frac{1}{4}$ inch, when the outer edge again becomes visible; but the inner edge will not have emerged till it is on the $7\frac{3}{4}$ inch mark. The point, then, at $6\frac{3}{4}$ inches may be supposed co-incident with the centre of the *limbus*. Deducting the half inch which the right thumb covers, the $6\frac{1}{4}$ inches will be the measure of the base of a triangle, whose sides are, a line, two feet long, drawn from each point of the base, and converging so as to meet at the focus of the lens: such measures give an angle of 15°. These lines, produced beyond their decussation, would, allowing for the converging powers of the lens, give a proportionate angle behind the focus, and the retina, as a base, would form with them a triangle; the points of the base being the centre of the limbus, and the axis of vision. With two such fixed points, would it not be possible, somehow or other, to compute the converging power of the lens? The measurement of the angle will of course vary with the length of the sides, the base being given; and though the base will vary also, in proportion to its distance from the eye, it does not vary in the same ratio as the distance or length of the sides. I suppose, too, different lenses will measure different bases, since all are not alike. But I made my thumbs two feet all but half an inch from my eye.

If, sir, you consider these remarks worth publishing, you will oblige me by giving them insertion.

I am, your obedient servant,

GROVE BERRY.

NEW DESIGNATION FOR HYDROCEPHALUS.

To the Editor of the London Medical Gazette.

SIR,

FEW things are more to be lamented than the unnecessary multiplication of undefined terms in medical nomenclature; and it is, therefore, with much regret that I see Dr. Marshall Hall has made use of the superfluous name of "hydrencephaloid disease" to denote a set of symptoms which have always hitherto been called hydrocephalus, and which new name gives no more correct view of the actual nature of the disease than the old one did. The symptoms described in the cases recorded in your Gazette of August 4, are precisely those of what is generally called hydrocephalus, and were originally treated as hydrocephalus usually is by the greater part of practitioners. The substitution of the term hydrencephaloid disease for hydrocephalus, is about equivalent to converting the word inflammation into the two words inflammatory action, an unnecessary incumbrance of words, tending to produce confusion rather than precision. In the Doctor's recommendation of treating cases of this description (which, had they proved fatal, would on dissection most probably have displayed effusion in the brain) I heartily concur, as being more judicious than the excessive depleting measures employed under the idea of the symptoms being produced by cerebral inflammation; and indeed I had endeavoured to draw the attention of the profession to the fact of these cerebral symptoms being merely symptomatic of intestinal irritation and constitutional debility, and not those of inflammation, in a small treatise published in 1825*; and I hope, now my opinion is corroborated by the experience of Dr. Hall and others, a more efficient treatment will be generally adopted. The value of the cases furnished by Dr. Hall consists in demonstrating that the symptoms of cerebral affection were not dependent upon inflammation of the brain or its membranes, and did not require anti-inflammatory treatment, a point upon which I strongly insisted in the little treatise I

* "The few observations offered in the following pages are intended to controvert the doctrine of water in the brain being a distinct specific disease, and to oppose the prevalent opinion of the proximate cause of the watery effusion being inflammation. It has been the author's endeavour to shew that this symptom, water in the brain, is an accidental occurrence, taking place in a great variety of diseases, and as the consequence of numerous causes acting upon the cerebral organs, depending upon a certain condition of those organs, constituting a state of predisposition merely, without the presence of actual cerebral disease."—Page vii.

mentioned* ; and I think this will hold good in a great proportion of the cases which are classed under the head of acute hydrocephalus. That cerebral inflammation sometimes takes place in children, cannot be denied ; but it is a matter of great consequence not to include under this term all states of cerebral disorder, and not to employ measures which, so far from being efficacious, are highly detrimental, instead of the stimulant and tonic treatment so successfully practised in the cases sent by Dr. Hall.

I am, sir,
Your obedient servant,
WILLIAM SHEARMAN.

Northampton-Square,
August 9, 1832.

CHOLERA — EFFICACY OF BLOOD-LETTING.

(Being a Report transmitted to the Army Medical Board.)

Dublin, 27th July, 1832.

SIR,

I BEG to state the satisfaction I feel in complying with your wish "to have such observations on the subject of cholera as may be thought useful," or in any way add to the information already acquired respecting this fatal, and I may add, in many instances, unmanageable distemper. I have only to express my fears that the result of my experience in the disease during the last six weeks will do but little towards the general stock of information ; and at the same time to say, that though I have seen much of the calamity in India, and latterly here, in my frequent visits to the General Cholera Hospital, as well as to the military ones, I shall confine my remarks principally to the cases which came immediately under my observation in the reserve of the 12th Foot, and in such details as were under my own care as staff-surgeon.

My immediate vicinity to the depôt of the 12th regiment, and cordial professional feeling with assistant-surgeon Dick of that corps, enabled me to see his cases in most instances, at the very commencement, and to continue my observation with him to the termination in each. The general results of *all*, have led me to a conviction of the propriety of *blood-letting*, and in no case of genuine cholera would I delay this remedy unless actual collapse, or a very near approach to it, was evident. Of course the constitution, sex, age, &c. of the patient, are to be considered ; and the greater number of characteristic symptoms present, *more particularly profuse purging*, the greater caution as to abstraction of blood, as in such cases it is sometimes an abuse of a remedy to use it, or, at all events, to persevere in it during actual collapse. By the latter term, I mean *a total cessation of pulse at the wrist, with a temperature of skin below natural:* if attended with chilled tongue and breath, blue colour, altered countenance, and sunken eye, so much the more intense is the collapse. I am thus particular on this point, from some referring the term to an aggravated state of debility, sunken countenance, &c. *without cessation* of arterial action in the extremities ; and asserting they have recovered patients from actual collapse, accompanied by total loss of pulse. *I have seen no such recovery in any military patient*, and believe it to be very rare in any. In the General Cholera Hospital, I am satisfied, it sometimes happens ; and I rather believe impoverished, or poor fed individuals, but of sound constitution, have a better chance of recovery when consecutive fever succeeds collapse, than well fed and robust persons ; and this, in my opinion, strengthens the propriety of bleeding in this class at the very onset.

From the previous sentiments it will be perceived (that like many others) I espouse a favourite remedy: it is so: at the same time I am not led away with the idea that bleeding is applicable to all, or rather that it can cure every

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* "My object is to direct the attention of practitioners to the view I have taken of acute hydrocephalus ; that of not considering it as a proper idiopathic disease, but the effect of some previously existing disease, the most frequent of which is fever ; or as the consequence of increased exhalation, the natural result of simple increased vascular excitement, arising from various causes, acting in children of debilitated constitutions or irritable habits ; and to point out the obvious inutility of combating a single symptom, in place of embracing a comprehensive view of the essential character, usual progress, and natural termination of the preceding or existing disease."—Page 123.

"As general irritability is found to exist in an eminent degree in children of a weakly habit, medicines which produce a tonic effect are frequently of very great service in counteracting the predisposition to this complaint."—Page 117.

case; on the contrary, from extensive Indian, as well as late experience, I am convinced that cases of cholera occur in which no practice will avail. It is evident I advocate *early bleeding;* but to go into particular varieties, or describe minutely the shades of difference in each case in which it is applicable or not, is beyond the limits I prescribe myself. As a general observation, however, I imagine it may be resorted to without fear, in every case in which the skin retains a moderate temperature, and the pulse remains distinct at the wrist. The more violent the spasms are, in most cases the more advantageous the remedy; but tact and observation must teach this. In addition to bleeding in the cases alluded to in this paper, calomel and opium, with purgatives, in the more advanced stage, were the medicines chiefly used, and such other adjuncts as were appropriate to particular symptoms. All the cases extensively bled recovered, without running into collapse, and the patients were out of danger in from six to twelve hours, though still requiring attention to restore the natural secretions, &c. &c. The fatal cases in the regiment (12th) were three; one only of these was bled, and that but to six or seven ounces; it came almost by drops, *and more could not be obtained,* though the heat was not materially reduced, nor the pulse feeble at the moment, or for a long period after; but the case was one of great intensity, with such sudden and complete prostration of strength, and uncontroulable purging, that there was no hope from the first, and the man, though robust and temperate in habits, died in eight hours from being seen. The other cases were nearly similar in severity, though not so rapid: neither were bled: both were removed to the Military Cholera Hospital as soon as possible, when the disease was characterized, one dying shortly after his arrival in the state of collapse, and the other in that of reaction, having survived to the third day. I shall insert the names and results of all the cases of cholera, in a tabular form, at the bottom of the report, which will more clearly elucidate the practice. With one exception, premonitory symptoms were acknowledged to a greater or less period: the exception was a sergeant; and this circumstance makes me feel some doubts as to his veracity; as it is probable, he being charged with minute instructions "to detect, and report any cases of purging among the men," might hesitate to admit such error in his own person.

It may further be right to observe, that the reserve of the 12th regiment arrived in Dublin the 4th June, having come from Drogheda, *an infected quarter.* Two fatal cases of cholera occurred while at the latter place in May. Neither were bled, and both died in less than ten hours from the first development of the symptoms. The first case occurred to a healthy plethoric young man, a prisoner in the county gaol, under sentence of a court-martial. Being in a distant part of the building, and locked up for the night, he was not seen until some hours after the seizure, when he was in a complete state of collapse: he died in about two hours after his removal to the hospital. The other case occurred to a man in the Regimental Hospital in the last stage of consumption. He was suddenly seized with the characteristic purging, and died in two hours. He had neither cramps nor vomiting; but the discharge from the bowels, the voice, countenance, &c. were very distinctly marked. In Dublin the first case occurred on the 20th June, sixteen days after arrival, and I should presume had no connexion whatever with Drogheda, as the disease had long existed in Dublin, and one or two cases had occurred in the barracks occupied by the 12th, previous to their arrival.

In addition to the cases of actual "epidemic cholera" since the 20th June, numerous severe cases of bilious cholera occurred in the men of the 12th, as also several of severe diarrhœa. Those cases, taken collectively, amounted to nearly fifty. In three-fourths of them, the subjects being young and robust, Mr. Dick abstracted blood, and with the very best effects on the immediate attack, as well as having the ulterior satisfaction of seeing, *that in no instance in which bleeding was had recourse to, did such affections degenerate into the epidemic character,* and which I am satisfied would in some instances have been the case, but for his great attention to this point. The remedies used, in combination with bleeding, were emetics when the stomach appeared loaded, or the patient harassed by retching. Immediately after, calomel

and opium, in the proportion of six or eight grains to one of opium, and in a few hours a purgative of castor oil, or a full dose of rhubarb, with aromatic confection. In the more mild cases, "where there was merely purging," with the abstraction of blood, a draught, composed of Pulv. Rhei. gr. xx. Magnes. gr. xij. Tinct. Opii, gtts. xv. ad xx. in Aq. Menth. Pip. ℥iss. was all that was required.

In this frequent occurrence of disease in the stomach and bowels *(Cholera being prevalent,)* it is evident that the strictest attention was necessary, many of the cases being of extreme violence, and only to be discriminated by minute attention to the symptoms: at the same time that the change from this state to genuine epidemic cholera was so insidious in a few, as to render it scarcely possible to mark the difference, till the distressing reality was rendered certain, by increased debility, loss of voice, &c. and a change in the evacuations to the characteristic appearance.

To render this more evident, I shall relate the case of Staff-sergeant Kelly, who is immediately attached to me as clerk, and the detail of which will show generally the means used in all the cholera cases; allowing some variety for particular circumstances:—

On the morning of the 4th instant, after examining a few recruits, and when about to leave the inspection room, Sergeant Kelly mentioned to me that he had been purged six or seven times from midnight to five o'clock that morning: from which time he had felt well, except thirst: the stools were watery, passed without griping or pain, but nothing known as to colour, from having gone to the privy. Countenance was natural, tongue clean, and pulse regular. He had been about his usual duties from early in the morning; and on the eve of my leaving, mentioned the circumstance more as conversation than as complaint. I directed him to keep quiet in his room, and gave the following draught:—

℞ Pulv. Rhei gr. xx. Mag. gr. x. Tinct. Opii gtt. xv. Aq. Menthæ Pip. ℥iss. M.

This was rejected in about ten minutes, but nothing beside. I now made him go to bed, and when there, gave Pil. Hyd. Subm. gr. vj. Ext. Opii. gr. j. and left him perfectly comfortable *at noon;* in fact, the necessity of being in bed did not meet his views.

At 2 o'clock, I was absolutely shocked at his altered countenance: his eyes sunk, though without dark areola; pulse small and 120; skin below natural temperature; feet cramped; and both vomiting and purging of clear watery fluid; his tongue was whitish, but not cold; he had made urine freely in the early part of the morning, but none since. I instantly tied up his arm, and made a free opening in the median vein; but though prominent, and pulse steady at the wrist, only the few drops between my thumb and the ligature flowed out; and this of a dark grumous description. By frictions upwards, and to the hand; applying a warm sponge occasionally to the orifice; by perseverance and encouragement, the flow at last improved from trickling along the arm to a small stream; on which, I made a second opening in the outer vein; both streams gradually improving, till I had abstracted full 32 oz. when it poured out in a rapid and uniform flow from both orifices, shewing a change in the centre of the stream to a more florid colour. I now could have obtained any quantity; but the cramps having subsided, and the man not robust, I tied up the arm. He vomited during the operation to the extent of nearly a quart of fluid, thrown up with great violence: part of this was evidently drink. He now had one of Dr. Stevens's powders, but to half the quantity, namely, Supercarbon. Sodæ gr. xv. Muriat. Sodæ gr. x. et Chlorate of Potass. gr. 3½, in two ounces and a half of water. This was repeated every two hours during the afternoon; and Hyd. Subm. gr. iv. with Op. gr. ss. every *four* hours. As he complained greatly of thirst, lemonade, whey, or acidulated barley-water, were allowed, according to his wish. His feet and legs were firmly rolled with a flannel bandage, and warmth applied to the stomach. He seemed to require nothing further.

At 11 o'clock at night his countenance had improved; he had three motions to this time, in all about a pint, and characteristic. The opium was now omitted in the pills, continuing the calomel alone.

On the morning of the 5th he was better: two stools passed, though still very watery and had a yellow tinge; and

he had made about three ounces of high-coloured urine; his pulse was still quick, and thirst great. At 10 A.M. he had a purgative of ʒss. Ol. Ricini in mint-water, which produced some blackish green evacuations in the afternoon, these changing gradually from all shades of black and green to natural, about the morning of the third day, when he had no complaint but debility. The calomel had been continued at longer intervals, and effervescing draughts in place of the saline powders.

In this case, which was both insidious and severe, I am satisfied that collapse, and the dangerous train of concomitants, were totally suspended by timely bloodletting; and equally certain, that had much longer delay occurred, the dangerous and fatal depression would have taken place to the almost total extinction of hope. In the foregoing remarks, I have purposely abstained from noticing other useful and powerful remedies, applicable to particular stages of the malady, and by some strongly recommended at the very commencement—I mean emetics of mustard, either pure or combined with salt; or the latter, in milder grades of the malady, alone. In one case (that of Pat. Byrne) inserted in the table, I used the salt emetic in eight hours after the bleeding: at this time, he complained of great loading and fulness at the epigastrium, and had ineffectual and slight vomiting of watery fluid. He had half an ounce of common salt in four ounces of tepid water, and no effect being produced in ten minutes, this was repeated. A large discharge of fluid now took place, with great relief to his sensations; and this was *yellow* in colour, and he said *bitter* tasted, such not having been the case in the previous ejections.

My experience of mustard emetics will not admit of my entering on the subject, though I believe them to be most useful in many cases: neither shall I notice the employment of the carbonate of ammonia in collapse, so strongly recommended. The principle on which I put these few hasty lines together, was that of actual experience of a particular and, in my mind, paramount remedy; and therefore I shall not enter on others, however valuable.

ANTHONY C. COLCLOUGH,
Staff-Surgeon.

To Dr. Renny,
Director General of Hospitals.

TABLE *exemplifying the Cases of* CHOLERA EPIDEMICA, *alluded to in Report.*

Regts.	Men's Names.	Age.	Extent of Bleeding.	Results and Remarks.
12th	Edm. Gidney	20	6 oz.	Rapid Collapse, and Death in 8 hours.
—	Owen Lesley	21	—	Collapse, Re-action, and Death 3d day.
—	Thomas Waters	27	35 oz.	Recovery.
—	John Burrows.....	22	—	Died in Collapse within 10 hours.
—	M. Stephenson	27	40 oz.	Recovery. — Cramps most violent, and all symptoms well marked; but discharge from stomach was slightly coloured throughout.
—	Serg.-major Daley ...	39	30 oz.	Recovery.
—	M. Hopkins.....	26	24 oz.	Recovery.
59th	Staff-serg. John Kelly	28	32 oz.	Recovery.
12th	Pat. Byrne	27	22 oz.	Recovery.

FACTS AND OBSERVATIONS
RELATIVE TO THE
Symptomatic Modifications and Communicability of the Fever of the Pestilence commonly called the
CHOLERA MORBUS.

BY WILLIAM JOHN THOMAS, M.R.C.S.

THE following observations and facts relative to the disease prevailing so generally over the United Kingdom, are drawn up from memoranda made upon one hundred cases which occurred principally in the thirteenth district of the town of Liverpool, as well as from a few private cases scattered in the neighbouring places. All the cases occurring in the thirteenth district were under the immediate supervision of four honorary

medical officers, appointed by the Board of Health, one of whom, the talented Dr. Traill, is, I regret to state, about to leave the town, having been appointed to the Chair of Medical Jurisprudence in the University of Edinburgh. Although I rejoice to see this scientific philosopher receive that honourable reward which his high attainments have so long merited, yet I cannot but deplore the loss we are about to sustain by his removal from town.

Having made notes of the cases which came immediately under my charge, I have thought it expedient to submit the recorded facts to the medical public. In a statistical point of view they have their appropriate value, and in medical records the accumulation of these recorded facts is duly appreciated.

Previous to the commencement of this pestilential visitation, an unusual number of cases of typhus fever prevailed, the majority of them exhibiting the usual symptoms of typhus gravior; and I am persuaded, if the gigantic invisible had not usurped the sceptre of morbid supremacy, we should have seen more of our old antagonist than we have witnessed this season. The typhus after a short period abdicated its imposing attitude in the rank of diseases, and was succeeded, after an interval of a few weeks, by that reign of devastation and woe which I trust never again to witness in the course of my medical career.

When the pestilence commenced its ravages in the capital of Ireland, we were in the momentary expectation of its appearance in town. The constant communication between the former place and Liverpool rendered it almost impossible for the latter to escape, although a quarantine was imposed upon all vessels sailing from the infected port.

The authorities of the town had provided hospitals for the accommodation of the sick; and a number of the most respectable practitioners having volunteered to aid the poor parishioners in the forthcoming storm, had districts assigned to them, in which they officiated as honorary medical officers. The arrangements were conducted by the Board of Health, constituted by the magistrates, the medical officers of the hospitals, and the Rector of Liverpool.

A curious circumstance may be alluded to in this place, and that was an epizootic mortality which preceded the pestilence. I do not assert that this circumstance had any connexion with the disease; but as a concomitant fact, it may be stated, that a number of flies, of a wasp-like appearance, were found scattered in many parts of the town. Some persons maintained that the deaths of these winged inhabitants of the air were caused by a pestilential contamination of the atmosphere; but upon this point I must decline advancing an opinion. The months of March and April were hotter than is usual at that season of the year; but a week or two previous to the arrival of the fever of the pestilence, a strong east wind swept the town, accompanied by cold and damp weather. About the commencement of May I remarked an unusual prevalence of cases of English cholera, and one in particular attracted my attention. On the 12th of the same month the Board of Health announced the existence of the disease. I was present at a lecture delivered by Dr. Baird, in the Medical Library, to the members of the profession, upon several cases of cholera which he had witnessed about this time in Liverpool, when Dr. Traill informed me after lecture that there was a case in the district committed to our charge, belonging to a private practitioner, who politely permitted me to visit his patient. This patient died in the consecutive fever (as it is called), on the morning of the following day. Upwards of twelve days elapsed before I had an opportunity of witnessing a second case, and nearly twenty days before the third came under my care. After this period the disease prevailed over the district, and the cases multiplied rapidly.

Having made these preliminary statements, I shall now proceed to relate the facts upon which the observations I have to offer will be founded. Of one hundred cases, selected as they stand first on the list, fifty-five were females, and forty-five males. Under the age of ten years, there were sixteen persons affected; between ten and twenty, there were four cases; between twenty and thirty, there were twenty-one individuals; between the ages of forty and fifty, fifteen persons were affected; between fifty and sixty, nine were attacked; between sixty and seventy, eight cases; and between seventy and eighty,

only one case occurred. It may be here remarked, that the majority of cases occurred between the ages of thirty and forty. It has been frequently observed, that the aged were the principal victims of this disease; but as the greatest number of cases occurred in my practice between the ages of thirty and forty, I was somewhat at a loss to reconcile the apparent discrepancy in the evidences, between the recorded observations of other practitioners and the facts which presented themselves to me. However, upon examining the tables of mortality in my note-book, I found that my observations corresponded in a great measure with those of my predecessors. I have arranged the cases in a tabular form, for the convenience of reference. The first and second columns refer to the age of the patients; the third to the number of cases; and the fourth denotes the number of deaths.

10	16	5
10 and 20	4	1
20 — 30	21	4
30 — 40	26	6
40 — 50	15	7
50 — 60	9	4
60 — 70	8	7
70 — 80	1	1
Total	100	35

It will be evident, therefore, from the inspection of this table, that although the greatest number of cases presented themselves in persons between the ages of 30 and 40, yet the amount of mortality was the largest between the ages of 60 and 80; for out of nine cases, only one recovered, and that person under 70. Between the ages of 20 and 30 the largest proportion of recoveries took place, which was about 73 per centum. The total amount of recoveries was 65, and there were 35 deaths; being a mortality of rather more than a third in the total number of cases.

It might here be remarked, that the great mortality (although nearly the average standard) would appear to imply professional mismanagement of the cases, or inattention upon the part of the medical attendant. I think it, therefore, but justice to myself to state, that a very large majority of the cases were treated under the greatest disadvantages. The patients resided principally in the most loathsome abodes of wretchedness, depravity, and vice,—in cellars polluted by prostitutes of the most abandoned class, and filled with filth and every abomination that the most abject penury could impose; — hot-bed of impurity and infection, the very idea of which is sickening to the memory, and which nothing but the imperative sense of the public duty committed to their charge, could have induced the medical gentlemen to enter! The magistrates ordered these places to be whitewashed and purified, but in a few days they were as filthy as usual. Many of these miserable people preferred their polluted abodes to the comforts of a clean and well-ventilated hospital. I visited some of these patients three or four times during *their* day—(for one day, or a few hours, generally settled their concerns with this world)—and I had the mortification of beholding the cloud of death gradually spreading over their countenances, uninterrupted by the administration of medicines, which, in a purer atmosphere, might have been attended with the happiest effects. Under these distressing circumstances, I am surprised that so many recovered.

I cannot leave this subject without expressing the satisfaction I felt at the benevolent operations of the District Provident Society—an admirable institution, founded by the Rector of the parish for the express purpose of supplying food and raiment to the victims of penury and want. This excellent Society co-operated with the honorary medical officers, and at their recommendation such relief was administrated to the survivors as their physical necessities required.

We shall cease to wonder, if in such places as these something like contagion did not present itself. It is not my intention to enter into a metaphysical disquisition upon the abstract question of contagion,—a subject which has afforded so much speculative amusement to the medical philosophers of the present day. I cannot, however, forbear expressing my surprise at a statement made by Drs. Hamett and Otto Daun, and attested by the British Consul at Danzig, that they had never found an instance in which persons affected with cholera had had any communication with each other. This, if I remember aright, was the substance of their declaration. The following facts relative to the communicability of this disease came under my observation. The profession may deduce their own

inferences from the premises. I shall probably take the liberty of attaching a few remarks, by way of observation, upon the doctrine of contagion, which the cases appear to demand.

A TABLE *of Twelve distinct instances of the apparent Personal Communicability of the* CHOLERA*.

No. I.

a. M. A. M. died in the blue stage on the 18th June.
b. The husband of *a*, died in the blue stage June 21.
c. An attendant on the funeral of *a*, died June 21.
d. The nurse of *a*, attacked and recovered June 24.
e. The coffin-maker to *b*, attacked and recovered June 24.

No. 2.

a. Residing in a cellar, died June 19.
b. The wife of *a*, died June 23.

No. 3.

a. Attacked and died June 24.
b. The mother of *a*, June 25 †. Died.
c. The father of *a*, June 25. Recovered.
d. The nurse of *b*, sent to the hospital, recovered.
e. The mother of *d*, sent to the hospital, recovered.

No. 4.

a. Died June 26.
b. Father of *a*, attacked with Dysentery June 26. Recovered.
c. The partner of *b*, attacked with cholera July 13. Recovered.
d. A carter, in the employment of *b*, attacked with cholera July 17. Died.

No. 5.

a. Attacked June 27. Recovered.
b. Sister of *a*, June 28. Died.

No. 6.

a. Attacked June 29. Died in the blue stage.
b. Husband to *a*, attacked July 25. Died in the blue stage.

No. 7.

a. Attacked July 7. Recovered.
b. Mother of *a*, July 8. Died.
c. Child of *a*, July 8. Died.
d. The father of *a*, July 10. Recovered.

No. 8.

(All inhabitants of the same house.)

a. Attacked July 6. Died.
b. Attacked — 10. Died.
c. Attacked — 11. Died.
d. Attacked — 12. Died.
e. Attacked — 12. Died.
f. Date of attack unknown. Died.

No. 9.

a. Attacked July 14. Died.
b. Wife of *a*, July 15. Died.

No. 10.

a. Attacked July 15. Recovered.
b. In the same house, July 22. Recovered.

No. 11.

a. Attacked June 23. Recovered.
b. The mother of *a*, July 12. Died.
c, The sister of *a*, July 25. Recovered.

No. 12.

a. Attacked July 28. Died.
b. Attacked August 4. Recovered.
c. In the next cellar, August 4. Recovered.
d. In the next cellar to *c*, August 7. Under treatment.

Now here is a mass of evidence relating to the subject of contagion. All the individuals were in communication with each other. No. 8 refers to the lamentable event of a whole family swept away in less than a week by the same disease. I was informed of the death of *f* by the surgeons of the Cholera Hospital. This patient resided in the same house with his ill-fated predecessors. I might multiply these instances of apparent contagion, but sufficient has been adduced to satisfy every reflecting and unprejudiced mind that something more than mere concomitant powers have been in active operation to produce this chain of morbid pheno-

* Any respectable member of the profession wishing to investigate the veracity of these facts, will be afforded every facility in my power to supply. A motive of professional delicacy, which I would willingly extend to the poor as well as to the wealthy, induces me to omit the names of individuals when not imperatively called for by peculiar contingencies or adventitious circumstances.

† This female was on the point of her confinement when attacked with cholera: she was delivered of a dead child, and expired herself a few hours afterwards, in the consecutive fever.

mena. It may, indeed, be stated that the same epidemical cause might have affected the same individuals at a given identical period; and that the time of the incubation and development of the morbific germs may have been arrested or precipitated by the idiosyncrasy of the individuals. My intention, however, is to state facts, and not to enter into a metaphysical speculation upon the abstract question of contagion; and although I have formed an opinion upon the subject, which I think future experience will not probably compel me to abandon, yet I do not think it expedient to state that opinion at the present moment. As a candid and unprejudiced narrator of facts and observations, relative to the communicability of this pestilential fever, I feel myself compelled to state that circumstances and cases have taken place exceedingly favourable to the propagation of the disease, in which no infection has followed. I shall relate one case to the point. I attended an elderly man on the 19th July, who died in the blue stage after an illness of about twelve hours. His wife was intoxicated during the day on which he died, and watched at his bed-side until the time of his death, which was in the evening. She then removed the dead body, and boldly entered the bed, sleeping between the same coverings under which her husband died. This person has not been infected up to the present date (August 10th), although a neighbouring female, who assisted her to remove the body, took the infection and went through the blue stage, from which she is now recovered. The neighbours informed me that the widow of the deceased had not been sober for a single day since her husband's death, and that "she did nothing but drink gin and eat gooseberries!" Whether we are to attribute her escape to the insusceptibility of her constitution, or to the permanent stimulus of the spirit, administered in almost perpetual potations, I shall leave the curious in these affairs to determine; but I think it will be generally allowed that the prophylactic virtue cannot be attributed to the gooseberries.

During the three months that this disease has prevailed in Liverpool, I have experienced in my own person almost perpetual annoyance from symptoms of gastro-enteritic irritation. From a constant communication with the sick, I was frequently affected with nausea and vomiting, occasional febrile excitement, headache, and wandering pains in the muscles. One evening I had sat by the bed-side of a poor person, watching the gradual progress of dissolution, and administering with my own hand brandy to the patient. I left her in the last stage of the consecutive fever, in the momentary expectation of her demise. The body exhaled an odour of an indescribable character, peculiar to this fatal disease. I retired to my study at that late hour, for the purpose of arranging some private papers, when I suddenly felt a sensation of pungent pain in the stomach, attended with nausea and dark mental despondency, as if a gloomy cloud had suddenly overshadowed the faculties of my mind. At this moment a rap came to the door, and I was informed that a messenger had arrived announcing my patient's death. Having transacted the business of the evening, I retired to rest, having first taken two glasses of Madeira, in the hope of banishing the distressing sensation from the stomach. It was then midnight. I was awakened out of a tumultuous slumber about half-after two in the morning, by an acute pain in the centre of the feet, as if some dislocating power had been applied. The toes were so severely cramped that I was obliged to replace them in their proper position with my hand. The nausea also had increased to faintness, and it was speedily followed by a profuse watery evacuation from the stomach, of a sweet and unpleasant taste. The vomiting was succeeded by several copious fluid dejections; and, finding how affairs were proceeding, I took two grains of solid opium and about six of camphor, which were washed down with a dose of strong brandy and water. The medicines arrested the disease. I prevented the recurrence of so serious an attack by the prudent use of port wine, the sulphate of quina, and strict attention to diet.

Having already exceeded the limits assigned in your journal to original communications, I must abruptly conclude by stating that I may, at an early period, again trespass upon your indulgence, by entering into a physiological examination of the symptomatic modifications of the fever of the pestilence. I shall also take the liberty of making a few practical observations upon the

comparative value of the different modes of treatment, and of the efficacy of the medicines employed in the different stages of the prevailing epidemic.

Liverpool, August 10, 1832.

HINTS ON THE SALINE TREATMENT OF CHOLERA, &c.

To the Editor of the London Medical Gazette.

SIR,

I HOPE you will allow me, in as few words as possible, to offer two or three suggestions on the treatment of cholera, which I have withheld for several months, in deference to the crowd of correspondents who appeared to be waiting for admission, and who, I was aware, had prior claims to your notice.

1. Would it not be an improvement on the present mode of administering the saline remedies proposed by Dr. Stevens, to give them in a state of effervescence, as in the following formula?—

℞ Sodæ Bicarb. ʒiv. Sodæ Mur. ʒiij. Potassæ Chlorat. gr. xxx. to xl. Aq. Puræ, f ℥vj. M.

℞ Acid Muriat. pur. ♏xc. Sp. Ether Nit. ʒiij. Aquæ Pur. f. ℥vss. Misce.

An equal quantity of each to be mixed, and given in a state of effervescence. Two table-spoonsful will contain the usual dose. The acid neutralizes about a fourth part of the bicarbonate of soda.

2. Sulphuric ether is sometimes prescribed in cholera; but I would suggest that the *nitric* should always be preferred, from its decided tendency to promote the urinary and cuticular secretions. Perhaps the muriatic ether may be still better; its properties appear to be very similar to the nitric. No preparation of it being admitted into our Pharmacopæias, it is seldom prescribed, but it is a favourite medicine with a few practitioners (under the form of Clutton's febrifuge spirit) in fevers of a low type.

3. Is not the supposed efficacy of chlorate of potash in this disease mainly attributable to its *diuretic* property? Are not its medicinal effects precisely similar to those of nitre?

4. The common aperient salts (Epsom and Glauber) are still in daily use in places where the cholera prevails, notwithstanding they have been denounced by the Board of Health, and other authorities. Might not those artificial Cheltenham, and other compound salts, which contain muriate and carbonate of soda, in combination with the sulphates, be safely recommended as a substitute to those who prefer saline aperients? Such compound salts as possess an effervescing property, and contain an excess of carbonated alkali, seem peculiarly eligible. The following is a formula of this kind:—

	Parts.
Dried Sulphate of Soda ...	20
——————— Magnesia	10
—— Muriate of Soda ...	30
—— Bisulphate of Soda	10
Bicarbonate of Soda.....	7¾
Proto-sulphate of iron	¼ Mix.

Dose, a tea-spoonful in a glass of warm water.

That the use of such an aperient should be not merely safe, but possibly in some measure a preventive, is in accordance with the saline theory of Dr. Stevens, the analysis of choleric blood, and the testimony of the physicians at Moscow, that those persons who took a course of artificial mineral waters escaped the cholera when it prevailed there.—I am, sir,

Your obedient servant,

H. B.

August 11, 1832.

REMARKS ON THE STRUCTURE OF THE PLACENTA, AND THE STATE OF THE MATERNAL VESSELS.

By JOHN BURNS, M.D. F.R.S.

Regius Professor of Surgery in the University of Glasgow*.

IN a former paper, I described some of the preparations, in Dr. Hunter's museum, intended to illustrate the structure of the placenta: I will now offer a short account of the appearance of the parts in their recent state, which will give more satisfactory information than can be derived from the inspection of preparations. Having this week, by

* The date of this shews it is not in answer to Dr. Lee's paper: see our Number for August 4. It was sent to a gentleman who had left London, which led to the delay.—ED. GAZ.

the kindness of a friend, obtained the uterus of a woman who died in the end of the sixth, or beginning of the seventh month of pregnancy; I carefully injected both its arteries and veins, but not those of the fœtal or umbilical system. I then, with the assistance of Dr. Lawrie and Mr. Rainey, and in presence of several other gentlemen, opened it in the university. Dr. Jeffrey afterwards saw the parts.

The uterine arteries were numerous, tortuous, and as large as stocking wires. The veins, or sinuses, varied in breadth, from a quarter to five-eighths of an inch.

On separating the decidua and placenta from the uterus, the arteries were found to be passing in great number into the decidua, and through it, in all the district of the placenta. They continued of the same size as in the substance of the uterus, and formed coils, some of which were half an inch long. Then they opened into the substance of the placenta, either terminating on its surface, or barely entering into its texture*. They at once ended in, or opened into, the cells of the placenta, which were finely filled with grains of injection all the way to the fœtal surface. A section exhibited no appearance of extravasation, even when examined with a glass, but the surface was distinctly granulated.

The portion of artery between its exit from the uterine parietes and its termination in the placenta, or the coil, was not of the same texture as an artery in other parts, but the coats were, like the decidua, soft and thin†.

The sinuses, like the arteries, proceeded from the internal surface of the uterus, but did not pass perpendicularly through the decidua, or go straight and directly to the placenta: they ran more or less obliquely, and for a greater or less length, between the uterus and placenta, or membranes; sometimes for considerably more than an inch; and one sinus, nearly three inches long, skirted to that extent the margin of the placenta. Many of these sinuses were so well filled, that although altogether within the uterus, they, from elevating it externally, appeared as if contained in its substance.

The continuation, or tract of the vein or sinus, after leaving the uterus, and entering the decidua, varied in breadth from a quarter to half an inch. The coats were thin and soft, as if the injections were contained in a canal, formed between, and by two layers of decidua.

On tracing a sinus to its termination, on the surface of the placenta, it was found, like the artery, not to enter as a trunk, but to end by application to the cellular structure, which was distinctly injected from it.

Hence, a section of a lobe of placenta exhibited throughout a granulated surface, the colour of the arterial or venous injection predominating according to the set of vessels which had been best filled.

Intermixed with these injected cells was a quantity of fibrous-looking substance, of a red colour, consisting chiefly of the terminations of the uninjected fœtal vessels, which were very numerous and minute in their subdivisions. This dissection proves distinctly the intimate structure of the placenta to be, as Dr. Hunter supposed, cellular in the maternal portion, and arborescent, or branching, in the fœtal portion. It proves the existence of intervening portions, of soft canals, going from the openings of the arteries and veins, on the inner surface of the uterus, to the cells of the placenta. These canals, when injected, may be left attached, either to the uterus or placenta, but are with equal readiness separated from both. We cannot trace them, as trunks, into the placenta, for they terminate in cells, which they cover; neither can we always expect to find them adhering to, or projecting from, the uterine or placental surface, being so easily broken or brushed off. It also illustrates the readiness with which uterine hæmorrhage may be produced, by the rupture of one of these fragile portions, especially of the sinuses. Strength and defence are afforded by the intimate adhesion of the ovum to the uterus; but if separation take place the vessels are left unsupported; and if not necessarily torn in the act of separation, they must be soon ruptured, and blood will be discharged. The coils of the arteries may

* I do not know, from examination, whether, at an earlier period of gestation, the vessels may not penetrate farther.

† In a strict sense, we should say that neither the uterine arteries nor veins passed beyond the parietes of the uterus. The canals, which continue the circulation, are new formations, and of quite a different nature.

also render them less apt to be torn by any pressure on the uterus, or change of shape produced.

Any great diminution of size of the uterus, and contraction of its fibres, must tear the connexion with these delicate vessels; and therefore, after the delivery of the child, the secundines are easily thrown off, although large vessels pass between the womb and placenta.

As a considerable portion of decidua remains attached to the surface of the uterus after the placenta is expelled, and as fragments of those vessels are contained in it, and blood coagulates in these, as well as in the decidua, we find that after delivery the uterus is lined with a pulpy-looking tenacious coating, of a dark colour, which has sometimes been mistaken for slough or gangrene. This coating lessens the risk of hæmorrhage.

An important subject still remains for investigation — namely, the process by which the blood circulates through the deciduous portions of vessels, and in the cells of the placenta.

Glasgow, July 28th.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Sources of Health and Disease in Communities; or, Elementary Views of "Hygiène," illustrating its importance to Legislators, Heads of Families, &c. By HENRY BELINAYE, Esq. Surgeon Extraordinary to Her Royal Highness the Duchess of Kent, &c. &c. 1832.

THIS is one of the most interesting little volumes that we have seen for some time: the subjects of which it treats are of vital importance, and the mode in which it treats them is brief and to the point. Mr. Belinaye has done well in discussing and laying before the public the topics which we find here, apart from the vast variety by which they are commonly surrounded in the best-known books of medical jurisprudence: medical police and *hygiène* demand a separate and special degree of consideration.

In the first chapter several curious items are noticed, such as the supposed influence of the planets, and the sun and moon—the difference of season—the effects of the presence and absence of light, and so forth; but the second and third chapters are on a subject—the Laws of Propagation—which is particularly attractive. We must make a few extracts.

"With respect to the present subject, there are only a few points which legislation can reach. On all others, therefore, such instruction should be imparted, as may enable conscience and religion to supply its place. There is another power, of which we wish we could command the aid,—which consists of the caprice of fashion in the rich, and blind imitation in the vulgar. Through its medium, J. J. Rousseau was enabled to compensate for the injury done to society by the immoral tendency of his writings. Having previously captivated the minds of his contemporaries, by the seductive charms of his "Nouvelle Héloise," and by his other writings, he produced the most salutary revolution in the physical education of the youth of his time.

"The physical improvement or degradation of the race of man, like that of animals, appears to work, or revolve, chiefly on this one great principle—inheritance. Let philosophers revolt at the doctrine of the inheritance of evil, of which religion establishes the belief—or at that of property, which legislation has consecrated—it is certain, that inheritance, whether of good or of evil, is the universal law of nature. If we look at a succession of men, nearly related—like the first series of the Roman emperors, so often cited—we find, if we may venture on the comparison, the same resemblance of feature (in their statues), and of disposition (recorded in history), that an Arab will show in the pedigree of a favourite horse. The noble features of better princes—those, for example, of the house of Lorraine, now reigning—no less resemble the portraits of their ancestors. Singular tribes of men, like the gypsies, have roamed throughout the world for centuries, unchanged in their lineaments, in spite of their intercourse with so many different nations—because habits, or laws, kept their marriages exclusively within their own caste.

"Unions of individuals of different

races greatly influence the breed. And, first, it alters the breed. Of this we have a striking instance in the union of the white and the negro. The mulatto, who derives his origin from the first admixture of races, is both whiter, his hair less woolly, he is straighter limbed, and at the same time more intelligent, than his African mother—the distinctive characteristics of the primary race continuing to diminish at each remove, although the creole will be able to tell you at first sight how many degrees one of the hybrid race is removed from his first origin.

"Union of individuals of different races improves the breed, and, as we shall presently repeat, increases the number of male offspring,—a circumstance which is always a proof of vigour, and a source of superiority. An abhorrence, we might call instinctive, pervades mankind with regard to marriages betwixt near relatives. Although at first prevalent, from necessity, for the increase of the species, it has justly fallen under the anathema of the Canon law, for it tends to the degeneracy, physical as well as moral, of mankind. Some tribes still inhabit the earth, among whom marriages betwixt near relatives take place: their women are comparatively barren, and their progeny dwarfish. If we select similar examples in modern times, either in this or in Catholic countries, where dispensations are granted for marriages betwixt persons nearly related; or if we refer to passages in ancient history—to the age of the Ptolemies, for instance—we find they no less generally afford grounds for animadversion."

On the subject of Hereditary Diseases, what our author says is very rational:—

"It is a difficult thing to decide what hereditary diseases should be a bar to matrimony. Madness, present or threatening, with some others, are palpable obstacles—entailing present misfortune on the married pair, and the probability of future evil on society. It is not less true, however, that pregnancy has been found to be (in some cases) a cure for madness.

"Consumption, as a disease which may be arrested during pregnancy, only to put a term to existence soon, and inevitably, afterwards, should be an insurmountable obstacle in every well-regulated mind.

"Epilepsy, as a transmissible disease, has been considered by many medico-legists, as a legal obstacle to marriage; yet some of the greatest men have been epileptic.

"It is a misfortune, that many of these hereditary maladies produce in the afflicted individuals a strong inclination to the married state; and some of them are thought to impart to women a far greater facility of conception. The sensual inclinations of idiots are well known, and the consumptive are observed to have, in general, a violent propensity towards that passion by which the species is perpetuated,—a propensity which, when yielded to, not only hastens their own death, but leaves the deadly germ behind.

"By suitable choices in marriage, the proneness of a race to disease may be diminished: the youth and strength of the woman will certainly wonderfully modify the offspring. One of the finest-formed women we ever saw, was the daughter of a nobleman of sixty-five years of age, formerly noted for his dissipation, and a martyr to its consequences; she was a young lady unusually developed in strength, fifteen years old."

The subject of the probable causes of the occasional preponderance of males or females, is well introduced:—

"Thoughtless persons are apt to attribute to what they vaguely call chance, or the caprice of fortune, what is really the most admirable manifestation of a paternal Providence. The success and riches often obtained by men of inferior abilities and means, furnish an example of the justice of an all-wise Creator; in the same light may we behold the unions which occur between the gifted and the inferior among the human species, the ugly and the handsome. Some unexplained sympathy, some irresistible attraction, unites them; and by these means is the human race improved. Were there not some powerful counterpoise, the arbitrary fancies of human caprice would soon erect a barrier, and the ugly and unattractive be as effectually excluded, as though nature had set upon them a sign of reprobation.

"The Christian religion, by consecrating monogamy as the law of the faithful, not only promoted the surest means of civilization, but also of the increase and physical improvement of the human race. Marriage, thus hallowed by the most enlightened of sys-

tems, produces, in its turn, all the elements necessary to the civil governments of states, and to their physical strength, — an effect which no other union of the sexes can achieve. But to obtain these results, there must be a freedom of choice, and certain physical conditions, observed in the conjugal compact, most of which fall within the scope of our particular views.

"Recent inquiries appear to lead to the conclusion, that, according as the strength predominates in the father or mother, the child will be male or female, and that infirm fathers generally give life to female offspring*. It would appear also, that in the great number of those hot countries where polygamy obtains, the number of female children greatly predominates; the fecundating power of man is too much divided—his wives are sterile, or have a larger proportion of female issue. The proportion, so general and useful, in Christian countries, of twenty-two men to twenty-one women, does not exist there. While all efforts towards civilization are abortive, and the influence of woman on society is lost, it is not strange to see such communities remaining, as they do, for ever barbarous and stationary.

"Should our remarks on the birth of females from debilitated fathers, appear to want confirmation, we may appeal to the analogy of comparative physiology. In the animal kingdom, do we not see more cows, ewes, hens, &c. than corresponding males of the respective species? and the reason is to be found in those habits that correspond to polygamy among mankind.

"A reason has been before stated for the extinction of great families,—the facts first mentioned may explain, perhaps, why they so often terminate in one female scion."

It is very difficult to make a selection where there are so many striking topics to attract the attention. We observe many remarkable passages in the chapters on Emanations and Effluvia, which we had marked as well worthy of being extracted, if our narrow limits would allow. It is almost *par hazard* that we pitch upon the following among them:—

"In the large towns, a great improvement has been introduced in the streets —that which is called *macadamization*; but if care be not taken to remove, during wet weather, the loose mud of the surface before dry heat and the friction of carriages turn it to powder—if, during dry weather, the surface be not regularly watered, and that sufficiently to keep down the dust, during the whole of the day—if these precautions, we repeat, be not taken, the fearful annual average of deaths from diseases of the lungs, in the bills of mortality, will be inevitably increased by the irritation which the powdered granite borne in the atmosphere, must necessarily engender in the respiratory organs.

"Although not strictly within the limits of our present subject, we shall take this opportunity of observing the danger incurred by delicate persons going out in the evening of a hot day, when large macadamized streets are watered. The cold and dampness of the atmosphere produced by the evaporation, may prove very prejudicial."

Treating of vegetable perfume, our author says:—

"To 'die of a rose, in aromatic pain,' is an idea that loses some of its facetiousness, when we really find some young women (for example, the daughters of Nicholas I. Count of Salin, and of a Polish Bishop, &c.) dying immediately after respiring the perfume of some heaps of those flowers, or of violets.

"The rooms in which flowers are most diligently amassed by our ladies of fashion, are generally the smallest; it is in the elegant penetralia of the boudoir that they shut them up. The heat there is favourable to the rapid elicitation of odour from the dying plant —the atmosphere is scarcely disturbed by a current, and seldom renewed—whilst, in their natural situation, the cooler air moderates the evaporation, and its undulation wafts towards us a diluted fragrance."

"We shall not expatiate on several other emanations, comparatively unimportant, but no less curious, and in some

* This rule, like many others we might mention, admits of innumerable exceptions. Natural laws are only cognizable to the philosophical student, in a field of wide and prolonged observation. They do not enable the finger of malice to point at particular instances, or afford aliment to conjugal recrimination. The Supreme Being, in the most healthy and moral of communities, has reserved to himself the manifestation of his will—and thus, while we see one family producing only sons, and another only daughters, in spite of the irregularities of numbers, the general proportion is still observed.

instances of beneficial operation on the human frame—such as those arising from fresh meat, and other articles of food, to which our butchers and victuallers are supposed to be partly indebted for their portliness and good looks—singular instances, if well-founded, of the control exercised on our bodies by surrounding media."

We cannot but make room for the following:—

"It appears often sufficient to confine men within a small space, without adequate air, exercise, food, &c. to produce a contagious fever, sometimes of the most fatal character known.

"A sick deserter was concealed in a small cavern in the south of France, where the only air admitted was by a small door. Fourteen persons administered charitably to his wants during the twenty-one days that he lived: all these persons fell sick, and eight died. The reader has probably read the accounts of the Black Assize at Oxford, in July 1577. Many of the persons present in the court, when the sentence was passed, were struck by a deadly disease from the pestilential exhalations of the prisoners. At Exeter, in 1586, and at Taunton, in 1730, from similar causes, the same evil occurred. In 1750, the contagious jail fever, brought into the court of the Old Bailey by the prisoners from Newgate, produced cruel ravages amongst the audience; and, amongst other eminent persons, proved fatal to the Lord Mayor and two Judges. It is well known that the greatest mortality occurs in ships when they are carrying out troops without commensurate accommodation.

"We must here offer an important reflection on the above facts. We know that, by slow degrees, the human system accustoms itself to poison—whether those deleterious substances, such as opium, tobacco, &c. which are habitually masticated by the inhabitants of different countries, or those which the medical man administers to combat dangerous diseases. It is not otherwise with respect to morbid poisons. Placed in an atmosphere growing every hour more vitiated, a prisoner will live comparatively scathless. The deadly halo that surrounds him will only be discovered when he comes forth apparently well, and mixes with his fellow-creatures, whose lives are thus sacrificed. In a healthy community, let a person weakened by extreme poverty, debauchery, and filth, be attacked by a fever, it may assume at once the most malignant character, and contagion arise, spreading rapidly to adjacent persons. Generally speaking, the atmosphere of disease only extends a few feet round the patient, and, with a little prudence, and recourse to anti-septics, the sick may be approached with little fear of consequences; and there is no plea, even founded on egotism, to justify neglect of the sufferer. It is far otherwise, however, when many members of a family are united in the same room, as occurs amongst the poor, with no renewal of the air of the room. The atmosphere occasionally becomes so impregnated and altered during contagions and epidemics, that persons free from the reigning fever, and labouring under other diseases widely differing, will still require a modification of the treatment generally adopted in their different disorders.

"The atmosphere once taken possession of by a reigning disease, every shade and degree of that affection will appear in different persons. The slighter degrees not showing so decidedly the characteristic features, by neglect or mistaken treatment a slight and ephemeral indisposition may be converted into an attack of the greatest degree of malignity."

The last chapter of Mr. Belinaye's book is on Civilization as a Source of Health or Disease, and abounds with topics of an interesting nature.

We cannot conclude without bestowing some share of praise on the manner as well as on the matter of the volume. It is written in a style well calculated to set off the importance of the subjects treated; it is always graceful, and often eloquent.

MEDICAL GAZETTE.

Saturday, August 18, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

COLLEGE OF SURGEONS AND THE IRISH EXCLUSIONISTS.

SOME months ago we had occasion to offer a few remarks on the Irish Grand

Jury Bill—more particularly that part of it which contained a cunningly-devised clause, entailing exclusion from certain sources of fame and emolument on all who were not members of the Irish College of Surgeons. That Bill has never passed into a law, having been cushioned and thrown aside after conferring only disgrace upon its framers. The stamp of its base nature was too glaringly fixed upon it, to allow it to be made available for its sordid ends. Even the leading Irish members of Parliament, some of whom would be ready enough to advocate any system of exclusive dealing in favour of Ireland, provided it were not absolutely odious, and calculated to defeat its own purpose, became ashamed to afford it their countenance and support. Petitions from aggrieved individuals were laid before the House; and some of the correspondence connected with the presentation of those petitions has happened to come under our cognizance. We shall quote a few passages. It will be observed that the writers take up the matter on a wide but equitable principle.

The member for Limerick (Mr. Spring Rice) proceeds to say, "I am aware that the professional education of Ireland is of a most liberal kind, and that our national schools of surgery and medicine have produced most distinguished and able men; but on that very account am I convinced that no restrictive measures are necessary; and I am certain that the best interests of the community will be best advanced by having all medical appointments open to the freest competition, and thus affording a full scope to individual exertion, and the best reward to high character and attainment." The member for Tipperary (Mr. T. Wyse) says, "I have long thought a complete reform was necessary in this, as in most other departments of the state. I think none would more benefit the cause of humanity, and the interests of all classes, than a new and comprehensive Infirmary Bill, framed in the liberal spirit, and on the enlightened principles, of the present age." The members for Mallow, and Cork, and Kilkenny, express themselves to the same effect; and even Mr. Stanley, the Secretary for Ireland, confesses "that the existing law appears to require much amendment."

The result of these and similar demonstrations of the objectionable nature of the Bill was, as we have said, to cause it to be thrown aside; but the spirit that originated the measure did not sleep: the exclusion which was sought to be confirmed by the Grand Jury Bill continued, and still continues, to be practised in all its disgraceful features of illiberality. A strong re-action has been the natural consequence, and has given birth to the following petition, which so well sets forth the chief points complained of, that we have no hesitation in giving it at length:—

"*To the Right Honourable and Honourable, &c.—The Petition of the undersigned Irishmen, Members of the Royal College of Surgeons in London, Graduates in Medicine, &c.*

"Humbly sheweth,

"That your Petitioners are Irishmen, who have enrolled themselves Members of the Royal College of Surgeons in London; a body whose members are esteemed all over Europe as the first surgeons in the world; whose members possess the confidence of Great Britain to such an extent that they are the surgical attendants upon the dearest objects of Britain's esteem—her sovereign, her sailors, and her soldiers: yet are the members of this body who reside in Ireland oppressed by an unjust law of the late Irish parliament, which incapacitates them from having the care of Irish County Infirmaries: and your Petitioners complain of this unjust exclusion, not on account of the peculiar value of those institutions, but because the fact of their exclusion tends to depreciate their professional character—an exclusion,

the absurdity of which is manifest from the fact that, though excluded in Ireland, they need but cross the Irish channel to become competent to the highest professional distinction in England;—a state of things which appears perfectly anomalous to all those who believe the interests of the two kingdoms to be identical, and is in direct opposition to the principle on which the legislative union of the two countries depends. Your Petitioners, therefore, beseech your Honourable House to remove this disability.

"Petitioners remind your Honourable House, that when this act was passed by the Irish parliament, the present Royal College of Surgeons in London was not in existence; and that therefore the exclusion of its members could not have been contemplated by the Irish parliament, whose act was passed for the sole purpose of securing the Infirmaries against men who had no qualification whatsoever, as is apparent from the very wording of the act, which seems to imply that the qualification of the Dublin College would be sufficient, and that 'no other qualification should be necessary;' which evidently means that the Irish parliament was well aware that other qualifications were as good, but, as Ireland was then a distinct kingdom, they relied for surgeons upon the Irish College; not contemplating that at any future period it should occur that members of the English College would reside in Ireland, whereas, since that period, the Irish College has so increased the expenses of its system of education, for purposes to which Petitioners shall not now allude, that several hundreds of Irishmen have been induced to become English surgeons, to avoid the payment of the enormous fees required in Dublin.

"They pray that your Honourable House will devise some means of removing those distinctions which oppress one class of surgeons and make monopolists of the other. Your Petitioners seek it as an act of justice which should and will be granted by a British parliament, because the union of the two countries requires it—a union whereby it was pledged that we should be as one nation; whereas a distinction is thus allowed to exist that does not exist even between Great Britain and the United States of America.

"And your Petitioners will ever pray."

The signatures are too numerous for insertion; but we may observe, that they include a large proportion of the *elite* of the medical and surgical profession in the sister country.

Now here, we must take leave to say, that this is no mere Irish question—no question that merely involves the interests of resident practitioners in Ireland; it is one that immediately affects the character and extent of privilege of the London College, and the protection which that establishment is able to afford to its members. Four or five hundred surgeons, provided with the diploma of the institution in Lincoln's-Inn-Fields, are practising their profession in Ireland; but they are excluded from the best rewards that should await their successful exertions in their practice, by the antiquated regulation of a rival body. The County Infirmaries are reserved for the scions of the Irish College.

But it is but natural to inquire why this should be; upon what reason the exclusionists rest their supposed privilege? Upon their superior merit? Although we have heard of some such silly assumption occasionally set forth, we can hardly fancy them so absurd as to set up such a claim seriously: none but their greatest enemies would expose them to such ridicule;—but we have said something on this point on a former occasion, and think it needless to revert to it again. Have they in these Infirmaries an eleemosynary endowment for pensioning off their quondam apprentices? Impossible they should assert such a claim with any show of common sense. They will not, surely, have the meanness to rest their pretensions on so humiliating a score, though now pretty familiar with the charge of

making those Infirmaries a *quid pro quo* for the enormous apprenticeship-fees taken by their leading members.

The only plea, then, that remains is, the impudent one of insisting upon *their rights* — rights that originated in no dishonourable or exclusive feeling, so far as we are aware, but which are maintained with all the pertinacity and narrow-mindedness belonging to both.

Another consideration is this. The Irish College has, or it has not, produced wonderfully clever surgeons. If it has, why should they dread a competition with their brethren of the College here,—especially as that competition is to be maintained on their own ground, and, we believe we may add, with their own countrymen only? If it has not, it is rather "too bad" that such good things should be set apart for people that are no better than their neighbours, and who have no specially good right to show to establish their claim to them in these much-famed days of liberal opinions.

If, however, the fact be, as we believe it is, that the Irish College turns out surgeons who are no such wonderful phenomena, and yet these special favourites and exclusives are put into places of high trust and great emolument—to the exclusion of others, at least equally competent—how unjust is the whole proceeding! and how injurious must be the consequences to persons situated as the Petitioners, for example, are! They have no fair play: they are labouring under unreasonable disability; and they are degraded in the eyes of the public. The public is imposed upon; and a monopoly of the most unjustifiable description — to the great disgrace of this age of boasted enlightenment — is maintained.

We have been pointing out, latterly, various circumstances which we conceived to be legitimate objects for medical reform — here, then, is one that must be "reformed altogether," as being founded in ignorance — nurtured by a spurious nationality — and supported in a spirit of gross injustice and illiberality. It is utterly impossible that any Parliament of honest legislators can suffer such a system to be perpetuated—can allow so absurd a system to pass uncorrected and unredressed; but, at the same time we must add, that we think it will require on the part of the Petitioners, and all who feel interested in the triumph of justice, to persist in stating fairly their grievances, and to "pull all together," until this fabric of empty pretension and impudent exclusion be levelled with the dust. All their efforts, we suspect, will be required; for the obnoxious party, grown strong by their perseverance in their system, and bronzed by their habit of defending it, are not to be dissuaded or diverted from insisting upon *their rights*; — as to making concessions, with an honest grace, that is utterly out of the question; for, from some circumstances which have lately come to our ears, we understand that they are even now pushing their exclusive proceedings equally far in another most unblushing respect. Of that, however, more anon, when we shall have all the facts before us for an honest *exposé*.

BILLS OF MORTALITY.

We are glad to perceive by these returns, that there has been a considerable decrease in the number of deaths (or burials, more correctly), in London during the last week. We have thought it advisable to give an abstract of the last "Bills," in another page; and we purpose sparing them a similar corner every week; for we are rather inclined to think that these venerable documents do not by any means merit that neglect with which they have been but too long treated. At all events, the attention of the public may lead to their improvement.

M. ORFILA.

M. Orfila is still in a very precarious state, after his severe attack of cholera. The treatment adopted towards him was of the most energetic description; from the very effects of which it would require some time and powerful stamina to recover. His *morale*, however, as his physicians report, is perfect; and we learn, by the latest accounts, that there are several favourable symptoms now about him.

REJOINDER OF DR. GRAVES.

To the Editor of the London Medical Gazette.

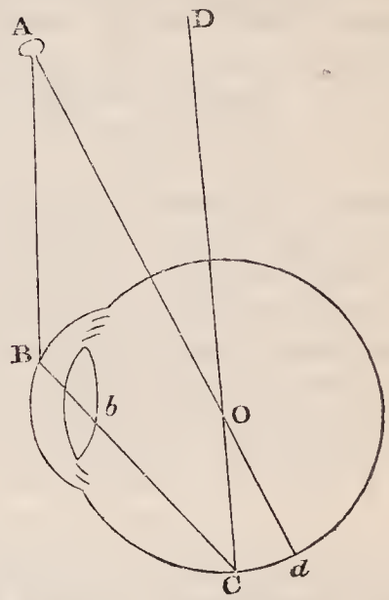
9, Harcourt-Street, Dublin,
August 9, 1832.

Sir,

I regret that your observations, published in the Medical Gazette, August 11th, require further notice on my part, as the point at issue between us has already occupied but too much space in your valuable journal. You say, "We had our doubts before—when penning our former remarks—where exactly the weak point lay in the Doctor's argument. Of the insufficiency of the whole, we had no doubt; but now we think we can expedite the matter, and shew, without difficulty, in what the mistake into which Dr. Graves has fallen consists."

"He assumes that the line of visible direction—or the perpendicular on the retina, at the place of the image—is not coincident with a line drawn from the object through the centre of the eye. Now we, with great respect for the opinions of Dr. Graves, affirm that it is."

This assertion of yours, Mr. Editor, is synonymous with asserting that all objects seen by the eye are seen in their true direction; for when the line of visible direction is coincident with the line drawn from the object through the centre of the eye, the object is of course seen in its true place. In a letter to me, upon the subject of the relative position of objects and their images on the retina, Sir David Brewster speaks of this very point *in the following words:*—"It is not true that all objects seen by the eye are seen in their true directions. The relation between the true and the visible directions is only that of parallelism. The true and the visible direction coincide only for rays that suffer no refraction—that is, for the single ray which passes along the axis.



Let A B be the direction of an oblique ray refracted into B C by the cornea and crystalline; then C O D will be its visible direction, *differing greatly* from A O *d*.

"Now it is a curious physical law, that as the incident ray A B passes from its extreme position of obliquity, to a position coincident with the axis of vision, the angle of refraction, or that corresponding with the last portion, *b* C, of the refracted ray, must be always the complement of the angle *b* C O, or that which the refracted ray forms with the direction in which it excites vision. Without this relation, the visible and true directions could not be parallel."

The above, Mr. Editor, are Sir David Brewster's words. The figure is also an exact copy of his. In a paper I have prepared for the November number of the Dublin Medical Journal, I have treated this subject more at large, and shewn that, for objects at *an infinite distance, viewed obliquely,* other considerations are necessary, in enabling us to arrive at a *general law,* capable of expressing the relation between their real and apparent directions. My experiments, however, only relating to

near objects, this law does not bear on the present question.

I have the honour to be, sir,
Yours very truly,
ROBERT J. GRAVES.

[In a more recent letter, Dr. Graves suggests that Sir David Brewster intended to have said, in the second sentence of the extract given above, "that the relation between the visible direction and the direction of the axis of a pencil of rays incident on the cornea, is that of parallelism," and that the remainder of the extract warrants such a suggestion. We have the greatest respect for the opinions of both gentlemen, opposed though they seem to be to our own: but touching the precise point at issue between us, perhaps it will be better to reserve what we have to say until Dr. Graves publishes the paper which he says is forthcoming—on the visible direction of objects seen obliquely.—E. G.]

LONDON HOSPITAL.

IN our last number but one, we should have said Dr. A. Frampton was elected *Assistant* Physician, not Physician.

VETERINARIAN STATISTICS.

To the Editor of the London Medical Gazette.

SIR,

WILL you permit me to ask your readers if they can afford me any information on all or either of the following points; *viz.* the average quantity of grass or hay consumed, in a given time, by any given number of sheep, or oxen, or horses; and the average age at which the two former are slaughtered for human food, by which the quantity they have previously consumed may be calculated.

I have the honour to remain,
Yours obediently,
G. T. B.

August 13, 1832.

MORBID ANATOMY.

(*From a Correspondent.*)

WE are happy to find that Dr. Carswell is at length about to publish Elements of Morbid Anatomy, illustrated by a selection from his almost numberless drawings. Such a work is still wanted in English medical literature, and we have reason to think Dr. Carswell's mode of treating the subject will be at once original, practically useful, and not too expensive.

WEEKLY ACCOUNT OF BURIALS,

From the "Bills of Mortality," Aug. 14, 1832.

Age and Debility	38	Inflammation of the Brain	2
Apoplexy	5	Lungs and Pleura	3
Asthma	8	Liver, Diseases of the	7
Cancer	2	Measles	12
Childbirth	7	Miscarriage	2
Cholera	103	Mortification	4
Consumption	65	Paralysis	4
Convulsions	41	Rheumatism	2
Dentition or Teething	5	Scrofula	1
Dropsy	16	Small-Pox	22
Dropsy on the Brain	14	Sore Throat and Quinsey	2
Erysipelas	1	Spasms	4
Fever	10	Stricture	1
Fever, Scarlet	4	Thrush	3
Fever, Typhus	8	Tumor	1
Fistula	1	Worms	1
Hæmorrhage	1	Unknown Causes	3
Hooping-Cough	7		
Inflammation	29	Stillborn	12
Inflammation of the Bowels & Stomach	24		

Decrease of Burials, as compared with the preceding Week 179

METEOROLOGICAL JOURNAL,

August 1832.	THERMOMETER.		BAROMETER.	
Thursday . 9	from 49 to	79	30·03	Stat.
Friday. . . 10	53	81	30·05 to	30·13
Saturday . 11	54	77	30·15	30·19
Sunday . . 12	50	72	30·17	30·02
Monday. . 13	45	76	29·95	29·89
Tuesday . 14	47	76	29.84	29·85
Wednesday 15	53	73	29·86	29·84

Wind, S.E. and S.W. the latter prevailing.

Except the 13th, generally clear; a few drops of rain in the morning of the 13th.

CHARLES HENRY ADAMS.

BOOKS RECEIVED FOR REVIEW.

The Effects of Arts, Trades, and Professions, and of Civic States and Habits of Living, on Health and Longevity. By C. Turner Thackrah, Esq. Second Edition.

Clinical Reports of the Surgical Practice of the Glasgow Royal Infirmary. By John Macfarlane, M.D. &c.

The Anatomy and Physiology of the Organ of Hearing, &c. &c. By David Tod, Member of the Royal College of Surgeons.

An Essay on the Epidemic Cholera; being an Inquiry into its New or Contagious Character; including Remarks on the Treatment, &c. By John Webster, M.D.

A Plain and Brief Sketch of Cholera, with a Simple and Economical Mode for its Treatment, submitted with confidence, from repeated Success in its Application. By Wm. H. Williams, M. D. President of the Ipswich Board of Health. Second Edition.

Observations on Spasmodic Cholera, its Origin, Nature, and Treatment; with Remarks on Epidemic Diseases generally. By Henry M'Cormac, M.D. &c. Belfast.

A Short Treatise on Cholera Morbus, or Indian Spasmodic Cholera; with Suggestions for an improved Mode of Treatment, and for obviating Contagion.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 25, 1832.

DR. STEVENS ON THE CHOLERA IN COLD-BATH-FIELDS PRISON,

AND ON THE

SALINE TREATMENT IN THAT DISEASE.

To the Editor of the London Medical Gazette.

SIR,

I FEAR I have done an injustice to the public, as well as to myself, in allowing Sir David Barry so long an interval for deliberate reflection on his hasty and reprehensible conduct; but certainly I did expect that before this he would have made such an apology as reason and common courtesy might have led him to offer to individuals whom he has so cruelly misrepresented, though their only object was a wish to do good, and their sole motive an anxious desire to benefit those of their fellow-creatures who were suffering from a new, but most malignant disease. Silence, however, with certain individuals, is only an invitation to further offence, and such has been the result of my past forbearance. It is true that we have no official dignity to protect us from illiberal attacks, nor situations to promise to those who may be induced to defend us; but Sir David must not believe that he can stifle truth, or shoot with impunity his poisoned shafts at those who are acting with fairness, even though he be a member of a National Board, nor fancy that he can gain his end by arraying against his antagonists, at one time his own misstatements, supported by certificates proceeding from individuals connected with the Board, and at others bringing forward documents partially selected from the desk of office, or unsuccessful cases laboriously sought after, either by himself or his minor agents. Such operations may succeed for a time, but they will scarcely save him, though subtilly devised, and nearly as fatal to truth as opium, calomel, and nitrous acid, are destructive to the innumerable victims of official prescription!

During the prevalence of the cholera in the prison at Cold-Bath-Fields, the medical gentleman of that establishment was not practising in concealment. On the contrary, the doors were laid open to numerous practitioners, who wished to examine the cases which were under his care. Several distinguished and disinterested medical gentlemen did visit the establishment, and so far as I know, they went away satisfied, not only with the fidelity of the reports as to the disease, but with an impression on their minds in favour of the efficacy of the treatment. The only exceptions, of which I have heard, were Sir David Barry and the official witnesses who escorted him to the prison.

It will naturally be asked, what influence could so far blind Sir David's judgment as to lead him to a conclusion so widely different from other individuals? This, however, is a question which his own conscience can best answer, and probably he will not confess that a feeling of professional jealousy had some share in inducing him to oppose us, for he was well aware that he would suffer in the estimation of the public, if the voluntary exertions of others were found to be more successful than either his own, or those of his medical associates, in a body which is most improperly termed the Board of *Health*.

But, independent of this, there is a

peculiar circumstance which will place Sir David in a painful situation, should the saline practice be found beneficial in cholera. Early in 1831 he was sent to Russia by the British Government to procure all the information that he could relative to this formidable disease, which was then rapidly approaching even to the isolated shores of this country; and, certainly, every thing that related to its treatment was at that moment, as it is now, an object of intense importance. In July of that year, Sir David sent home an official report to the then Board of Health, in which he stated the important fact, that two German physicians had tried the saline treatment in thirty cases of cholera, and that every one of their patients had been saved. Now, sir, let me ask, what would any man of common medical sagacity have done under such circumstances,—but particularly one who had been sent there under a public responsibility, and at the public expense? Would he not have taken every pains, and exerted his official influence with the Russian government and physicians, to confirm the truth as to the efficacy of this new practice, by subjecting it to the test of further experience? And this inquiry was the more to be expected from Sir David Barry, who was well aware at the time, that the saline treatment had been used with decided success in the malignant diseases of the Western world.

Whether Sir David was present at the College of Physicians when my paper was read before them, in 1830, I do not know; but he called on me spontaneously soon thereafter, and bestowed eulogiums of the most flattering kind on what he was pleased to call "my important discoveries:"—now, if there was any sincerity in this, why did he not pursue the train of these "important discoveries," when he had so fair an opportunity of doing so, not only in Russia, but since his official appointment as a member of the Board?—and from what has been proved, not only in London, but in other places, and from what I have seen of the effects of the saline treatment in cholera, my belief is, that if Dr. Barry had done his duty to his country and mankind, many of those who have lately died of the effects of this pestilence, might have been saved. It appears, however, but too clearly, that he lost sight of the facts which had been ascertained relative to the Blood, as well as the important communication made in his presence by two respectable physicians; and I leave it to the zealous members of the profession to say if such oversight, whether arising from negligence or dread of contagion, was not highly culpable in one who was charged with a mission of such vital importance!

On his return to this country, Sir David became a member of the Central Board, and after his appointment a sufficient number of cases were laid before them in favour of the saline practice to have induced an impartial body of men, in their situation, to have appointed a commission to inquire minutely and faithfully into the subject. This, however, was not done; and when, at last, they were under the necessity of recommending a modification of the saline treatment, they prescribed the muriate of soda; but giving along with it, other improper and counteracting prescriptions. Hence the good which would undoubtedly have been done by the one, was completely undone by the other; consequently, the practice recommended by the Board was not only unscientific, but, as might have been expected, most unsuccessful.

Still, however, the saline treatment was gaining ground with impartial practitioners, and numerous individuals were almost daily laying claim to the credit of having first introduced it. Sir David, on the other hand, seemed particularly anxious to disparage its value; and in place of making any fair or philosophical trial of its merits, he took great pains to insinuate that it was not the energetic saline matter which was doing good, but the sinapisms and the other external means generally used at the same time. But we know well that such adjuncts, when combined with his own treatment, are totally inefficacious; and when he found that such medical sophistry was of no avail against the force of facts, except with his own dependents, he then resolved to crush the practice by a bold attempt; and counting on the fidelity of his own witnesses, together with the aid of an influential journal, he proceeded to work in a way which shewed, that if, during his residence in the East, he had bestowed less time on the study of cholera, he had at least devoted some attention in learning the character and imitating the manners of a Russian autocrat.

The second irruption of the cholera into the prison at Cold-Bath-Fields com-

menced on the 3d of June; but I did not attend the cases regularly until the evening of the 21st. The disease by this time had spread to a most alarming extent, and out of about twenty patients who were then severely attacked, five of them were actually dying.

It had been ascertained in the prison, that washing the blankets, &c. was a most dangerous occupation; for out of fourteen individuals who were employed in this service, ten were attacked with cholera; almost all of them had the disease in its most malignant form, and four of them died. Being fully aware of the danger of this manner of purifying the clothes, both Mr. Wakefield and myself represented to the magistrates the necessity of immediately getting rid of the infected clothing. Regardless of economy, where human life was at stake, they immediately issued an order to bury all the suspicious bedding, clothes, &c. as speedily as possible. The process of separation was commenced on the 22d, and nearly finished on the 25th. By this and other means the infected wards were so completely cleared out that there was scarcely a pin-head left for infection to hang upon. They were also well fumigated; and from that period there was not only a decided decrease in the number affected, but the subsequent cases were, comparatively speaking, more mild than those which had occurred previously to that period.

On the 27th of June, and under such ameliorating circumstances, Sir David Barry first honoured the prison with a visit; on which occasion his demeanour was evidently that of a man unwilling to see the truth, or to investigate fairly. Knowing the powerful professional motives which pressed upon him, his conduct did not surprise me in the least; but assuredly all the other impartial individuals present, medical and non-medical, even to the matron of the house, expressed a feeling of disgust at his conduct. In fact, aware of the awkward questions that might be asked relative to his conduct in Russia, he had no alternative when he came to the prison but to admit that he had neglected his duty, or to deny the utility of the saline practice. He preferred the latter; and in doing this, he allowed his chagrin to escape him to such a degree as exposed him to the derision of those who had seen the cases from the time that they were first admitted up to the moment of his visit.

I never expected that any new practice could be introduced without opposition; but I must say that I was not prepared for the glaringly erroneous and barefaced statements which have been made by some of our opponents. On losing a few very bad cases, a report was most industriously circulated that the saline practice had completely failed, even in my own hands; and that the mortality was greater in the prison at Cold-Bath-Fields, under this treatment, than in any other place; while some of our antagonists, with the true *odium medicum*, were base enough to cause paragraphs to be inserted in the newspapers, asserting that we had lost seventeen cases in one day. When Sir David Barry was officially informed by the Governor that these reports of failure were unfounded, he then took up an opposite position, and, after he had visited the cases, in place of giving the saline practice credit for its efficacy, he declared that he had not seen a case of cholera in the whole establishment; and this, with the vulgar observations which he made afterwards, was surely no very flattering encouragement to those who had been harrassed almost to death in conducting the patients to the favourable situation which they were then in.

It is only through the medium of others that I know what occurred at Sir David's second visit, on the 28th; for on that day he took care to come to the prison at an hour when he knew, from previous information, that he would be least likely to find me there. Fortunately, however, two of the magistrates were present; and these gentlemen have openly expressed their opinion with respect to the unfairness of his conduct, as well as the superficial manner in which both himself and his coadjutors examined the cases. Sir David, however, and his agents, try to disparage the testimony of these gentlemen; and the manner in which they do this, does little credit either to themselves or their cause.

In the first place, it is insinuated that these two individuals are unworthy of credit, because they belong to a body which his partizans are pleased to designate as "venerable twaddles." Now the fact is, that the two gentlemen who accompanied Sir David round the wards are men pre-eminent for their talents, and, so far as age is concerned, younger by many years than Sir David himself.

But does it not indicate a miserable want of matter, when the champion of Sir David has recourse to such unwarrantable sarcasms against talented individuals, merely because they have given an impartial evidence against his patron?

In the introductory observations to Sir David's letter, published in the Medical Gazette for July 21, he observes, that *though the magistrates were present at his first visit*, yet they had not the candour to state, at their meeting on the 12th of July, that his visit on the 28th of June was the second that he had made within twenty-six hours; and from this he would lead us to infer, that his hasty examination on the 28th was quite justifiable, in consequence of the minute examination which he had made on the preceding day. Now, sir, with respect to the fact on which all this reasoning is founded, I must say that it is either a gross error, or something worse than a mere misstatement; for Sir David could scarcely be so deficient in discernment as to mistake two of the turnkeys for two of the visiting justices; and the fact is, that *not one of the magistrates was present at his first visit;* and if they had, they would have seen that his examination on the 27th was just as superficial as that which they witnessed on the following day: consequently, his assertion about the want of candour on the part of the magistrates is not correct; and his superficial examination on the 27th is certainly no excuse for the haste in which he ran through the wards on the 28th, and still less for that of the two *employés* of the Board, who had not seen the cases the day before.

When he visited the prison on the 27th, so far from making any thing like minute inquiry, he was evidently in a state of great perturbation, and appeared anxious to leave the infected wards as speedily as possible*. In fact, there never was a more perfect farce than the whole of this pretended examination of the cases. He scarcely put a single query either to the nurses or the medical attendants. He had but one list of questions, and these in general were only put to those patients who appeared to be least indisposed. We will subjoin, however, his catechism of cholera, for the benefit of the profession:—1st, Are you sick at your stomach? 2d, Are you purging? 3d, Have you cramps in your legs? And when these erudite queries had been answered, generally in the negative, he then moved off to another ward, where the same able interrogation was again repeated. It was in vain that I pointed out, both to himself and Dr. O'Shaughnessy, who came with him, case after case where the patients had recently recovered from the worst state of collapse. To all my statements, however, which could have been confirmed by the patients or the bystanders, both professional and others, Sir David was perfectly deaf. He would not condescend to inquire what symptoms the patients had had when they were first admitted; disdaining, in fact, to listen to every information that could have thrown any light on the nature of the cases which he was then pretending to examine, either for his own satisfaction or the good of the public.

After his first *coup d'œil*, on the 27th, he lost no time in declaring that he had not seen one patient with cholera in any of the wards. This decision, however, was, to say the least of it, a very rash one, and what must the prisoners have inferred from so many sudden deaths, if there was no disease? but besides this, in an establishment where there are upwards of one thousand individuals all circumscribed within a small space, there is a wide difference betwixt having and not having cholera. Sir David Barry declared that we had no cholera, or at least that he had not seen a case; but even his advocate, the Editor of the Lancet, admits, on the authority of Sir David's friend, who came with him, that the said friend had seen two or three recognizable cases of cholera even at Sir D. B.'s first visit; and in fact, in more cases than one, Dr. O'S. called him back, and pointed them out to Sir David Barry at the time. But none are so blind as those who do not wish to see; and as none of the patients were at that moment actually dying, Sir David persisted in declaring, both to myself and others, that we had not one case of cholera in the whole prison.

At his next visit, on the following day, he had two medical attendants in place of one; and these, however devoted to his interests, could not agree to shut their eyes altogether, and Sir

* For proofs that this is not the first time that Sir David Barry has acted in an equally improper manner, see Fraser's reply to papers of Dr. D. Barry on the Gibraltar Epidemic of 1828.

David was compelled on this occasion to be more open to conviction, for three of these were so ill that even he himself was forced to confess that these three were cases of genuine cholera.

The Governor had stated, in answer to a letter from the Privy Council, that there were at that time in the prison about seventy patients with cholera, *who were labouring under the various forms of the disease.* Sir David, however, flatly contradicted this statement, and reported to the Board that there were only *three* cases at that time in the whole prison. Now, sir, what can be the cause of this wide difference in the two statements? they cannot both be correct; and may we not suppose that it can only be accounted for either from hasty observation or a wish to deceive. Neither of these charges can well be preferred against Mr. Chesterton, who was much better informed as to the details of the cases than Sir David Barry either was or could possibly be. Nor could he have any assignable motive for multiplying the cases, however strong a one Sir David might have for diminishing the number. And I do say that when he certified that there were only three cases in the whole prison, he gave a decision which his slovenly investigation by no means warranted him in giving, and which was not only at variance with what he had witnessed, but with all the impartial evidence that has been given on the subject.

In visiting the prison, Sir David saw, probably for the first time in his life, a number of patients who had been fairly subjected to successful treatment. The convalescents were making rapid advances to health, and therefore he denied that they had ever been indisposed—merely because a number of the patients were not then actually dying, which I may venture to say would have been the case had they been treated with the unsuccessful and unscientific drugs recommended by the Board. And if one of the patients, as he asserts, had tooth-ache at the time of his visit, or if twenty of the convalescents were allowed to walk in a purer air during the middle of a warm day, was this any proof that they had not been recently affected with cholera? Some of them had decided symptoms of cholera, even when he visited the prison, and as to the others, those who saw the patients when first admitted can and will testify to the appalling scenes which they had witnessed only a few days before; and I do affirm that, according to the common custom in cholera reports, there was not one case then in the prison which would not have been considered as cholera in some of its stages by any man who had not some object to serve in disguising the truth. Sir David would have said what was true had he reported that he had seen a few mild cases, where the progress of the disease had been suddenly checked by successful treatment, and a number of others where the patients had recently been in a state of collapse, and now convalescent; but surely he must know that collapse is not necessary to constitute cholera; neither is it necessary that they should have vomiting, purging, and cramps, from the moment they are admitted up to the time that they are dismissed cured; and as it can be proved by most unexceptionable evidence that every one of the patients which he saw had decided symptoms of cholera, when first admitted, and as the majority of them would have been included under the head of "Cases remaining" in any cholera report, he consequently stated that which was not correct when he asserted at his first visit that there was no cholera in the prison, and, at his second visit, on the following day, that there were only *three* patients with that disease in the whole establishment.

Suppose a patient, for example, admitted into an hospital early in the morning with all the symptoms of inflammation of the chest, and let him be immediately put under the most active treatment. It is probable that in a very few hours the whole of the symptoms may be completely relieved; but what should we say to any visitor who would go into the ward at this period of convalescence, and without inquiring minutely into the previous history of the case, would pronounce, off-hand, that this neither was nor had been a case of pneumonia? A practitioner who wished to injure another might easily under such circumstances shrug up his shoulders, exclaim, "Umph! and this is what they call pneumonia in this place!" Those who do not belong to the profession may easily be deceived by this sort of reasoning, but every candid medical observer would denounce such criticism as not only unprofessional, but altogether unworthy of an honourable mind.

But to come still closer to the point:

we all know that those who are under the influence of the poison which causes cholera are most frequently attacked during the night; and let me ask, if a patient be brought into any hospital early in the morning, with sickness in the stomach, a bowel complaint, with rice-water dejections and cramps, is there a single practitioner in any infected district who would not consider this as a case of cholera in the first stage of the disease? If we give opium in such a case, we relieve for a time every one of these symptoms; but unfortunately under this treatment the collapse comes on with fearful rapidity; probably because the practitioner has checked that discharge which appears to be a salutary effort of the preservative power to throw a part at least of the noxious agent out of the system. But, on the other hand, when we use proper remedies, such as a Seidlitz powder, or effervescing draughts, at the beginning, and then throw an excess of the non-purgative salts into the circulation, the poison is thereby either removed or neutralized, and the progress of the disease is speedily arrested. And if the patient be completely relieved in eight hours from the commencement of this treatment, has any one who sees the case now for the first time, a right to assert that this is not cholera, merely because the patient is neither sick at the stomach, purging, nor cramped, at the identical moment that he happens to visit him? but this is the logic that was used to prove that there were only three cases of cholera in the prison at Cold-Bath-Fields, on the 28th of June.

At one period (that is, previous to the interment of the infected bedding) it was no uncommon occurrence for several patients to be taken ill during the night, and brought to the infirmary on the same morning in a state of complete collapse, before we had an opportunity of giving them one particle of medicine. Now we all know, that often in severe cases the disease runs its course with such rapidity, that the fate of the patient is generally decided either in one way or another in a few hours. Those who die are removed to the dead-house; and in other cases, where reaction comes on, they are soon relieved. But does it follow that such persons had never been ill, because they are not in a state of suffering when visited twenty-four hours, or a week, afterwards? Yet those who do not know Sir David Barry, will scarcely believe that this is the shallow ground on which he has ventured to impeach the veracity of others, whose evidence is at least as credible as his own.

I have formerly stated, that several cases had been lost from having dismissed the patients too soon from the cholera infirmary, in consequence of which Mr. Wakefield very properly came to the determination not to dismiss any patients who had been very ill, until they had been convalescent for at least ten days. When Sir David Barry visited the prison on the 27th, the epidemic was then, as I have said, decidedly on the decline; but there were at that moment in the establishment twenty-two patients, who had recently recovered from collapse, and *sixteen of these were then in the wards.* And let me ask any impartial person in the whole profession, is it not the fact, that in every cholera hospital such cases are reported as cholera, from the time that they are first admitted up to the moment they are dismissed as cured? And if so, is it not evident that Sir David had neither custom nor common sense to support him, when he asserted that he had not seen a case that could be considered as cholera at Cold-Bath-Fields?

Most of the facts which have been stated by Sir David Barry are so manifestly incorrect, and the whole train of his reasoning is so very baseless, that it were scarcely worth while to examine this subject any further. There is one observation, however, which is worthy of notice, for it appears at first sight to be an argument in his favour, particularly with those who have not witnessed the effects of the practice which was used in these cases.

It is one of the great disadvantages of the old treatment, that even if the patients do recover from cholera under the use of calomel and opium, the disease is generally followed by a secondary fever, which is nearly as intractable as the cholera itself. On the other hand it has been found, not only at Cold-Bath-Fields, but at the Greville-Street Hospital, at Leith, and numerous other places where the patients have recovered under the saline treatment, *that no secondary fever supervenes;* and this I consider to be one great argument in its favour; for the neutral salts not only correct the poisoned or diseased state of the blood, which appears to be the immediate cause of the cholera, but

they prevent that vitiated condition of the whole current, which is probably the cause of the secondary typhus. Now, though Sir David Barry was perhaps ignorant of this important fact when he visited the prison, yet why, in his race round the wards, did he not point out the absence of typhus? His course, however, was too rapid for deliberate observation; and showed that an inquiry into truth was evidently no part of his object. But it is rather hard that others are to be condemned, who are at least as honest as himself; and that too merely because he was totally ignorant of a most important fact, which is well known to every individual who has given the saline treatment a fair trial.

There is another circumstance with respect to the effect of the saline practice which does not yet appear to be generally known, and that is, the sudden change of colour in the skin, when the patients are recovering from a state of collapse. When the excessive draining from the bowels is checked by the injecting laudanum, port-wine, or other proper remedies, into the rectum, and when the salts are given internally, so as to enter the circulation, and affect, not only the blood, but the whole body, the sudden change in the skin, from blue or almost black, to a colour that is nearly florid, is, in some cases, almost instantaneous; the features recover their usual appearance, and though there is no fever, the face becomes flushed; but those who are not aware of this fact ought to be cautious in giving their opinion on such cases; for, under these circumstances, a man may easily err even when he intends to be strictly honest.

Some of Sir David's agents have said with truth, that the Governor reported a larger number of cholera patients than appear to have existed even by the official report of Mr. Wakefield. Now to reconcile this apparent discrepancy, it is necessary to state, that during my first attendance in April last, I was occasionally present in the morning when patients were brought into the infirmary with the symptoms of the disease so distinctly marked, that they would have been considered as cholera in any part of the metropolis where this disease was then raging. On inquiry next day why these cases had not been reported, I was shown an official document which had been brought to the prison, from the Board, by Dr. Macann, with a request, that those cases only should be reported as cholera which corresponded with *his* definition, and that all others should only be reported as cases with premonitory symptoms. The following is a correct copy of the two first paragraphs of this document:—

"In order to prevent any misconception with respect to the nature of the cases to be included in these Reports, it may be proper to state, that by the word CHOLERA is here to be understood a disease characterized by the following symptoms; viz.

"A purging and vomiting of fluids, neither feculent nor bilious, with cramps and prostration;—to which in extreme cases are added, a coldness, and shrinking, and lividity of the surface, particularly of the extremities; with suspended pulsation at the wrist, and suppression of urine."

I need not say that the above symptoms, when taken in the aggregate, form the description of a patient in the last stage of cholera: and, when I pointed out this to the authorities, they said, that in communicating with the Central Board of Health it was necessary to comply with the instructions which they had received. I observed, that even Dr. Macann, as was evident from another paragraph, only required a majority of the symptoms. Yet even this made no impression, at least with respect to the reporting of the cases; and my belief is, that the authorities were glad to have so good an excuse for not spreading any alarm relative to the prison; consequently, the report of cases at Cold-Bath-Fields, so far from being exaggerated, was much below the actual number.

It may here be observed, that it is necessary for the surgeon, once in five weeks, to make a return to the Visiting Magistrates of every case of sickness in the infirmaries; and in his usual report for June, he returned upwards of 70 cases, where the patients were in the various stages of cholera. It was in consequence of this report that the Visiting Justices conceived it their duty to inform the presiding magistrates, at the various Police-offices, of the alarming extent of the disease at Cold-Bath-Fields. Now at a later period, when Mr. Chesterton was requested to state the exact number of cases in the prison, he included all those that were then on the sick list, and considered as labouring under cholera in its various stages. He

acted, in this instance, as any other individual would have done in the same situation. But when Mr. Wakefield was requested to make out an official return of those who were attacked with cholera on three successive days, he made out the list according to the instructions he had received from the Central Board. Mr. Chesterton's list included all who were ill, as well as those who had not yet been dismissed cured; while Mr. Wakefield reported those only *who had been taken ill on three given successive days*; it is for this reason his list of cholera cases is much less than that of the Governor, who, when he reported the whole, was regulated by his own common sense, and did not choose in that private correspondence to be fettered by a definition which he knew to be improper: and though Mr. Wakefield made out his list in compliance with his instructions, yet he considered the others as cases that would have been denominated cholera by any individual who had not been ordered to report those cases only where the patients were actually on the brink of the grave. The Board may define cholera in any way that they please; but on this subject every independent practitioner will take the liberty of judging for himself. Those, however, who are paid for their services, must appear at least to be doing something; but my belief is, that the definitions and interference of the Board in such cases, are as useless and unnecessary as red-hot irons to the spine in cases of cholera!

With respect to the minor points of Sir David Barry's statement, they are scarcely deserving of notice. He asserts, for example, that I told him that the saline remedies prevented collapse in every instance. But after what I had then seen, I could scarcely have made any such assertion; for my conviction is, that there are some cases so severe that no earthly means can arrest their progress. When we use opium or other improper means, it is of no consequence how early we see the cases, for such treatment does no good; but when the patients are seen early, and properly treated, the collapse may be prevented in twenty-nine cases out of thirty. This is what I have said to others, and this, I believe, is what I said to Sir David. He also states, that our having lost twelve cases was a proof that our treatment could not prevent the collapse. This, however, is of a piece with his other remarks, for the patients who were attacked during the night, were frequently in a state of collapse before we had an opportunity of giving them even one particle of medicine; as happened to be the case with a woman who was taken ill during the night of his second visit, and died early next morning, in a very few hours after the attack: this, I believe, frequently resulted from the dread on the part of the patients themselves to be removed to the cholera wards, and either concealing their sickness, or desiring delay, under the vain hope of a speedy amelioration of their ailments.

If Sir David were as well known abroad as he is in London, the present explanation would be less necessary; but he has done no good, and unfortunately his official situation gives him the power to do much evil. He has patronage at his disposal, and those may support him who are looking forward to the Board for further promotion; but those will probably act differently who have no favour to ask from the Board of Health; and, for my own part, if Sir David Barry had not interfered with me, I should have been the last person in the world to have wished for the honour of any connexion with him; but he forced himself upon us, and the public may now judge from what motives.

After having long tried the remedies in common use in the treatment of malignant diseases without success, and after having witnessed the beneficial effect of the saline treatment in the West India fevers, I came to this country for the purpose of laying an outline of the practice before the profession; and, with the firm conviction which I have of its utility, not to have done so would have been criminal. Since my arrival in England, I have had an opportunity of extending the same principle to another disease*; but I never dreamed that even one member of the profession would make up his mind with respect to its value merely from my statements. I trusted, however, that I had said enough to induce

* It is in the first stage of cholera that the saline remedies are of intense value; but independent of this, is it not the fact that there has scarcely been a recovery from complete collapse under any other treatment? and is it not equally true, that the recoveries, even in the most hopeless cases, have been very numerous, where the saline practice has been properly used?

many impartial practitioners to try the treatment, and to judge for themselves. This was all that I expected; and every day I have additional reason to believe that the mortality will be diminished when the principle of this practice becomes more generally understood, and where it is tried by practitioners who know how to vary the treatment according to the circumstances of the case. But every physician must be unsuccessful who (like the anonymous correspondent, in your number before last) forces a strong saline fluid into an irritable stomach at a time when it cannot be retained. The result, however, would probably have been very different, even in these cases, if he had first quieted the stomach with saline effervescing draughts *, and then administered the stronger salts, when there was some chance of their doing good. But perhaps your correspondent will be pleased to inform your readers whether or not these cases occurred in a certain madhouse, where, in similar cases, there has not been a recovery under any practice; and whether these unsuccessful cases were published by the attendant of his own accord, or were ferreted out and laid before the public, *not by the writer*, but either by Sir David or some of the individuals connected with the Board.

Though the saline treatment is not invariably successful, I am happy to state that it has been tried by many impartial individuals with decided success; and the members of the Central Board will scarcely venture to deny that *they also have received strong reports in its favour*, and if so, what excuse can Sir David make for concealing these, and picking out those cases only which are unsuccessful, and sending them for publication to the various journals, apparently for the purpose of deceiving the public? Or, should it appear that Sir David has had recourse to this unfair means with a view to impede the progress of a useful practice, and that he who is so liberally paid for his services to the community is sacrificing the lives of others to screen himself, he must then answer for his conduct to a higher tribunal than that of a medical journal, where the apparently impartial writer takes upon himself to decide the case; and that, too, at a time when he has seen only one side of the question.

The points at issue betwixt Sir David Barry and myself are not mere matters of personal dispute between two professional individuals. The cholera is now in almost every town and every village of the British empire, and if there be one method of treatment more likely than the others to diminish the mortality, it would be at any time an affair of national importance to inquire into the facts, but particularly at this moment, when the lives of thousands are at stake, and many are actually dying, who were it not for the official and unfair opposition of the Board, might easily be saved. Sir David Barry has been openly accused of making misstatements on this subject; and if he had been conscious that this accusation was not correct, it is natural to suppose that before this he would have sent in a petition to government to appoint a committee to inquire into the propriety or impropriety of his conduct with respect to the cases at Cold Bath-Fields, for on this subject he must not expect that his own assertions will be taken as proofs, or that he can clear himself of a serious charge merely by the evidence or certificates of individuals who are in the employ of the Board—particularly as it is evident that these very individuals knew as little of the cases as he did himself. Such proofs may be satisfactory to Sir David, but they will not be likely to convince others; and as he has not thought proper, for his own exculpation, to apply to the government to appoint an impartial committee to inquire into the facts, I shall now do so, for mine; and when this is done, I will leave it to them to examine the evidence on both sides, and decide on the points which are now at issue between Sir David Barry and myself; but above all, relative to the efficacy of the saline remedies in the treatment of cholera.

Trusting that you will insert this communication as early as possible,

I have the honour to be, sir,

Your obedient servant,

Wm. Stevens, M.D.

Albany-Street, August 20th, 1832.

* When the stomach is irritable, which it generally is in cholera, the saline draughts are of great value; and I feel confident that the mortality from this disease would be greatly lessened even were we to trust almost entirely to this simple remedy. When half a drachm of the carbonate of soda, or potass, is dissolved in a little common or cinnamon water, and when we add to this as much citric acid, or lemon juice, as is necessary *nearly* to neutralize the alkali; this, given frequently, lessens the irritation of the gastric organs, and does more good than any other remedy that I have seen tried in those cases where the stomach is so irritable that it cannot retain the stronger salts.

ON THE

RE-ACTION, OR SECONDARY FEVER OF CHOLERA.

By J. Adair Lawrie, M.D.

Physician to the Glasgow Cholera Hospital; Professor of Surgery, Andersonian University*.

Hospital practice has enabled me to accumulate facts on this subject, of which I was previously ignorant. I am inclined to divide it into several parts, according to the organ affected and the most prominent symptoms. Some there are who recover from severe attacks without exhibiting any unpleasant symptoms. In these the tongue and skin become warm, bile is secreted in moderate quantity, the pulse returns to the wrist, the urine flows freely, and convalescence is rapidly confirmed. These, however, are the fortunate few, and probably do not form a twelfth part of true cholera cases. Our hospital experience would prove that about a fifth of those who die of cholera are carried off by hæmorrhage from the intestines, and secondary fever; and at least a half of those who recover from severe attacks, pass through a train of secondary symptoms before their convalescence is secured.

I would arrange the secondary symptoms as follows:—1st, Bloody stools; 2d, profuse bilious vomiting; 3d, affections of the mucous membrane of the ilium, colon, and rectum; 4th, congestion and inflammation of the lungs; 5th, subacute inflammation of the arachnoid and pia mater; 6th, suspension of the secretion of urine. Of each of these I shall say a few words.

1st, *Hæmorrhage from the bowels*, or, as they are styled in our Hospital Journals, "*bloody stools*." Of this form of termination we have met with upwards of forty examples. They may be divided into those which occur before the pulse returns to the wrists, and those which appear after secondary fever has been established. They almost universally appear in patients beyond the age of forty; and are so common in those upwards of fifty, as to be the cause of death in almost all of that age who escape the acute stage. They appear under three circumstances:—1st, In many cases the secondary stools are early tinged with florid blood, without any precursory symptoms; the quantity gradually increases, becomes dark coloured, putrid, and exceedingly offensive. 2d, Violent irritation and restlessness, with manifest symptoms of congestion of some other organ, precede the discharge, and are materially relieved by its appearance. 3d, Some patients complain of pain across the abdomen, usually its upper regions, at times excruciating, which is followed and relieved by the discharge of blood by stool. In whatever manner the stools are ushered in, the progress and termination of the cases are very uniform. At first the patient does not suffer, or appears relieved; soon, however, his hands, arms, face, and head, become cold and livid; he is uneasy, gradually gets more and more restless, until his sufferings from this cause seem beyond endurance; he tosses from side to side in bed, has a constant desire to leave it, and frequently dies in one of his abortive attempts to do so. The purging usually ceases before death; the pulse is gone. Almost all are delirious; a few furious. Bloody stools are almost a mortal symptom. If the patient be old, and the discharge profuse, his fate is sealed; if young, and the purging moderate, he may survive—but even under the most favourable circumstances, few escape. The cause of this symptom seems to be a discharge of blood from the same capillaries which poured out the primary watery stools. The serous portion of the blood being drained off, the vessels which yielded it become gorged to over-distention with red blood, which soon finds its way through them, and is discharged by stool. This symptom (in my experience) is never seen in children; their mucous membrane and capillaries being powerful, bear purging well: for the opposite reasons it is exceedingly common in old persons. In middle aged, robust people, it either appears as a relief to congestion of some other organ, or is preceded by violent pain, the latter probably caused by the passage of blood through a previously strong membrane.

Bloody stools sometimes appear in patients labouring under congestion of some distant organ, in general the

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* The readers of the Gazette were indebted to Dr. Lawrie for an interesting paper on Saline Injections, published a few weeks ago. The present observations are extracted from the last number of the Glasgow Medical Journal.

lungs. Several of our patients, distressed with violent dyspnœa and extreme restlessness, have been so suddenly relieved by a discharge of blood from the rectum, as to induce us to conclude that in them it was temporarily beneficial. Their termination, however, was fatal, the disease passing through the stages above described.

The discharge of blood is sometimes preceded by violent pain across the upper part of the abdomen. Dissection shows, that the discharge is usually yielded by the lower portion of the ilium, cæcum, upper part of colon, and lower portion of rectum. In a few the jejunum was the intestine affected, all below being sound. In these last, the discharge was preceded by violent abdominal pains.

The appearances on dissection vary from slight redness, with effusion of a little blood mixed with mucus on the inner surface of the intestine, to the deepest injection of the mucous membrane, ecchymosis, injection and thickening of the submucous cellular tissue, elevation of the membrane into irregular, pile-like excrescences; softening, roughening, abrasion, and lastly, partial sphacelus of the membrane itself.

I need hardly add, that I know not how to cure this affection. I have tried leeches, blood-letting, blisters, warm bath, stimulants—in a word, every thing that empiricism could suggest, without avail. The standard prescription in the hospital is an emulsion of turpentine, given in doses of from thirty to sixty drops of the turpentine, every hour or second hour. This mixture, with whisky in such doses as the head will bear, has been more useful than any plan of treatment we have tried.

Bloody stools more rarely appear in the secondary fever, and are then usually preceded by profuse bilious purging. Although always a most unpleasant symptom, they are much less dangerous at this than an earlier period, and are frequently under the control of the turpentine emulsion, the warm bath, and sinapisms to the abdomen. My intelligent assistant, Mr. Sisson, is of opinion, that, in the secondary cases, the blood is yielded by the large intestines. Be this as it may, when we examine the intestines of those who have died a few days after the discharge has ceased, the mucous membrane is found less generally injected, the irregular pile-like excrescences already alluded to, are found to have subsided, and the membrane which covered them, rough, and of a greenish-yellow colour. It is probable that this appearance is a stage of sphacelation, and if the patient had survived, the portion so affected would have separated, and left an ulcer.* These appearances are most commonly met with in the cæcum and rectum.

2d. *Profuse bilious discharges, principally vomiting.* After the secretion of the liver has been restored, bile continues to be discharged from the stomach in immense quantities; the stools are not numerous, and yellow; the pulse returns, but re-action is never fairly established, the skin never becoming hot, and the tongue being cold, moist, and yellow, or green. No urine is secreted, and distressing hiccup is at times the cause of great irritation. If the case be about to terminate fatally, the vomiting continues, and stupor, coma, and sinking, are the precursors of death. If recovery be to take place, the sufferer usually lapses into some of the forms about to be described, bilious vomiting, more or less profuse, being almost universally the first stage of severe secondary affections. The colour of the matter vomited is peculiar, varying from yellow, which it is at first, to the deepest sea-green; at times it is so intense as at first sight to have the appearance of indigo dissolved in water. When the vomiting is the apparent cause of death, the gall-bladder is found full of bile, the stomach, duodenum, jejunum, and upper part of ilium, coated with very tenacious mucus, deeply imbued with bile, and the lower half of the mucous membrane of ilium deeply injected and ecchymosed. The treatment which I have found best adapted for this secondary affection, consists of emetics, followed by stimulants and counter-irritation. Half an ounce of salt, or a scruple of ipecacuanha, repeated as the quantity of bile discharged demands, with a sinapism over the upper part of the abdomen, and whisky internally, when the stomach will retain it, constitutes the principal part of the treatment. The emetic seems to act by mechanically emptying the gall-bladder, and ridding the stomach of a

* Since writing the above I have met with a case in which separation had taken place, leaving ulcers with well defined margins.

constant cause of irritation. I prefer sinapisms to blisters, because with us vesication of the surface has produced great irritation, and been very difficult to heal, probably in part owing to the incessant tossing of the patients.

3d. *Affection of the mucous membrane of the ilium, colon, and rectum.* 4th. *Congestion and inflammation of the lungs.* 5th. *Subacute inflammation of arachnoid and pia mater, ending in typhoid symptoms.* I class these three affections together, because the symptoms which precede their development are very often alike—because they are frequently found combined in the same individual—and because they all terminate in typhoid delirium and depression. Their progress is this:—A patient recovers from a severe collapse, at times rapidly, at other times more slowly, after copious deep-green vomiting. During the third day of his disease, he says he is quite well, is in good spirits, and all symptoms appear favourable. In a very few hours he is observed to sleep longer and more soundly than natural, but when awake he is rational, and his sensations are so pleasurable as evidently to indicate a peculiar cerebral affection or sympathy. A Scotch patient, when asked how he is at this stage, replies, "I am fine." I never met with a recovery after this answer, given in the above circumstances. There is no urine; the bowels are slow, or the discharges bilious; the temperature and pulse good. The impetus of the heart, almost uniformly of the right side, becomes augmented. In some this symptom is only discernible by the stethoscope, in others it is so powerful that the action of the heart and larger vessels is distinctly seen, the pulsation being at times so great as visibly to shake the upper part of the body. The stupor and profound sleep go on increasing. The breathing is deep and snoring, and even if disease be not subsequently developed in the lungs, the respiration is sonorous and bronchial. The subsequent symptoms are modified by the organ chiefly affected.

1st. *If the principal seat of the disease be the intestinal mucous membrane,* the patient soon becomes restless, tosses incessantly from side to side, his belly is painful on pressure, he has hiccup, seems to suffer excessive agony from an unbearable general irritation, his pulse flags, his skin is below the natural temperature, his tongue dry, crusted, and cold, he has no stools, the vomiting continues bilious, no urine is secreted, and he sinks rapidly. Dissection discovers the mucous membrane in one or other of the states already described, but most frequently in that in which the disease is seated in the large intestines, exhibiting the ecchymosed pile-like patches, in a state approaching to sloughing and ulceration. Some cases are more prolonged, the symptoms are more acute, the pulse full and hard, and indicative of inflammatory action. The blood drawn is deeply buffed and cupped. The patient dies, and dissection discovers no appearance besides those stated.

I know of no certain cure for the above affection; but leeches to the lower belly or anus, the warm bath, sinapisms, and small doses of turpentine, are more useful than others which we have tried.

2d. *The Lungs.*—When the lungs are the organs chiefly affected, the symptoms having proceeded to the stage already described as common to this class of secondary affections, the respiration becomes gradually more imperfect. At first the percussion is good, and the stethoscope usually discovers the murmur, bronchial and sonorous anteriorly, and sibilous posteriorly, especially on the right side. There is something peculiar in the respiration at this stage, to which I confess my knowledge of the stethoscope does not enable me to give a name. By degrees the murmur becomes more and more indistinct, till at last nothing is heard but the mucous râle in the larger air tubes. I have rarely discovered the *râle crepitant* well marked, a circumstance for which I find it difficult to account. The patient never complains of pain in his chest, and becomes exceedingly restless, throws himself out of bed, and dies exhibiting typhoid symptoms in their most rapid and aggravated form. Dissection discovers the bronchial tubes filled with frothy mucus, and their mucous membrane highly injected. The lungs themselves, according to the duration of the case, are found in one of the stages between posterior engorgement and complete hepatization, at times with pure pus in the smaller bronchial tubes; this last is comparatively rare. The lungs vary remarkably in cases of cholera. In death, from collapse, they are frequently exsanguine, without the

usual posterior engorgement, and weigh from eighteen to thirty ounces. In examples of secondary affection, I have found them engorged, hepatized, and weighing 65 ounces—one right lung weighing 39 ounces.

We may at times avert or ward off this sequela of the cholera, but if it gain a footing, treatment is unavailing. After twenty or thirty grains of calomel in divided doses, I am much inclined to put the patient into the warm bath, and bleed him from the foot; if he be improved, and stand the loss of blood, I repeat the bleeding; if not, he is put on wine and stimulants. I have cupped and blistered very freely, with very little benefit. I think I have checked this terrible malady by the means mentioned, but I have never cured it after it was fairly established.

3d. *Head affection.* The symptoms here, in many respects, very closely resemble common typhus. We have the typhoid expression, muttering delirium, and injected eye, with cerebral symptoms seemingly proportioned to the amount of disease in the head. It is easily distinguishable from the affections already described, by the absence of the pain in the abdomen, and of the stethoscopic indications, with less restlessness; and from typhus fever, by the coldness of the tongue, the quantity of bile in the stools, and the absence of urine. In the last days of these three forms of secondary affection, the catheter finds urine in the bladder in considerable quantity, but the secretion is more deficient than in typhus.

In some cases we meet with acute delirium, at times closely resembling delirium tremens, the prominent symptoms of which last have always been, deep suspicion, and a dread of being murdered. More than one did their utmost to alarm the neighbourhood. In other cases again, the stupor and profound sleep previously alluded to have gone on increasing, the patient becoming almost as insensible to external stimuli as in cases of compressed brain.

On dissection, in some of these cases, the vessels of the dura mater, both the trunks and ramifications, were found turgid; the brain moist with fluid—in very minute quantity between the folds of the arachnoid, in greater quantity between that membrane and the pia mater, under the pia mater, in the ventricles, at the base of the brain, and in the spinal canal. The brain generally very firm. The mucous membrane of the small bowels was occasionally so much affected, especially in the very typhoid cases, as to induce me to consider it the seat of the disease. The glands of Peyer and Brunner were also found in great numbers and very large, but their appearance has been so little uniform in any stage of cholera, as to induce us to think that disease in them is accidental, not necessary. In a few cases, post-mortem examination threw no light on the cause of the symptoms, the brain being very firm, and every other organ and tissue perfectly healthy.

Wine in full doses, opiates, or leeches, and cold applications to the head, with calomel, according to the form assumed, constitutes the principal part of the treatment. I have never seen counter-irritation useful. We ought never to forget, that in this, and in all the forms of secondary fever, we may, by early treatment, arrest the progress of the symptoms, but cannot cure the formed disease. I know of no prophylactics so powerful as the warm bath, bleeding, and Seidlitz powders, proportioned to the patients' strength.

6th. *Suppression of Urine.* The state of the secretion of urine in the different stages of cholera is remarkable. Its absence during the early stages is sufficiently well known. If convalescence be rapid, even after a severe attack, the urine is secreted early, first in small quantity and turbid, and next abundantly and limpid. I have never seen relapse, severe secondary symptoms, nor death, when limpid urine had been discharged copiously, before coma had shewn itself. This coincidence is too uniform to be accidental, and is probably one cause of recovery. It also forms an excellent test of certain convalescence. During the early stages of the secondary affections already sketched, the kidneys continue inactive, and if the patient be rapidly carried off, they continue so till death. If, however, he survive until the typhoid symptoms are fully developed, the suppression ceases, but retention in most cases continues. At this period the catheter generally finds urine in the bladder, at times limpid, and in considerable quantity. I have never, however, seen it as abundant as in health. I was at one time inclined to consider the absence of urine as the cause of the secondary symptoms, and

gave diuretics of all kinds very freely. Subsequent experience, however, has induced me to reject the theory, and lay aside the practice.

Although in the majority of instances suppression of urine must be regarded as an effect, not a cause, yet I have seen cases in which the kidneys remained so obstinate after all other secretions were restored, that I cannot help concluding, that on whatever their inertness may depend, it must be looked on as the immediate cause of the subsequent train of mortal symptoms. The cases alluded to have been all old persons, who, having passed through a severe form of cholera, continued convalescing for some days, until their progress seemed checked by the obstinacy of the kidneys. Sleepiness, stupor, coma, floccitatio, coldness of the surface, and typhoid delirium, appeared in regular succession, and put a period to the patient's sufferings.

Although I have thus endeavoured to arrange the secondary affections in cholera, according to the organ principally implicated, yet in very many cases, two, or even all of the classes of symptoms separately detailed, are met with. It would be strange, indeed, were it otherwise. They have most probably all a common cause, modified by individual circumstances. It becomes a highly interesting question what is this cause, and how happens it, that a peculiar fever, remarkably uniform in many of its characters, follows every case of severe collapse? The effects left by the primary disease seem to become the causes of the secondary symptoms. These effects appear to me, as already stated, to be injury inflicted on the mucous membrane by the passage of the serous discharges through its vessels, and deterioration of the blood thus deprived of some of its essential constituent parts. That several of the secondary symptoms depend on the first of these causes I have already attempted to prove. The sympathy which exists between the intestinal mucous membrane, and the head and chest, has been long known to the profession. That the deteriorated condition of the blood is a cause of many of the symptoms, I infer from the circumstance already stated, that in some of our fatal secondary cases no lesion of the solids was discovered after the most minute examination; leading me to conclude, that the liver discharges enormous quantities of bile, because the ingredients which go to form it are in excess in the blood—that the kidneys are inert, because the fluid and saline portions are deficient—and that coma and muttering delirium follow, because the brain is implicated by sympathy, or not supplied with its healthy stimulus. Elderly persons, or younger individuals who have suffered severely from collapse, seldom recover, because the intestinal mucous membrane is not in a condition to supply by absorption the ingredients deficient in the blood, whereas children, for the opposite reasons, are almost certain to recover, if the pulse return to the wrists. I have not seen a single instance of death in the Albion-Street Hospital from secondary head or chest affection in a child.

This report is already too long, and I will not try the reader's patience by a detail of my experience of individual remedies. Suffice it to say, that I have not been slow to put every rational proposal to the test of experience, and the result has been the utter failure of every measure which has come recommended as a specific. Practice in cholera, to be successful, must be simple, and must accommodate itself to symptoms.

Saline injections into the veins is the most novel practice which has been attempted since cholera appeared in Great Britain. The results of our experience on this most interesting subject I communicated to the Medical Gazette* a few weeks ago. I had then injected nearly thirty patients, four of whom recovered. Since that time, I have tried in ten cases injection of distilled water, in quantities of from ten to twenty-four ounces, the patient being in the warm bath at the time of injection. Two cases rallied, but died in secondary fever; a third was powerfully stimulated by the injection, sat up in bed, and sang a hymn, but died delirious in a few hours. None recovered, and the practice of injecting has since been laid aside as useless, if not injurious. These last trials have confirmed the opinion I ventured to give in the Gazette, that the saline injections act as stimulants, not as specifics.

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* See our number for July 7th, page 443.—E. G.

DR. PHILIP ON THE CIRCULATION.

To the Editor of the London Medical Gazette.

SIR,

As more than one gentleman have, in the Medical Gazette, criticised my paper on the Circulation, it might be deemed a want of courtesy on my part to make no reply to them. On the head of courtesy, indeed, it will be admitted that one of these gentlemen has no claim. Dr. Badham, I am ready to acknowledge, has not deviated from the usual style of discussion among educated men.

He thinks that I am inclined to dismiss too hastily the lately supposed aids of the venous part of the circulation; but for some of the same reasons which induce him to exclude from these aids the effects of respiration, he must also abandon the resilience of the lungs, which, like respiration, can have no existence in the fœtal state. If, then, we overlook the occasional action of the muscles of voluntary motion, which he also for very sufficient reasons sets aside, there only remains of those supposed aids, the elastic power of the ventricle, which, we have seen, even in some of the more perfect animals, has no existence, and is always extremely feeble; so much so, that it can easily be proved that, even if other circumstances admitted of its operation in promoting the venous part of the circulation, it could produce no sensible effect on the motion of the blood.

But I think Dr. Badham will on reflection admit, that were it a thousand times more powerful than it is, my argument against the power of suction promoting the ascent of the blood in the veins would not be influenced by the circumstance of the *vis a tergo* being sufficient to preserve these vessels in a state of distention. This, were it the case, would only prove that there is no occasion for such a power. Dr. Badham must be aware, that the power of suction in raising the blood, can only operate in proportion to the pressure of the atmosphere on the external surface of the vessels, and the degree in which they resist this pressure; and therefore, that if the vessels can in no degree resist it, the blood can in no degree through them be raised by suction.

He will also, I think, on farther considering the subject, admit that no property of the blood itself, with the exception of its stimulating property with respect to the vessels, can in any degree promote its motion in them. Motion, which is the relative change of place among bodies, cannot, of course, originate in any property of a body which has no relation to any thing external to itself, whether the body be animate or inanimate; for it is evident that a relative change of position in its own component parts, can have no tendency to change its position with respect to other bodies.

Dr. Badham will perceive, in recurring to my paper, that I by no means deny the existence of a *vis a tergo* in the circulating system. In all fluids propelled through tubes, whether by their own action or other means, there must necessarily be more or less *vis a tergo*. I only say, as was self-evident, that the ligature wholly prevented it in the part of the vessel which was the subject of my experiment, except as far as it was regenerated by the action of that part itself.

The vein beyond the ligature was gorged with blood, by the combined power of the *vis a tergo* and the vessel itself. Dr. Badham cannot suppose it necessary to have recourse to a power of distention in the vessel, to account for so simple a consequence.

WITH regard to Dr. Hall's paper, I can only say that I believe a farther acquaintance with the subject will induce him to change his opinions. The circumstance of his imagining that I ascribe the emptying of the vessel to its collapse, and not its collapse to its having emptied itself, throws light on other parts of his communication.

There is no occasion to put the Royal Society to the trouble Dr. Hall proposes. He will find twenty of its members who have seen the capillary part of the circulation; but he stands alone, I believe, in imagining he can see in it the influence of the individual beats of the heart.

The only thing in Dr. Hall's paper which I am at a loss to account for, is the tone in which it is written. He seems to suppose that I had in view the positions in his Treatise. I can with truth assure him that I have never even seen it.

It is very provoking, that no sooner is a physiological point ascertained, however carefully, than some, little accustomed to such inquiries, are found ready again to perplex it. I know of but one remedy for such evils—time; which always at length separates truth and error. To it I willingly commit the task, and must be allowed to say that I shall not feel myself called upon to reply, except to those who have given proof of their experimental accuracy. If controversy is to be the consequence of the productions of all who choose to amuse themselves with physiological experiments, our journals will be filled with discussions which can only disgrace the science. It is easy to err in reasoning on physiological subjects and repeating physiological experiments. Let experimentalists, in the first place, prove that they have made some addition, however slender, to our stock of knowledge: it will then be time enough for them to judge their predecessors.

I am, sir,
Your obedient servant,
A. P. W. Philip.

Cavendish-Square, Aug. 20, 1832.

ON THE USE OF THE NITRATE OF SILVER,

As an Application in Burns and Scalds.

By J. C. Cox, Surgeon, F.L.S. &c.

To the Editor of the London Medical Gazette.

Sir,

I took occasion, some time since, to call the attention of the profession to the use of the nitrate of silver in certain cutaneous affections, especially herpes zoster, for which I expressed the opinion that it was almost a specific; and the employment of that remedy in many severe cases subsequently, has fully confirmed that opinion.

I now desire to recommend the use of the same remedy in burns and scalds, as one more fitted than any other with which I am acquainted to relieve the present suffering and obviate the future mischief. Where the burn is deep, and has destroyed the vitality of the cutis, of course no superficial application can do more than hasten the separation of the slough, and the spirits of turpentine is still probably the best application; but where there is extensive superficial lesion, either producing intense redness and pain, or vesication, the nitrate of silver is certainly unrivalled. I have employed it both in very strong solution and also in the solid form; but, on the whole, I think the latter by far the best mode of application. The parts being moistened with cold water, the stick of caustic is passed over the whole surface, which may be afterwards covered with cotton wadding and bandaged, where that can be conveniently done. The application does not increase the suffering—all uneasiness quickly subsides, and in a day or two the only traces of injury will be the desquamation of the blackened cuticle. Where the process of vesication has commenced, it is immediately arrested; but if the vesications have been extensive, and the cuticle torn off, or adherent to the dress, there is no application which forms so effectual and complete a protection from the external atmosphere as the lunar caustic. In the latter case, it should be rolled tightly and rapidly over the surface. It appears to combine chemically with the albumen, and to form a covering most beneficial and efficient. Where extensive sloughs have formed, and there is a large granulating surface, the nitrate of silver, in the form of strong solution, applied with a camel's-hair brush, diminishes the irritability of the surface, prevents, to a great degree, the distressing itching, and hastens cicatrization. I would here state that much mischief is frequently done by the use of astringents—as the oxyde of zinc, &c.—which causes the cicatrix to contract quickly, and thus to form seams and bridles, which produce frequently great deformity. A young lady, a patient of mine, in dressing for a party, set fire to her pelerine, and severely burned the neck, back, and shoulder. On the shoulder, a large patch was completely disorganized; to this I applied the spirits of turpentine. The other parts were either vesicated or severely scorched, extending over the back and neck; to these I applied the lunar caustic all over the surface, and wrapped the whole up in cotton wadding. In half an hour she became quite easy, went to sleep, and omitted to take

the anodyne which was prepared for her. The shoulder was the only part which required any protracted attention.

Master C——, while casting bullets, scalded his hand with the melted lead. He was in great pain, which was only temporarily relieved by cold. I applied the lunar caustic to the surface; the pain was immediately relieved, and the next day there was no soreness or inconvenience.

A boy's face was severely burnt by the explosion of gunpowder: the application of the nitrate of silver completely obviated the ill effects which would otherwise probably have arisen from the accident. The face certainly appeared grotesquely tattooed for a few days, by the application, but all traces were quickly removed.

The above are sufficient exemplifications of a mode of treatment which will, I am convinced, on trial, be found very beneficial.

I am, sir,
Your obedient servant,
J. C. Cox.

33, Montague Square,
August 11, 1832.

SUCCESSFUL TREATMENT OF CHOLERA BY THE MURIATE OF SODA.

To the Editor of the London Medical Gazette.

Sir,

At the time of the first inroad of cholera into this country, a printed paper fell into my hands, recommending large doses of muriate of soda, which had been employed with great success by Dr. Ochel (I believe) of Moscow. The successful employment of the same remedy by Drs. Ysenback and Brailow, of St. Petersburgh, was also announced in the official reports on cholera, page 108.

The first opportunity I had of trying the salt was in the case of a poor woman, in November 1831. The effect, shortly after its administration, was to produce a copious vomiting of bile, with great relief of the oppression at the præcordia, and of the spasms. Her state of utter destitution not admitting of proper attention being paid in the place where she lodged, she was removed to the cholera hospital, where, under the stimulating plan of treatment, she expired some hours afterwards.

Subsequently to this, in six other cases the salt proved successful, with only one death, in which case the patient was moribund, and the salt did not operate.

At that time I did not deem it incumbent upon me to adopt any other mode of publishing the result of those trials of the salt, than merely by recommending it privately to the attention of those who held official situations in the hospital in my own neighbourhood. But whether any attention was paid to that recommendation, I have no means of ascertaining.

On the second irruption of cholera in St. Giles's, finding the administration of salt equally successful, I considered it my duty to wait upon the physician at the local station, to write to the Central Board, and likewise to the chairman of the local board in St. Giles's.

These communications were met by the remark, "there is nothing new in the practice of giving large doses of salt as an emetic at the commencement of the treatment, but this has failed, like every other remedy which has been tried."

In a subsequent communication to the Central Board, in reply to their inquiries as to my mode of treatment, (which had enabled me to report twenty cases, eighteen recoveries, one death, one remaining) I took the liberty to suggest that the alleged failures in previous trials of the salt were owing to stimulants and narcotics having been given after its first operation as an emetic, the fatal effects of which I have witnessed in several instances. It is an error into which I once fell myself; and the patient, though apparently recovered from collapse by the salt, relapsed and died.

Turning hopelessly away from the Central Board and Local Hospital, I resolved to pursue the experiment among the poor in my district, convinced that the brandy and laudanum system had been too highly recommended, and too long sanctioned by authority, to admit of the introduction by the same individuals of another system so diametrically opposed to it. Perhaps such a revolution in a cherished opinion, and a favourite practice, would be a stretch of

candour and liberality almost superhuman. In order to diffuse a knowledge of this method of cure the more widely, I caused a circular to be printed, of which the following is a brief outline.

After the ordinary preparations are made for favouring the restoration of heat to the extremities, and promoting a profuse perspiration, by putting the patient to bed in a flannel shirt next his skin, wrapping him up in hot blankets, covering him with plenty of bed-clothes, and applying large stone-ware bottles, filled with hot water, to his feet and sides, one ounce, or two table-spoonfuls of common salt, dissolved in eight ounces of warm water, is to be administered. The patient's head is then to be covered with the bed-clothes, and on *no account* is he to be permitted to rise. Half an ounce of salt, dissolved in four ounces of cold water, is to be given every hour till a copious vomiting and purging of bile is produced, and a profuse sweat breaks out. It has rarely been found necessary to repeat the salt more than three times. After the free evacuation of bile by vomiting and purging, the blueness of the skin gradually vanishes, and when the perspiration subsides, the tongue loses its cold, moist, livid character, and assumes a dry yellow, or brown appearance, with a red margin, and the patient suffers from intense thirst. He also frequently complains of acute pain in the ears, and of tenderness in the epigastric region.

For the relief of these symptoms, one or two drachms of oleum ricini, repeated every six hours, to carry off the vast accumulation of bile, the citrate of potass, with a few grains of carbonate of soda, in a state of effervescence, repeated every three hours, and the application of a few leeches behind the ears, or to the epigastrium, are generally sufficient. To allay the thirst, let him drink moderately of thin gruel, soda-water, or even cold water, which, instead of retarding, seems to promote free perspiration, and the abundant discharge of bile, upon which his safety depends, and the retention of which appears to be the cause of all the distressing and dangerous symptoms.

The only cautions I have found it necessary to enjoin are, that the patient be not permitted to rise from bed, not to eat any solid food, and not to drink any stimulating liquor, for at least three days after convalescence is fairly established.

Under this very simple plan of treatment, only six deaths have happened out of forty cases reported to the Board.

It may be questioned if these were cases of *real* spasmodic cholera. To this I reply, that twelve were recovered from the stage of collapse, cold, blue, and pulseless; some of which were seen by the physician to the Cholera Hospital, and, in all, the characteristic symptoms of spasmodic cholera were distinctly marked. Of the six deaths, the first was a woman moribund before the salt was administered, which failed to operate. The remaining five were notorious drunkards, one of whom sunk from exhaustion, after recovering a second time from collapse, on the ninth day from the first attack: he had not been known to be sober for a week together during the last five years of his life.

The above experiments were instituted under every possible disadvantage; among a class of persons intemperate, dissolute, dirty in their habits, surrounded by filth, and destitute of every comfort, and many of them of the necessaries of life.

To the activity and zeal of Mr. Donaldson, the agent of the St. Giles's North-east District Visiting Society, who had frequently to perform every office for the sick, besides administering the salt, &c. the success attending this practice, under the divine blessing, is to be mainly ascribed.

I have only to add, that, in order to afford the profession every facility of estimating the comparative value of this method, in my reply to the inquiries of the Central Board I offered to undertake the care of any number of genuine and acknowledged cases of cholera which they might please to subject to the trial.

I am, sir,

Your obedient servant,

J. Pidduck, M.D.

87, Great Russell-Street,
Aug. 21, 1832.

IRISH COUNTY INFIRMARIES—BILL FOR THE SUPPRESSION OF QUACKERY.

To the Editor of the London Medical Gazette.

Dublin, August 4, 1832.

SIR,

I AM extremely anxious to draw your attention once again to the subject of throwing open the County Infirmaries in Ireland,—a subject in which the interests of this country are deeply involved, and one in the result of our application about which the interests of the larger body of surgeons practising here under the same circumstances as myself, are particularly concerned. You have been mainly instrumental, sir, in setting this question in its proper light before the British public; and you have, with a disinterested, enlightened, and generous spirit, taken up the cause of the Surgeons of the London College, who are exposed, in this part of the kingdom, to a most unjust and ruinous monopoly. Let us hope, sir, that we may still enjoy the assistance of your valuable pen; and that, with the support of your highly-independent journal, we may succeed in completely removing a restriction, which presses heavily on genius—is an obstacle to the advancement of science—and is maintained by the Dublin College of Surgeons for the most unworthy purposes.

I am convinced, sir, that if the subject were fully laid before the legislature, the obnoxious statute on which the monopolists rely, which shuts out the members of the College of Surgeons in London from holding Irish County Infirmaries, would be repealed; and it is partly with this view, and for the purpose of bringing the matter more immediately under the consideration of the profession in England, I would beg to submit the following draft of a Bill, prepared by the learned Professor to the University of Dublin, Dr. Macartney:—

"Whereas great injury is sustained by the public in consequence of many persons professing to understand the nature and cure of diseases and injuries, without having employed the usual means of acquiring such knowledge, or without having been examined touching their qualification to practise Medicine or Surgery by any competent authority, although by an Act passed in the 55th year of the reign of his Majesty George II. due provision is made that no unqualified persons can practise Pharmacy:—

"Be it enacted by the King's most excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal and the Commons of the United Kingdom, that no person shall hereafter prescribe medicine, or perform surgical operations, or act in any way whatsoever as a Physician or Surgeon, unless he be qualified by the license or authority of some one of the Colleges, or Corporations, or Faculties of the United Kingdom, empowered to grant medical or surgical Diplomas; or, in default thereof, to be subject to the fine of 20*l.* for every such instance in which he or she may prescribe or administer medicine, or perform surgical operations, without being so qualified or authorized, which fine shall be levied by any Magistrate, on proof of the fact, on the oath of two credible witnesses.

"And whereas much confusion and inconvenience has arisen to individual practitioners in medicine and surgery, who are duly authorised in one part of the United Kingdom, being prohibited from practising or holding public hospitals in other parts of the United Kingdom; be it enacted, from the passing of this act, that all persons duly qualified and authorised by the license of any of the legally constituted colleges, corporations, or faculties of the United Kingdom, empowered to grant medical and surgical diplomas, shall be authorised to practise medicine and surgery, and shall be eligible as physician or surgeon, as the case may be, to all public hospitals, infirmaries, and dispensaries, for the cure of disease, in the United Kingdom. Any thing contained in former acts of parliament to the contrary are hereby repealed."

This bill, sir, as you will perceive, has two objects in view: the first, to prevent ignorant quacks from trifling with the lives of their fellow-creatures; and secondly, to remove the restriction placed upon legally educated surgeons in Ireland. It cannot be denied that both these objects are most desirable, not only for the protection of the legally educated practitioner, but for the safety of the public at large; and I trust you

will be able to give it a place in your journal. A numerously signed petition to parliament has just been sent over, which Mr. Hume has promised to present and support; and, indeed, it has been at his suggestion that the foregoing draft of a bill has been drawn up*.

I remain, sir,
Your obedient servant,
C. L.

Member of the Royal College of Surgeons in London.

TREATMENT OF HERNIA.

To the Editor of the London Medical Gazette.

Sir,

In your review of Mr. Clement's work on "Surgical Affections," you speak in high terms of the information which it contains. With respect to the propriety of having early recourse to the operation in strangulated hernia, I perfectly agree with him; but I consider it highly important to notice some observations on a passage which you have quoted concerning the means to be attempted for the reduction of strangulated hernia. Mr. C. says, "after having tried the taxis, bleeding, and the warm bath, disregard all those proposals which have been made of TRYING what will be the effect of the tobacco-clyster, whether a bladder of pounded ice will do good, whether a weight left upon the tumor will reduce it, whether a purgative may possibly extricate the gut," &c.

Now, sir, I have practised surgery for thirty-five years, and cannot call to mind one case of strangulated hernia in which the reduction of the gut was *decidedly* effected by bleeding and the warm bath. But I could relate many *well marked* cases in which a tobacco-clyster and ice caused the reduction of the tumor at the moment the operation had been decided on.

I will relate one case only. About a year ago, a man, aged 65, had strangulated femoral hernia. After the taxis had been fairly and carefully tried in vain, the symptoms becoming very urgent, we determined to operate. But we thought it right first to administer an enema of tobacco, and to apply pounded ice and salt in a bladder on the tumor. These measures, to our great surprise and satisfaction, enabled us to reduce the hernia.

With respect to the idea of placing a weight on an inflamed and tender tumor, in order to effect its reduction, it is a practice I never heard of before, and surely ought not to be classed with such powerful means as tobacco and ice.

I remain, sir,
Your constant reader,
A Country Surgeon.

* This letter should have appeared a week or fortnight ago, but was deferred for want of room. —Ed. Gaz.

MEDICAL GAZETTE.

Saturday, August 25, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—Cicero.

MEDICAL REFORM—COLLEGE OF SURGEONS.

We are told by Mr. Lemuel Gulliver, of facetious memory, that when, in the course of his travels, he visited the Flying Island, he found his majesty the king, and his principal courtiers, so much abstracted by "intense speculations," that passing events made no impression upon them, till their attention was recalled by an admonitory touch on the mouth from their attendant 'flappers.' Now we begin to suspect that the Council of the Royal College of Surgeons are somewhat in the predicament of the aforesaid monarch of Laputa, and we respectfully offer our services to them in the capacity of flappers; nothing doubting but that our admonitions, which, like those of our prototypes, are well intended, will, like them, be well received.

It has been a thousand times surmised —now whispered as a secret, and now openly announced as matter for congratulation—that "something" was to be done to meet the general wish. What

this something was, no one, indeed, exactly knew: perhaps the mode of election was to be altered—perhaps the accounts were to be published annually—perhaps the library and museum were to be farther improved, and rendered worthy of a great national institution—in short, there might be difference of opinion as to what was intended, but every one agreed that "something" was positively to be done. Now all we know about the matter is, that if any change, in the way of improvement, or any act of graceful concession, be or ever were contemplated, it still remains unperformed—it still remains as one of those "intense speculations" described by our friend Gulliver as occupying the mind, without influencing the acts of the parties. Will the Council, then, not be warned? Will they still disregard alike the advice of their friends and the threats of their enemies? Are they so blind as to fancy that they are popular? Do they imagine that because the number who chose to join in a disgraceful riot within the walls of the College was small, the proportion of members who approve of their corporate acts is large? Do they suppose that, though the sobriquet of "bats" be incapable of injuring them, the terms "self-elected and irresponsible" are equally powerless? Let them not be deceived: it is one thing to give a quietus to half a dozen hot-headed and intemperate men, who violate the law of the land, but quite a different matter to smother the deep dissatisfaction of the great majority of their members. Not that the gentlemen who compose the Council are personally disliked—far otherwise. It would probably be difficult to select a body of equal number comprising so many persons entitled to, and meeting with, consideration and respect; but this very circumstance of the individual popularity of many among them, perhaps misleads them as to the estimation in which they are held collectively. It is notorious, that while personally most of them are esteemed as honourable men and skilful surgeons, and while some of them hold the highest places in science and in practice, yet the very moment that they are contemplated collectively, through the atmosphere of Lincoln's-Inn-Fields, the whole becomes changed as in a phantasmagoria, and they assume the ungracious aspect of monopolists, hostile to liberal measures, and upholding an obnoxious system for their own proper and individual gain. Nay, even their neighbour, Mr. Professor Green, who stepped forward some short time ago as a mediator, and whose distance from the council-chamber is in every sense of the word so small, cannot look upon it through the short space that intervenes, without seeing around its inmates "descensive" and other bewildering "circles," till he becomes involved in the perplexing maze of "distinction without separation;" and all his proposed improvements are lost in the confusion.

The first point is this—the mode of election must be changed. It is felt as an anomaly and a nuisance; every member looks upon it as a personal grievance, and, so long as it endures, there will be no cordiality between the Council and the general body of English surgeons — no bond of union — no mutual feeling, save mutual jealousy. We are not among those who find fault merely for the love of censure, or who raise the hackneyed cry of reform, to make our journal sell; but, looking dispassionately upon the subject, we have no difficulty in perceiving, and no hesitation in declaring, that the constitution and management of the College of Surgeons is in several respects faulty, and that the Council do not set about their reformation *as they ought to do.* It behoves them not to wait till they be driven—they ought to lead; and we earnestly exhort them—

if they would shew that they prize their own best interests, or do what seems graceful in the eyes of others —to come forward, and that quickly too, with some plan of reformation. It is no answer to tell us that they are restricted by their charter, for they must feel that an application to the legislature to liberalize their constitution would at once be listened to; and they may rest assured, that if they do not bestir themselves, the matter will be taken out of their hands, and managed for them. We are confident that if the Council were to lead the way, the Commonalty would join them in any reasonably liberal measure. The propriety, nay the absolute necessity, of some provision by which the introduction of unfit persons into the governing body might be prevented, would be as apparent to members generally as to the Council; the Commonalty, indeed, would then have an interest, and would take a pride, in electing the fittest men. Besides, they might (like the Common Council of the city, in the choice of Lord Mayor) send up two names, one of which it would rest with the higher court to choose; or various other methods might be contrived, to effect a similar purpose. One thing appears quite clear,—that those only should be entitled to vote, or be eligible as candidates, who are unconnected with any other medical corporation. At present, however, we would but urge the principle of a more popular mode of election into the Council, without pretending to dictate as to the precise manner of granting the franchise, or the precise qualification which ought to be required of the candidate.

Again, we are told that no such change is necessary, because the system "works well:" now we shall not stay to inquire whether this position be true or not—we shall not hurt the feelings of any member of the Council by suggesting a doubt of his being the most eligible person possible to fill the place he occupies, but shall content ourselves with declaring, that the question is not whether the present mode leads to the choice of the best possible persons as the governing body, but whether the odium which attaches to the manner of election does not extend to those elected, whenever they are contemplated in their corporate capacity. The Commonalty would not view the acts of the Council with one half the jealousy they now do, were that body composed of persons whom *they* had elected to their official seats. Besides, in the present instance, we are not sure that the members would be unreasonable if they called in question the absolute wisdom and disinterestedness of their rulers. If not collectively, certainly many of them did individually, give reason to believe, that they contemplated some improvements in their system. What has become of them? A year and a half has elapsed since the riots which gave immediate rise to those discussions; and the only subsequent acts that we know any thing of, are certain legal proceedings, in which they long since gained their object; and the confirmation, by the Judges, of certain by-laws, empowering them, hereafter, to expel those members who violate the obligations they have bound themselves by oath to observe. This is all very proper; but do the Council mean to stop here? and having gained all for themselves, and granted nothing to the Commonalty, are they now to cry,—hold! —if we go further, we endanger the safety of the College? Possibly we may be mistaken, but we think the edifice has much more to fear from making it the wish of all its members to undermine it, than in giving them a mutual interest in its support.

We take leave, in conclusion, to recommend to the Council to carry into effect without delay, some of those

"intense speculations" with which we doubt not they are occupied; or if it really be, that by the nature of their charter, they cannot be so liberal as they would wish, let them remember that we shall soon have assembled at St. Stephen's a reformed parliament, consisting of members returned by the suffrages of a much larger body of the people than have hitherto enjoyed the privilege of voting, and, it is to be presumed, the friends of liberal measures;—let the Council, if they require legislative interference to do what is right, be among their earliest petitioners, and let them say something of this kind,—

The Petition of the Council, &c.

Humbly sheweth,

That your petitioners are surgeons, practising in London, who have been elected into the Council of the Royal College of Surgeons by their predecessors in office,—a mode of election which gives great offence and scandal to the members at large.

That at present they are restrained by their charter from carrying into effect many liberal and salutary enactments for the advancement of science and benefit of the public; but inasmuch as the reform bill, which lately passed your honourable house, shews that "chartered rights" are no longer held to be immutable, your petitioners humbly hope that the purification which you have deemed necessary for yourselves will not be denied to them.

They trust that your honourable house will assist them in procuring a modification of their charter, adapted to the spirit of the times, which shall enable them to improve their mode of election—shall empower them to expend their surplus funds in rendering their library and museum the first in the world, and shall warrant the annual publication of an authenticated statement of their accounts. They further venture to hope that it may be rendered imperative upon them to institute such an examination of every candidate for their diploma, as shall prove incontestibly that he has received an education in keeping with the improvements which have recently taken place in all departments of science.

And your petitioners will ever pray, &c.

HOMŒOPATHY.

We expect presently a full account of the proceedings at Leipsic on the 10th of this month, the fifty-third anniversary of the doctorate of Samuel Hahnemann. The event was to have been celebrated with a kind of triumphal festivity in that very town from which the great homœopath had been twice obliged to withdraw, in the course of his mission. His disciples, from all quarters, were to have met together, and *feasted* on that day; and it is known that in dietetic matters the Hahnemanians do not abide by their usual *infinitessimal* doses.

PRIVY COUNCIL CHOLERA ACT.

Mr. Greirson, surgeon of Warrington, was convicted last week in the full penalty of 5l. for refusing to return a case of cholera to the Local Board of Health. Proceedings were taken against him for a single case only, as he promised not to withhold his returns in future.

CHOLERA IN CANADA.

A correspondent, on whose veracity we can rely, has forwarded to us the following curious piece of information:—

"No ships reached (or could reach) Quebec from England this season before the 8th or 9th of May, in consequence of the ice, and there was no cholera there the week previous to the 8th of June. A little before the latter date some ships arrived with emigrants, having had cholera on board—two of them having each lost between thirty and forty from the disease. Cholera appeared in Quebec about the 9th of June."

ANATOMICAL INSPECTORS.

Dr. James Somerville has been appointed inspector for England; and Dr. Craigie, of Edinburgh, inspector of Scotland, under the new anatomical bill.

EARLY RISERS.

(From a Correspondent.)

Among eminent early risers might have been mentioned Frederick the Great, king of Prussia, who, having found himself otherwise incapable of overcoming his natural repugnance to get out of bed, ordered one of his domestics to rouse him every morning at 4 o'clock, and to apply a wet towel to his face, if necessary, to enforce his getting up,—on pain, in case of neglecting this duty, of being condemned to serve as a private soldier — for life! This expedient completely succeeded, and the practice of rising early soon acquired the force of a habit, to which the extraordinary exploits of this most eminent person may in a great measure be attributed.

ILLNESS OF SCARPA.

This illustrious anatomist and surgeon, now at a very advanced age, has recently recovered from an attack of illness so severe, that, by the last accounts from Italy, his life was despaired of.

ON THE REGENERATION OF NERVES*.

By F. Tiedemann.

Divided nerves unite and heal. This fact has been established by the experiments of Fontana, Michaelis, Arnemann, Cruickshank, Haighton, Mayer, by Bichat, and by the more recent observations of Swan, Descot, and Larrey.

The reunion of divided nerves is attended by the following phenomena:—the ends of the nerve retract; the extent of the separation is from two to six lines, or more, and is greater in large nerves than small ones. This separation of the extremities of a divided nerve is not owing to elasticity, but is the result of an organic action, or contractility of the neurilema and surrounding cellular tissue; in proof of which it may be stated that divided nerves in a dead animal do not retract. Inflammation soon commences; the surrounding vessels contain more blood; the nerves become of a red colour, and are thicker; the inflammation extends from half an inch to an inch above and below the divided extremities; the distention of the vessels, the redness and swelling, are, however, more remarkable in the upper than the lower end. Coagulable lymph is deposited around the separated nerves, and in this lymph minute vessels are observable. In consequence of this deposition in the sheath of the nerves, and among their fibrils, the nerves appear enlarged; the swelling is greater in the upper than in the lower ends. Similar enlargements are discoverable, after the lapse of time, in the ends of nerves divided in amputation.

The coagulable lymph effused during the inflammation connects the divided nerves in the course of a few days; it gradually assumes a firmer consistence, and the blood-vessels dispersed through it appear to contract, or to contain less blood. The enlarged, or bulbous extremities of the nerves, gradually approaching nearer to each other, at length become incorporated, and thus the connexion of the divided nerve is re-established. If the swelling be examined after some time, it is found reddish externally, white internally, and exhibiting fibrils similar in appearance to the nervous fasiculi, and by means of these the nerves become perfectly continuous.

Whether the substance connecting the nerves is similar in organization to the original nerve, and is capable of transmitting sensation, and the influence of the brain in the performance of voluntary motion, are questions which have divided the opinions of physiologists. Arnemann rejected the opinion that the regeneration of true nervous substance took place, having found that, 150 days after their division, the parts supplied by such nerves were destitute of sensation. Breschet, Richerand, and Delpech, adopted the same view.

Fontana, on the contrary, Michaelis, Mayer, Cruikshank, and Haighton, maintain that the reunion of divided nerves takes place by means of true nervous fibrils. Michaelis recognized these fibrils by the microscope, and Mayer demonstrated them by the test of nitric acid. Haighton, in proof of the regeneration of nervous matter, stated that the reunited *nervi vagi* were capable of performing their natural functions. He divided the nervus vagus of the one side in a dog, and in six weeks afterwards, that of the other side. The ani-

* Translated and abridged from Tiedemann's Zeitschrift für Physiologie, iv. band, 1 heft. Heidelberg and Leipsic. 1831.

mal lived; but when the two nervi vagi were divided at the same time, or within a shorter interval, the animals invariably died.

From the preceding experiments and observations, Tiedemann considered it highly probable that true nervous matter was regenerated; he thought, however, that the return of sensation, and the power of motion in parts whose nerves had been divided, was not established so satisfactorily as was desirable. He therefore instituted some experiments on the subject, and one of the most conclusive of these he has detailed nearly as follows. On the 16th of August, 1827, having exposed the brachial plexus of nerves [*arm-nerven-Geflecht*] in a dog, he separated the several nerves, and cut out of each a portion from ten to twelve lines in length. The leg and foot were immediately deprived of sensation and the power of motion. The wound healed in three weeks, but the leg and foot remained without sensation, or the power of motion, for a long time. It became smaller than the opposite one, and in walking or running, was drawn upwards by means of the muscles of the shoulder.

In May 1828, eight months after the excision of the portions of nerves, the animal began to use the foot in progression, and shewed signs of sensation when it was much pressed, or was pricked with needles; and during this and the following years, sensation and motion were gradually but perfectly restored. In order to examine the condition of the nerves, the animal was killed on June 2d, 1829, twenty-one months after the operation. Where the portions of nerves had been removed, at each extremity of the incision an oval enlargement was found, which was greater at the end nearest the body than the opposite one. In the interval between these enlargements, and connecting them, newly-formed portions, apparently of nerve, were seen. These intermediate portions were thinner than the uninjured parts of the nerve. In order to ascertain whether the regenerated parts really consisted of nervous fibrils, a portion was laid on a piece of glass, and nitric acid applied, but the integrity of the structure remained unimpaired. Hence, from the return of the power of motion and sensation, from the structure of the newly-formed portion, and from the test of nitric acid, Tiedemann concludes this experiment to supply a demonstration of the regeneration of true nervous matter.

Numerous cases are related, for the most part by English authors, (as Abernethy, Balfour, Pring, and Swan) of the restoration of sensation after the healing of wounds in which nerves were divided either by accident, or in operations for the relief of neuralgia. Of these the most remarkable is related by Abernethy, and one very analogous to it has been communicated to Tiedemann by Dr. Schott, of Frankfort.

A woman, 40 years of age, had suffered most severely for fourteen years from neuralgia of the ring-finger, particularly of the last joint, for which she could not obtain any relief. Dr. Schott determined, therefore, to remove a portion of the cubital nerve. He laid bare the trunk of this nerve above the inner condyle of the humerus, and removed a portion of it an inch long. After the division of the nerve, the neuralgia immediately ceased, and the ring and little finger of that hand were deprived of sensation. In order to prevent the reunion of the ends of the nerve, the wound was dressed to the bottom, and healed by suppuration. After three months, the wound was cicatrized, and there was no return of neuralgia; but gradually sensation returned in the fourth and fifth fingers, and after six months had elapsed, she again suffered severe pain in the ring finger, which, however, did not become so violent as before the operation.

ST. GEORGE'S HOSPITAL.

Two Cases of Concussion, *attended with peculiar symptoms: with* Clinical Remarks *on* Bloodletting *in Injuries of the Head.*

By Mr. Cæsar Hawkins.

Case I.—William Edwards, æt. 35, admitted July 25, under the care of Mr. Hawkins.

The evening before his admission, he fell from behind a carriage, and struck the back of his head violently against the ground, by which he was stunned for a considerable time.

Directly after the accident, he was bled from the arm, and in the course of the evening from the temporal artery, so that by his account he lost altogether a considerable quantity of blood. On his admission, he seemed to be still labouring under the effects

of concussion, sleeping a good deal, and complaining much of pain in the head.

Ordered to use cold lotion to the head, and to take saline mixture, with 20 drops of Antimonial wine, and half a drachm of Epsom salts every six hours. Fever diet.

26th.—Less sleepiness, but great pain over the whole head, with much intolerance of light, the pain being aggravated by it, so that he lies with his eyes constantly shut, though totally unable to obtain any sleep. Both pupils act, but sluggishly, especially the left. Tongue foul; bowels open; pulse slow and weak, 60.

27th.—Pain rather less; tongue more loaded with a white crust, brownish in the centre; bowels open; pulse 64, slightly intermittent.

Antimonial powder and Calomel, of each 5 grains, at bed-time. Half pint of beef-tea.

28th.—Antim. powder, Calomel, of each 3 grains, at bed-time. A pint of beef-tea.

29th.—Pain in the head and eyes equally violent; pulse 64, small and weak, though not intermittent; tongue thickly coated with creamy fur, less brown in the centre; bowels moderately open each day.

Twelve leeches to the forehead, continuing the cold lotion. Increased diet, with 1½ pint of beef-tea.

30th.—Pain directly relieved by the leeches, though not quite removed; less intolerance of light; slept better; pulse considerably *fuller*, but soft, 64.

31st.—Ordered to have the ordinary diet of meat, &c.

3d August.—Gradually improving, though not quite free from pain; countenance cheerful; pulse *seventy, fuller*; tongue gradually cleaning; pupils act naturally and readily.

14th.—No complaint.

Case II.—James Neill, æt. 28, admitted July 28, under the care of Mr. Hawkins.

He fell from a gig upon his head and shoulder, by which he was stunned for an hour, and fractured his left clavicle; and he had some bleeding from the nose and throat, and from the left ear. He was bled largely from the arm before his admission, and was intoxicated. He was admitted in the evening, and during the whole night was violently delirious, and constantly moving about, wholly regardless of his fractured clavicle, from which he has removed all the bandages.

30th.—He has recovered his senses in great measure, but cannot be kept quiet; he complains much of violent pain in the head; pupils contract, though slowly, but the light hurts his eyes very much; bowels open; pulse small, but not increased in frequency.

Saline mixture. Fever diet.

31st.—The pulse having slightly risen, he was *bled* last night from the arm, but directly became faint, so that *only four ounces* were taken, and the blood was found to be neither inflamed nor firm; bowels not open.

Calomel, Antimon. powder, of each 5 grs. at bed-time. Senna draught in the morning.

2d August.—Complains much of the pain in the head and intolerance of light; pupils contract; bowels not much opened by his medicine; pulse quick, but weak; tongue coated with creamy fur. He is still very restless, but has less tendency to delirium, from which he has not been hitherto quite free. Head rather hot and flushed, and the eyes slightly suffused.

Calomel 2 grs. Opium 1 gr. this evening and in the morning.—Ten leeches to the forehead, with spirit lotion.—Beef-tea a pint.

3d.—Pain not relieved; pulse 120, weak and irritable.

Beef-tea, a pint and half.—Calomel, Antim. powder, of each 4 grs. at bed-time.

4th.—No amendment; very restless; tongue more thickly coated with the same white fur; pulse 120.

Twelve leeches to be repeated.

5th.—In the same state.

Beef-tea, two pints.—Calomel 3 grs. Ext. of Hyoscyamus 5 grs. each, night and morning.

7th.—Considerably improved; much less pain in the head; pulse fallen to 76, fuller; tongue cleaner; bowels open; mouth and lips affected by the calomel.

To omit the pills.—Ordinary diet.

℞ Mist. Camphoræ ℥iss. Potass. Tart. ʒj. Confect. Aromat. ℈i. Carbon. Ammon. gr. iv. M. 6tis horis sumend.

From this time he continued to mend, but left the house contrary to advice, in order to return to his duty of writing as a clerk, a few days afterwards.

We have continued the notes of the cases to their conclusion, but we should observe that the clinical lecture of Mr. Hawkins, which we subjoin, was given on the 2d of August, while the symptoms in Neill's case were at the worst.

Here are two cases presented to your notice, not of very common occurrence, al-

though, as is often the case, they happen, fortunately for us, to have been admitted at the same time; they are precisely similar in their most prominent features, and suggest many useful reflections upon *Bloodletting* after Injuries of the Head, and they will enable me to shew you, that bloodletting, when carried to too great an extent, is equally injurious with abstaining from bleeding altogether.

Both these patients, after an injury of the head, have complained of intolerable pain in the head, which produced an expression of great anxiety, and has been so severe that it has kept them awake for several successive nights, and has occasioned so much restlessness in one of them, that it has been impossible to keep his broken clavicle moderately quiet. Each of them has had excessive intolerance of light, and the pupils have contracted very feebly and sluggishly, and irregularly, and the attempt to move or sit up is attended with great giddiness and increase of pain. Now such symptoms as these may attend inflammation of the brain, or such intense pain as they experienced may arise from pressure produced by effusion of blood on the surface of the brain. Mr. Brodie met with a case, in which this pain was instantly removed by elevating a portion of depressed bone. What is it, then, which induced me to increase the diet of these patients, while I ordered leeches to the temples,—to blow hot and cold, as it were, at once, though, in reality, the two plans of treatment are perfectly compatible with each other? It is briefly because I believed they suffered, not from either of these causes, but rather from too great or uncalled-for bloodletting in the first instance, which has subsequently obscured the symptoms, and may easily give occasion to the mistake of such patients being still further lowered by depletion, the effects of which it may be long before they recover from. Neill, indeed, has been bleeding from the ear and nose, which sometimes indicates fracture of the base of the skull; but in his case, from the bleeding being arterial—from his having fallen upon his shoulder—and other circumstances which I shall not enumerate, I am induced to think it only leads to the suspicion of the concussion having been rather severe at first.

It is, you are aware, a very common practice to take blood away from a person directly after he has received a severe blow upon the head. The public expect it to be done, because the practice is so generally adopted; and, accordingly, each of these men was bled copiously, in compliance with this expectation, or because their surgeons had not considered whether it was really necessary or not. But let us examine shortly for what reasons we should really have recourse to depletion at any time, after such injuries as these.

1. The first and direct effect of concussion of the brain is upon the function of circulation, producing syncope, or a state of collapse more or less tending to syncope. The circulation is in fact so far under the influence of the nervous system, that any sudden or violent injury of the brain or spinal marrow, or any severe injury which does not directly implicate these organs, but which only induces a shock upon the nervous system, may be directly or very rapidly fatal. Is bleeding, then, a mode of practice which can be expected to recover a person from this state, when the heart's action is almost stopped, and the pulse vacillating, and scarcely to be distinguished? Clearly not. [Mr. Hawkins then related the case of a boy who was knocked down by a carriage, and stunned, and directly bled; who was in a state of complete collapse when admitted into the hospital, from which he never recovered, but died a few hours afterwards.] Wait, then, till this condition is recovered from—till the heart acts with greater force, and the pulse is again felt at the wrist. Reaction takes place at various periods after the concussion; and the force of the re-action is generally proportionate to the previous depression, so that depletion to a moderate extent then becomes highly necessary, to guard the injured brain from the effects of excited circulation. If you bleed before this period, you may prevent re-action taking place at all, and the consequences may be fatal, as in the instance I have related. Even when re-action does take place, the collapse may recur, so that you must watch the state of the pulse, and the effect it produces; you must restrain the circulation within moderate bounds, but not depress it too much by too early depletion, or by carrying it beyond just limits. You observed the immediate effect produced by a very small bleeding, when slight re-action took place in Neill. The depletion was therefore very properly discontinued, or it might have been very injurious.

But perhaps you may ask, whether, since fatal syncope may be the effect of severe concussion, you ought not to exhibit stimulants to obviate this danger, and induce reaction at an earlier period. And no doubt in some rare cases stimulants are necessary; but it is in very few cases indeed that you are required to exhibit them. If you give them unnecessarily, the obvious consequence will be, a proportionate degree of re-action, requiring active depletion, to prevent in fact the effects of your own treatment, rather than those of the injury. In by far the majority of cases nothing at all is necessary during the first stage of concussion, but the quietly placing your patient in the recumbent pos-

ture. If, however, you should find, that instead of his gradually recovering from the state approaching to syncope, the pulse should in some unusual case become more and more feeble and intermitting, and the extremities more cold, administer cautiously some ammonia, or æther, or other diffusible stimulant, which may rouse the heart's action without producing permanent excitement.

2. Let us suppose, then, that the first stage of collapse has passed, and that the circulation is restored nearly to its natural standard, or perhaps a little above this; what are now the motives for having recourse to bloodletting in the manner that I recommend? The reasons for depletion are of two kinds; the first, to obviate immediate danger, and the second, to ward off remote consequences. First, the immediate danger is the occurrence of hæmorrhage. The smaller vessels of the surface of the brain are often ruptured, and the substance of the brain slightly torn, by severe concussion, so that numerous bloody points are found towards the surface of the convolutions; and if the patient dies two or three days afterwards, you will find these points softened and pulpy. [Mr. H. here shewed a preparation in which this condition of the brain was seen.]

When re-action, therefore, has taken place, bleeding becomes highly necessary, to prevent effusion of blood over all this surface; indeed, it is no uncommon occurrence for some larger blood-vessel to be ruptured, so that the patient recovers his senses when the circulation begins to return, but quickly loses them again, and falls into a state of complete and fatal compression. It is plain, that if any measures can prevent this internal hæmorrhage, it must be bleeding; and you will observe another reason for great caution in the employment of stimulants in the first stage of concussion. Moderate collapse is exactly the state you would wish to exist; and your bleeding, when re-action takes place, in all severe injuries of the head, is designed purposely to continue the state of depression of the powers of the circulation, till coagulation has taken place to defend the weakened or ruptured vessels against further hæmorrhage, if you have any reason to apprehend this danger.

The second, or remote consequence of concussion, against which you have to guard, and to prevent, if possible, is inflammation of the brain, with which view bloodletting is no doubt highly proper; which may be repeated once or twice, if the pulse is full and quick, in the next few days. But here also, gentlemen, take care that you do not bleed too often or too copiously, lest you run into other dangers of not less magnitude. Perhaps your patient has the laceration of the substance of the brain which I have just alluded to;—bleed him largely, as I have often seen done, till the face is pallid and the whole capillary system nearly empty, and your patient may die, because the capillaries of the brain cannot throw out lymph, or otherwise repair the mischief which has been effected. Such an injury can no more be recovered from without *some* activity of circulation, than a patient in a similar condition could obtain union of a fractured bone. Or perhaps your patient may be an habitual drunkard—or he may be actually intoxicated at the time of the injury, as one of these men was. You know how frequently severe or even fatal delirium traumaticum takes place, when such persons are merely deprived of their accustomed stimulant potations, and how cautious you find us sometimes in keeping them on the low diet of the hospital, even when severe local inflammation is going on; *à fortiori*, therefore, will they be liable to it if active depletion is at the same time employed, and the injury is one of the brain itself. In fact, you must almost always abstain from bleeding any person while in a state of intoxication, as this man was when he was bled, previous to his admission. But further, are you certain to prevent inflammation of the brain even by active depletion?—and if it does occur, what further resource have you left, when the patient is already lowered as much as he will bear? Recollect, that it is not the strong and vigorous who are most liable to inflammation, though it is often most severe in them, if it does occur; but it is the naturally weak, or those who are debilitated and disturbed in health from any cause, who are most easily acted on by any of the causes of inflammatory action.

Or, in the last place, if you have recourse to active depletion, you may so disturb and derange the circulation within the brain, that a state of general debility, with local congestion, may be the consequence; as I believe to be the case in the two cases under our consideration.

The general line of practice, therefore, in concussion, should be to bleed your patient pretty largely when reaction is established; largely, that is to say, according to the system of each individual; and if the pulse remains excited, or rises at some subsequent time, to repeat the bleeding in smaller quantities, paying at the same time great attention to the diet of the patient, and keeping him quiet and cool, and free from excitement; employing at the same time active purgatives and tolerably large doses of tartrate of antimony. But even with these you must be careful to employ the same caution and judgment which I am endeavouring to inculcate with regard to actual bloodletting. You may be assured that too great starvation, or too active purging, as well as undue loss of blood, are all contrary to the principles which should regulate your treatment

of these interesting and often obscure cases of injury of the head.

Probably much error has arisen, both in medical and surgical practice, from the peculiar nature of the circulation in the brain not being properly understood. We use the terms fulness of blood in the head, and emptying the vessels of the brain, without recollecting that neither of the expressions are correct, as they may be in other parts of the body. The other organs of the body are subject to the influence of the air, and may contain much less blood at one time than at another; but atmospheric pressure cannot be exerted through the cranium, and consequently, granting the incompressibility of the cerebral matter, the actual *quantity* of blood within the cranium must always be the same. The circulation may, however, be altered and disturbed in various other ways, and thus the functions of the brain may suffer. 1st. If a person is bled largely, or depletion is employed in any other way, the quantity of blood in the whole body being materially lessened, the *relative proportion* of blood in the brain to that elsewhere is altered in a corresponding degree; the brain containing just as much as usual, the diminution must be suffered in the rest of the body.

2dly. The quantity of blood in the brain being the same, the *relative situation* of the blood may be altered. If blood is extravasated, producing what is termed compression of the brain, blood to the same amount is prevented from entering the cranium; or the vessels of one hemisphere, or of the surface of the brain, being unusually full, the vessels of the other hemisphere, or of the interior of the brain, in each case respectively, will be proportionately contracted and empty.

Or, 3dly, the *rapidity* of the circulation may be very different, under different circumstances; so that although the brain contains the same quantity as usual, and that quantity is equally distributed through its substance, yet a much larger quantity may circulate through the brain within a given time, under some conditions of the heart's action, than in others; and the functions of the brain may consequently be excited at one time, or depressed at another, in correspondence with the rapidity or tardiness of the circulation.

Or, lastly, the *balance* of the circulation in *the arteries and veins* may be much disturbed, so that although the quantity of blood actually within the cranium be always the same, yet the veins may be full at one time and the arteries at another; there may be venous congestion or arterial action—the blood being, in the former case, principally found on the surface of the brain and in the sinuses, and in the other being in the interior and at the basis of the brain.

In all these various ways the circulation may be altered and deranged; and thus it is that the functions of the brain are excited or depressed, or irritated and disturbed, so that the most opposite states of the system may give rise to the same symptoms. Thus it is, to return to the subject of our present cases, that the debility arising from unnecessary depletion may produce the same symptoms which in another case may arise from excitement of the circulation.

But, you will ask, are there no means of distinguishing in any case from what cause the disturbance of the functions of the brain arise, so as, on one hand, to avoid the error of bleeding, when we ought to feed our patients generously; or, on the other, of giving them porter or wine, when we ought to take away blood? Observe the distinctions in the present cases. Are the acute pain and intolerance of light to be regarded as symptoms of inflammation? This opinion is contradicted by these circumstances. The pains commenced from the first, soon after they had been bled; and, consequently, before inflammation was likely to occur. The pulse in each of them is small, soft, compressible: in Edwards it has been very slow for several days, not above 60; and although it has been quickened in Neill, yet its softness shews it was not an inflammatory pulse. The same treatment quickened the pulse of one, and lowered its frequency in the other. The intolerance of light has not been attended with a contracted and easily excited pupil, but although the candle induces action, yet that action is slow and variable. The countenance has been for the most part pale, and the skin cool; and in Edwards, at least, there has been no redness of the conjunctiva. Neill, it is true, had some flushing of the face, and redness of the eyes, with increased quickness of pulse, which induced Mr. Cooper to take away a little blood, but he very properly desisted from it before four ounces had been taken, because the loss of this small quantity brought on fainting, and depressed the pulse, and the blood presented none of the appearances of inflammation, shewing that the apparent increased action of the arterial circulation was the result of irregularity only, not of the excitement. Both these patients, instead of greater liveliness and quickness of manner, were dull and heavy: even Neill was so, in spite of his irritability and restlessness. Another diagnostic mark might be seen in the condition of their tongues, which were both coated with a thick, white, creamy fur, inclining to yellowness in the centre—a state of tongue peculiarly observed in great nervous irritation.

But, again, might not the intense pain, with sluggishness of the pupils, and dulness of countenance, and of intellect, with torpor of the bowels and slowness of pulse,

indicate pressure from effusion of blood? In that case you would not have had a soft and small pulse, but it would have been full and laboured with the slowness; and in Neill you will observe it was actually quickened to 120. The pupils, it is true, were sluggish, but they were in neither case dilated, nor on either side; and although they contracted slowly, yet this action was regular and uniform, and the diminished contractility of the iris was accompanied with increased sensibility of the retina to light, which would not have been the case if the nervous energy of the organ of vision was itself impaired.

On the whole, then, I judged that the symptoms in these two cases arose principally from their having lost too much blood, by which the circulation in the brain was rendered irregular in one or more of the ways I endeavoured to explain to you, by which, probably, a state of congestion was produced, the blood circulating with less force or less rapidity, or not equally, through all the vessels of the brain. You have lately seen an instance in which this congestion, without any attempt at excited action, was actually fatal.

Mr. Hawkins here mentioned the case of a patient of Mr. Babington's, in which a woman, after an injury of the head, never recovered from the depression of the circulation, but gradually sunk some days afterwards, notwithstanding the free employment of stimuli, without any morbid appearances being observed, except a small quantity of serum in the ventricles.

When, then, after injuries of the head, you observe from such signs as those which I have pointed out that the symptoms probably arise from disturbance only of the circulation, accompanied with a weak pulse, and other indications of debility, you must beware of further depletion, notwithstanding the intensity of the pain. You must cautiously increase the general strength of the patient by augmenting his diet, by giving him gentle stimulants of carbonate of ammonia and camphor, and nitrous or sulphuric æther. You may quiet the irritability of the brain by cold lotion, or by ice applied to the head, which frequently affords great relief. I do not like to employ opium, in any quantity at least, in this irritability following an injury of the head, though it is of great service in the similar irritability which arises from depletion after other morbid affections.

If there is evidence of vascular congestion, you can employ with advantage a few leeches to the temples. You saw, indeed, immediate relief from them in Edwards; slight local depletion being perfectly compatible with a general tonic system—the torpid vessels being thus unloaded, and the passage of the blood through them facilitated. Very often the patient is much benefitted by small doses of calomel; under the use of which the tongue will gradually clean, and the other symptoms diminish. Neill was more benefitted by mercury than by the leeches; and I have sometimes found a blister to the back of the neck required, to prevent effusion of serum, which you have seen may attend a state of irregular circulation, with depression of strength.

Under such a method of treatment as this, you observe that the symptoms are already much mitigated in Edwards, whom, in fact, I consider convalescent; and I have little doubt that the same measures will be equally successful in the other patient. It is true, nevertheless, that you will meet with cases in which there will be more doubt than in these two persons; yet if you have any doubt as to the nature of the symptoms, you will do well to stay your hand a little; and if the symptoms are not mitigated by fair measures of depletion, which are most frequently called for in concussion, you should wait a little, observe the patient carefully, and perhaps try with proper caution the opposite plan of treatment. [Mr. Hawkins here mentioned a case of violent convulsions, which were stopped by allowing nourishing diet to a patient, who had been bled without benefit, quoting it from Mr. Brodie's paper on Injuries of the Head.]

LONDON HOSPITAL.

Fungus Hæmatodes on the Hip—Removal.

E. Rippen, aged 34, admitted February 29th, has usually enjoyed good health. About seven months since, she first perceived a tumor upon the right hip, which was slowly increasing in size. She was seen at home by Dr. Ramsbotham and Mr. Luke, about five weeks ago. The tumor, which was then about the size of a small orange, was rapidly increasing, and she was in the sixth month of pregnancy. As it was evident, that if an operation was not performed till after the period of parturition, the disease would be too much advanced to admit of removal, it was determined to bring on premature labour as a prior step to the performance of an early operation, and this was done about a month since by Dr. Ramsbotham.

The tumor is now about the size of a small cocoa-nut, situated over the trochanter major, and appears to be completely detached from the deep structures; the skin covering it is shining and discoloured: it has ulcerated at one spot, from which a dark fungous matter has sprung up, which sometimes bleeds; there is no affection of the glands of the groin, and she has never experienced any pain from the tumor. Her general

health has failed; she is weak, and strongly prepossessed with the idea that an operation would prove fatal.

March 1st.—A consultation of the surgeons having taken place, it was determined that the disease should be removed. Mr. Luke performed the operation by making two elliptical incisions, and then dissecting the tumor from the glutæus maximus muscle to the fascia, covering which it was closely connected. A few vessels only required ligatures, and the wound was dressed. The tumor was enclosed in a firm capsule, attached to the fascia, over the glutæus maximus muscle, and was of the character of medullary sarcoma, affording a beautiful specimen of the disease.

From the day of the operation her health improved; the wound proceeded favourably, and continued gradually to heal.

April 19th.—The wound had nearly closed, and she was discharged.

Recto-Vaginal Fistula cured by an Operation.

Bridget Codley, aged 30, admitted into the hospital, under Mr. Scott, May 24th, with a fistulous communication existing between the vagina and rectum. She stated, that she was confined about ten weeks ago: the labour lasted from Tuesday night till Saturday morning, and the child was born dead. Ever since, she has been constantly subjected to the passage of part of the fæces through the vagina, which miserable condition induced her to apply for relief at the hospital. The fistulous communication is situated about two inches from the commencement of the vagina, and will admit the point of the finger to be passed into the rectum; the margin is extremely callous, and the mucous membrane of the rectum protrudes a little into the vagina.

June 13th.—A dose of castor-oil having been given the evening previous, and an enema in the morning, so as completely to evacuate the bowels, the patient was placed upon the operating table, and the hands and feet were tied together, as in the operation of lithotomy. The upper part of the vagina being kept back by a spatula, Mr. Scott caught hold of the edges of the opening with a pair of hooks, and with a blunt-pointed bistoury paired the edges. By means of a porte-aiguille, three needles armed with ligatures were separately passed through the margins of the openings, which were brought closely in apposition, and the ligatures tied. The woman was then sent to bed, and allowed nothing but fluid nutriment.

18th.—The fifth day after the operation, the sutures were carefully removed. There was no passage of the fæces through the vagina until June 27th, when a small quantity was observed to have passed; and on examination, nearly the whole of the fistula was found closed, with the exception of a small aperture at the outer edge. This was left, in expectation that the granulated edges might unite, which accordingly took place. No more fæces passed through the vagina, and the woman was discharged cured July 19th.

Case of Self-Castration, in a fit of Temporary Derangement.

Thomas Walker, aged 16, admitted June 7th, for two wounds in the front part of the scrotum, each about an inch in length, and situated one on each side of the raphé; there was considerable bleeding, from which he had become exhausted and faint; he was delirious, but speedily recovered. Upon examination it was discovered that the scrotum did not contain any testicles; and on inquiry, he gave the following account, at the same time expressing much regret for his folly. He stated, that for about a week previous he had been in low spirits, but without any assignable cause. He had gone to bed the night before in a tranquil state of mind. Early in the morning, however, a sudden thought struck him that he would do some injury to himself. His first determination was to cut his throat: this afterwards changed to a resolution to perform the following act of mutilation. Having left his home in the Whitechapel Road for some fields in the neighbourhood, he first passed a string tightly around the root of the scrotum; with a pen-knife he then made an incision to the extent of an inch on one side, and having squeezed the testicle through the opening, excised it, and then proceeded to treat the other in the same way. The loss of blood he stated to be very great, which he restrained by drawing the string tighter. After admission, ligatures were placed on the spermatic arteries, and the wounds closed with adhesive plaister. The testicles were subsequently found in the field where he committed the act of mutilation. On one side, the cord was cut about an inch above the testicle; on the other, close to its upper part. He was not conscious of any pain in the operation; and though he could not assign any reason for selecting this mode of mutilation, he said that he had read in an Encyclopædia an account of castration.

July 19th.—The ligatures separated in a short time, and the wounds are now entirely healed. His voice is slightly changed, and he enjoys good health and spirits, not having evinced any symptoms of insanity since the time at which he was admitted. He talks and jokes concerning his situation, without appearing at all to feel his loss.

Discharged.

IMPORTANCE OF MIDWIFERY.

To the Editor of the London Medical Gazette.

Sir,

On looking over the weekly account of burials, which you have given from the "Bills of Mortality," in the Gazette of last week, I could not but feel impressed with the truth of an old and often-repeated observation—namely, that in entering into the medical profession, a man took upon himself duties of the most responsible kind; but I have neither time nor inclination just now to moralize upon the subject, my object being simply to point out what I am sure every sensible and well-disposed mind will consider a great defect in our medico-legal code. I observe, and I confess with no surprise, that in one week nine women had died in child-bed. It is true that two are reported under the head of "Miscarriage," but as abortion seldom terminates fatally in the early months, we may reasonably conclude that they were cases of premature labour. Now, although this mortality may not be thought great, when the population of this vast metropolis is considered, still it is enough to convince every impartial and thinking mind that the obstetric department of medicine is of more importance than our corporate bodies are willing to believe, and that, as a branch of science, it does not receive its proportionate share of cultivation. It is certainly a strange anomaly in our medico-legal system, that candidates for a diploma, or license, should be expected to give some account of their acquirements in every branch of medicine and surgery, with the single exception of midwifery. The principles of midwifery are few, and, if well understood, I am satisfied, from personal observation, that many valuable members of society would be spared. Surely, then, it is a duty which our medical corporate bodies owe to the community at large, to afford their protection to the "most amiable part of the creation" in the hour of peril, by ascertaining the competency of every candidate to practise in this particular branch. To obtain an object so long and so loudly called for, it was imagined, was the object of the "Obstetric Society:" but, alas! *parturiunt montes*, &c. The corporate bodies at length consented to receive certificates of attendance on midwifery lectures, and the Society became defunct!

In the hope that these few remarks may find a place in your journal,

I remain, sir,

Your obedient servant,

A General Practitioner.

August 21, 1832.

DR. ELLIOTSON'S LECTURES.

We have received several letters inquiring about the resumption of Dr. Elliotson's Lectures: in answer, we beg to state that they are to be resumed the beginning of October, in our next volume. Their discontinuance was not optional with us.

WEEKLY ACCOUNT OF BURIALS,

From the "Bills of Mortality," Aug. 21, 1832.

Abscess	3	Hooping Cough	10
Age and Debility	31	Inflammation	41
Apoplexy	9	Inflammation of the Bowels & Stomach	27
Asthma	9	Inflammation of the Brain	2
Childbirth	5	Lungs and Pleura	3
Cholera	116	Insanity	16
Consumption	63	Liver, Diseases of the	3
Convulsions	40	Measles	15
Dentition or Teething	13	Mortification	7
Dropsy	23	Paralysis	3
Dropsy on the Brain	13	Small-Pox	18
Dropsy on the Chest	4	Spasms	1
Erysipelas	2	Stricture	1
Fever	21	Thrush	4
Fever, Scarlet	9		
Fever, Typhus	6	Stillborn	12
Gout	2		
Hæmorrhage	1		
Heart, Diseases of	1		

Increase of Burials, as compared with the preceding Week 62

METEOROLOGICAL JOURNAL,

August 1832.	Thermometer.		Barometer.	
Thursday . 16	from 50	to 77	29·90	to 29·98
Friday. . . 17	49	74	30·04	30·10
Saturday . 18	47	71	30·00	29·90
Sunday . . 19	48	71	29 73	29·79
Monday. . 20	47	73	29·96	29 99
Tuesday . 21	47	71	29.87	29·76
Wednesday 22	52	69	29·62	29·73

Wind, S.W.

Except the 16th, 17th, and 19th, generally cloudy, with frequent light showers of rain; thunder in the afternoon of the 22d.

Rain fallen, ·3 of an inch.

Charles Henry Adams.

NOTICES.

Mr. Grove Berry. The images, in the case alluded to, are formed anterior to the intersection of their visible direction,—that is, what *we* understand by the lines of visible direction, and what Sir D. Brewster seems to understand, in his Treatise on Optics.

An Irishman in London should have authenticated his letter.

We have been favoured, by the Central Board, with the perusal of a paper by Mr. Moss, of Eton, relative to the saline treatment of cholera. We regret that we have not been able to make room for it.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 1, 1832.

ON THE POISONOUS PROPERTIES OF THE SALTS OF LEAD.

By Anthony Todd Thomson, M.D. F.L.S. &c.

The opinion that the salts of lead possess poisonous properties, originated at an early period in Europe. We find, not only that Galen and Celsus mention the injurious influence of water transmitted through leaden pipes, on the bowels of those who drank it, but that Vitruvius, the Roman architect, cautions builders against employing lead for conducting rain-water from the roofs of houses, because cerusse, that is, carbonate of lead, is formed on it, which is hurtful to the animal system. Modern observation and experience have proved the accuracy of these opinions; and have, besides, demonstrated, that a great number of the accidents which have been attributed to metallic lead, are solely owing to the carbonate, which is formed under a variety of circumstances not formerly suspected.

It is not the object of this paper to enter into any details in proof of the poisonous influence of carbonate of lead—sufficient evidences of it are contained in the writings of Sir George Baker, in the essays of Dr. Percival, and in many other medical publications; but it is intended to prove that erroneous opinions exist with respect to the other salts of lead, especially the acetate and the subacetate, which are supposed to be nearly as poisonous as the carbonate.

The prevailing opinion is, that these salts act in the same manner as the carbonate, and produce painter's colic and paralysis. I trust, however, I shall be able to prove that, when poisonous effects follow their employment, whether they are taken into the stomach, or applied to the surface of the body, they are converted into the carbonate of lead.

Those salts which are not capable of being converted into the carbonate, exert, as I shall endeavour to prove, no deleterious influence whatever on the animal economy, further than that degree of irritation which all saline substances produce on the coats of the stomach, when introduced into that organ in a large quantity. I was induced to adopt this opinion respecting the salts of lead, from having frequently observed that the carbonate, in whatever manner it be brought into immediate contact with the human body, whether received into the stomach, or thrown into the intestinal canal, or applied to the surface, especially if the skin be denuded of the cuticle, causes, sooner or later, either colic, or paralysis of the extremities, and sometimes proves fatal. On the other hand, acetate of lead, which was introduced as a remedial agent by Paracelsus, and has ever since been employed very generally as a powerful sedative astringent in hæmorrhagies and several other complaints, although it is suspected of producing the same effects as the carbonate of lead, yet it has rarely, as far as my observation has extended, been followed by either colic or paralysis, even when its use has been continued for a considerable length of time, and when it has been administered in larger doses than are generally considered safe. Instances are recorded in which from one to six drachms have been accidentally swal-

lowed without any injurious consequences.

I do not, however, mean to deny that the use of the acetate may occasionally be productive of colic. I once, indeed, witnessed an instance of violent colic, followed by paralysis, which ultimately proved fatal, from the internal administration of the sub-acetate, Goulard's extract of lead, in solution. Many cases are also related in Sir George Baker's and Dr. Percival's writings, of paralysis rapidly ensuing on the application of both the acetate and the sub-acetate of lead, combining with fermenting substances, in the form of lotions and of poultices; but I am not aware that such accidents have ever occurred when these salts have been used in solution, as in lotions, in combination with vinegar.

In Dr. Percival's essay it is stated that the workmen in the sugar-house at Manchester, when they were supplied with beer, prepared from malt and the refuse of sugar, fermented together in a large leaden cistern, were liable to severe and excruciating attacks of colic; but that when proper measures were taken to check the progress of the fermentation, the sugar-boilers were less subject to these violent attacks.

These facts, and the following occurrence, tended further to confirm my belief in the opinion which I had formed respecting the change of the acetate and sub-acetate of lead into the carbonate, where either colic or paralysis have followed the use of these salts. A gentleman, who resided in the island of Grenada, returned to England, labouring under paralysis of both the upper and lower extremities, the consequence of colica pictonum, (or dry belly-ache, as the disease is termed in the West Indies). He consulted me respecting his disease, and I happily succeeded in restoring to him the free use of his limbs. He returned shortly afterwards to Grenada; but was obliged, in less than eighteen months, to revisit England, on account of a renewal of the complaint. Consulting me a second time, he stated that he was inclined to attribute his complaint to Sangaree, made with rum, which in the process of distillation had passed through a leaden pipe, in the refrigeratory. One fact seemed to render this opinion doubtful. Many other persons had drank the same rum in Sangaree, yet had not suffered either from colic or paralysis. On making strict inquiry, however, into all the habits of my patient, I discovered that he had, on both occasions, taken soda-powders with his Sangaree, a short time before the disease commenced. These facts, and a remark of Dr. Latham, senior, in a paper on the use of the acetate of lead as a remedy in phthisis, published in the fifth volume of the Transactions of the College of Physicians, hinting that the increased quantity of acid contained in the acetate is the cause of its being innocuous, as he himself had ascertained, confirmed my opinion, and determined me, in using acetate of lead, always to give it in conjunction with a dilute solution of distilled vinegar. Since I adopted this plan of prescribing the acetate, although it has been taken in several instances to the extent of eight and ten grains three times a-day, without opium, for ten and twelve successive days, yet no colic, nor any other deleterious effects, have resulted from its employment.

These facts were sufficient to satisfy my own mind that my theory was tenable. Direct experiments on the deleterious influence of the carbonate, compared with that of the other salts of lead, were necessary to satisfy others of the profession and the public, and to induce them to adopt an opinion so opposite to that which has been, and still is, generally maintained.

The following experiments, therefore, were made with this view; and as the fact which I think they have established is one of great importance in the medical art, and in general science, I have been induced to make them publicly known through this institution*.

The comparative affinity of the two medicinal salts of lead, and of the citrate and the tartrate, in the state of solution for carbonic acid, was to be first ascertained.

Experiment I.—Half a fluid ounce of a clear aqueous solution of sub-acetate of lead, containing twenty-eight grains of the salt, was put into a wine-glass; and at the same time half a fluid ounce of a solution of the same quantity of the acetate into another glass, and both freely exposed to the air.

In a few hours, the solution of the sub-acetate became of a milky hue; and next day it was quite opaque, and con-

* This essay was read at the late meeting of the British Scientific Institution, at Oxford, in the chemical section.

tained a copious precipitate of carbonate of lead, whilst scarcely any change, except a slight film on the surface of the fluid, was perceptible in the solution of the acetate.

Experiment II.—Two fluid ounces of a solution of the sub-acetate of lead, containing one hundred and twelve grains of the salt, were put into a proper apparatus, and a stream of well-washed carbonic acid gas, procured from the decomposition of two drachms of white marble by muriatic acid, passed through it: the result was, twenty-three grains of carbonate of lead.

Experiment III.—Two fluid ounces of a solution of the acetate of lead, containing one hundred and twelve grains of the salt, were treated in the same manner, and with the same quantity of carbonic acid, as the solution of the sub-acetate: four grains only of the carbonate were procured.

Experiment IV.—A saturated aqueous solution of carbonic acid was poured into an aqueous solution of the citrate of lead: it became of a milky hue, and carbonate of lead was precipitated.

These experiments were repeated with fresh quantities of the different solutions, after a fluid drachm of distilled vinegar had been added to each. In none of them, except in that with the sub-acetate, was any carbonate of lead obtained; and in this, instead of twenty-three grains of the carbonate, six grains only were procured, when a stream of carbonic acid gas was passed through the solution.

These experiments demonstrate the great affinity of the sub-acetate and the citrate of lead, and the comparatively slight affinity of the acetate for carbonic acid. In my opinion, they explain also why the sub-acetate is more poisonous than the acetate, and lead to the conclusion that the citrate, which, probably, as well as the sub-acetate, is contained in sour wines, and pricked cider, corrected by means of litharge, is readily converted into the carbonate, and thus causes the Devonshire colic. It is unnecessary to say that carbonic acid effects no change in either the nitrate, the muriate, the tartrate, the sulphate, or the phosphate of lead.

My next object was to determine the effect of the two salts of lead which have affinity for carbonic acid, and of two of those which have none, as well as that of the carbonate, on the animal system. For this purpose the following experiments were made upon dogs:—

Experiment I.—One drachm of acetate of lead was introduced into the stomach of a strong cur dog; on the following day the animal remained in perfect health. Two drachms were then administered, and on the day after that three drachms, without any perceptible change on the health of the animal.

Four days then elapsed, and six drachms of the acetate were given at one dose, but without producing any deleterious effect. No vomiting supervened; the animal continued playful, eat his food as usual, and, except a slightly increased action of the bowels after the last dose, suffered no inconvenience.

Experiment II.—The same quantities of the sub-acetate as of the acetate in the former experiment, were administered in the same manner, and at the same intervals of time, to another dog of the same size and apparent strength as the former, with a similar result; no change in point of health being perceptible.

Experiment III.—To the first dog, a week after he had taken his last dose of the acetate, one drachm, two drachms, four drachms, and six drachms, of the nitrate were given; the three first doses on successive days, and the last dose after one intervening day. No perceptible effect was produced on the habit of the animal.

Experiment IV.—The muriate, in the same doses, and at similar intervals, was given to another strong dog, without producing any effect.

Experiment V.—To a strong dog of the poodle kind, one drachm, two drachms, and four drachms of the carbonate were administered on successive days, without any effect. Three days afterwards, one ounce was given in one dose; but, except slight vomiting, which did not occur until the following day, the health of the dog remained unimpaired.

Experiment VI.—Four days after the last dose of the acetate was given to the dog which had been the subject of the second experiment, one drachm of the nitrate of lead in solution was injected under the skin, on the inside of the right thigh: no effect was produced.

Experiment VII.—Two days after the last experiment, one drachm of the carbonate of lead, suspended in two fluid drachms of mucilage of starch,

were injected under the skin on the left thigh of the same dog. On the following day the limb became paralysed, and continued so for nearly a week; after which the dog recovered the complete command of the limb.

Experiment VIII.—Eight grains of the acetate, dissolved in a fluid drachm of distilled water, were carefully injected into the right jugular vein of a strong cur. The injection was scarcely finished ere the animal, making two or three deep inspirations, died without any signs of pain or convulsion. Great care was taken to exclude the introduction of air with the fluid. On opening the body, the heart was still pulsating, and continued to beat for the space of a minute. Nothing peculiar was observable either in the thorax or in the abdomen, except that the blood was fluid and of a bright rose-colour. Orfila had observed the same state of the blood after injecting thirteen grains of the acetate into the jugular vein of a small dog *.

These experiments appear to me to warrant the conclusion that dogs are bad subjects of experiment with the salts of lead. In Orfila's experiments on these animals, the doses were so extravagantly large that irritation was excited in the stomach, and vomiting produced, independent of the specific effect of the salt employed; a circumstance which would follow the administration of the same dose of common salt. To prevent also the ejection of the dose of the acetate, in some of the experiments a ligature was thrown round the œsophagus, so that the animal could not be regarded as altogether in a natural condition. Where death occurred, the appearances presented by the stomach were those of acute inflammation, resembling that caused by a corrosive poison; but neither colic nor paralysis were induced. As in my experiments, when the dose was moderate the alvine ejectments were accelerated. From the character of these discharges, I am disposed to attribute much of the safety of dogs in those cases to the decomposition of the salts of lead in the intestines by sulphuretted hydrogen gas, which is largely evolved in the intestines of the dog. This, however, is undoubtedly regulated by the nature of the food employed during the experiments. In the foregoing experiments the dogs were fed upon milk, bread, and a moderate proportion of animal matter. It is remarkable, that although these dogs resisted large doses of the carbonate of lead, yet instances are recorded in which hounds, fed out of leaden troughs, were so much influenced by the poison as to drop down and die whilst engaged in the chase *.

Having ascertained that dogs were useless for my experiments, rabbits were next selected as the subjects of a series of experiments.

Experiment I.—Six grains of nitrate of lead, dissolved in one fluid drachm of distilled water, were injected into the stomach of a healthy strong rabbit. The animal was much agitated by the operation, but an hour afterwards appeared quite well, and continued so until the next day, when nine grains, dissolved in the same quantity of water, were again injected into the stomach. No inconvenience followed even this augmented dose.

Experiment II.—Three days afterwards, six grains of the carbonate of lead, rubbed up with a fluid drachm of mucilage of starch, were injected into the stomach of the same rabbit. The animal seemed little incommoded for two hours. On the following morning he was found dead.

On opening the body, the only peculiarity that could be perceived was the turgid state of all the cavities of the heart. The blood was slightly coagulated in the right auricle, and was discovered to be fluid in the other three cavities; it was not of the bright rose-colour which followed the injection of the acetate into the jugular vein.

Experiment III.—Six grains of muriate of lead, rubbed up with a fluid drachm of mucilage of starch, were injected into the stomach of a strong young rabbit: no inconvenience resulted to the animal.

Experiment IV.—Three days afterwards, six grains of the carbonate of lead were injected into the stomach of the same rabbit. On the following morning the animal appeared dull and uncomfortable, but he survived until

* In one of the experiments of that distinguished toxicologist, five grains were followed by symptoms of general paralysis; which terminated fatally on the fifth day.—*Toxicologie Générale*, tom. i. p. 249.

* See Percival's Essays, vol. i.

the morning of the second day, and then died.

On the dissection of the body, the heart was found in the same state as in the rabbit which was destroyed in the second experiment. Two other rabbits were treated exactly in the same manner, with a fluid drachm of the solutions of the acetate and of the sub-acetate, each containing seven grains of the salts. The acetate produced no inconvenience whatsoever, but the administration of the sub-acetate was followed by symptoms of dejection and languor, with disinclination to move and to take food. The animal, however, was alive at the termination of a week after the administration of the salt, when it was at length killed by four grains of the carbonate.

From these experiments, I conceive that I am authorised to conclude;—first, that the carbonate of lead proves fatal to rabbits by operating slowly on the nervous energy, and by at length suddenly arresting the action of the heart; secondly, that the sub-acetate of lead appears to have some tendency to act on these animals in a similar manner; thirdly, that the nitrate and muriate of lead produce no effect on these animals.

If I may be permitted to draw an analogy between these effects, and those which are observed in man, from the medicinal use, or the accidental introduction of the salts of lead into his system, I should venture to infer, that, in moderate doses, they would act on the human species nearly in the same manner as on rabbits.

In these animals, we find that the sedative influence of the carbonate is sufficient to destroy life; also, that among the salts having an affinity for carbonic acid, the sub-acetate, which has the greatest, approaches nearest in its effects to the carbonate; and that those salts which have no affinity for carbonic acid, exert little or no influence on the animal economy, except as local irritants, when they are administered in very large doses.

On a review of the foregoing facts, I think we arrive at this conclusion—that there is only one direct poison among the salts of lead, which is the CARBONATE; and that, when the other salts of lead display poisonous effects, these are to be attributed either wholly, or in part, to their conversion into the CARBONATE.

This salt acts as a powerful sedative astringent on the living system; diminishing the nervous energy, and consequently greatly depressing the powers of the circulation, and lowering the tone of the muscular system. It is probably taken into the blood, which may account for its slow operation when it is introduced into the stomach in minute doses, for a considerable length of time, and also for its producing similar effects, when applied to the surface of the body denuded of the cuticle, or in a state of ulceration. That it operates, however, on the nervous system, both locally and generally, independent of absorption, is probable. Instances have occurred of painters having been affected with paralysis of the wrists without colic*; a circumstance which can only be attributed to the local influence of the carbonate on the cutaneous nerves: and instances are also recorded, in which colic and paralysis have followed the application of poultices, containing the sub-acetate of lead, to simple inflamed surfaces. In the last mentioned cases, there is no doubt that the carbonate is formed in this case by the decomposition of the sub-acetate during the fermentation of the farinaceous matter of the poultices. When the acetate of lead is taken into the stomach, it is probable that it is partly changed into the muriate; and that the portion which passes the pylorus undecomposed, is partially afterwards changed into the carbonate by the evolution of carbonic acid, which is always going on in the alimentary canal. The first influence of this change is to bring the digestive organs into such a state that a similar decomposition will be continued, and even accelerated; and thus the same effects may follow the long-continued use of the *acetate* of lead, as those which occur when the *carbonate* has been directly introduced into the stomach.

Let us now examine how the opinion which is endeavoured to be maintained in this essay, is capable of explaining cases of poisoning which have occurred by lead, in its various combinations, with oxygen and acids.

Instances have occurred of individuals having suffered from colic and paralysis, owing to the use of water contained in leaden cisterns at sea, or collected from

* Med. Gaz. vol. iii. p. 283.

the eaves of newly-erected houses, in the construction of the gutters on the roofs of which lead was employed. In such cases, and in all those in which water kept in leaden vessels has been the poisoning agent, the deleterious effect may be confidently referred to the carbonate; the experiments of the French chemists, confirmed and extended by those of Dr. Christison, of Edinburgh, having demonstrated that water acts upon metallic lead only when air is present, and that the result of this action is the production of carbonate of lead. It has also been ascertained, that the purer the water is, the larger is the proportion of the carbonate produced; and that the protecting power of the salts contained in water, such as it is when employed for the ordinary purposes of life, prevents the inhabitants of large towns, who usually employ leaden cisterns for preserving water, from suffering. The cases of paralysis which have frequently occurred to compositors, and other persons who work in metallic lead, and even those which happen to porters employed to load carts with pig and sheet lead, in manufactories of these articles, are all referrible to the carbonate formed by the action of the air, in a very short time, whenever metallic lead is exposed to air of a temperature exceeding 60° Fahr., however dry.

Wines, cider, and other fermented fluids, which have been sweetened by the addition of litharge, contain chiefly the citrate of lead, which is readily converted into the carbonate, when the wines or other liquors are drunk; unless so small a portion of the citrate has been formed, that the acid of the wines still predominates; in which case the acid acts as a protecting agent by preventing the formation of the carbonate. The same explanation is applicable to cases of colic and paralysis caused by the employment of glazed dishes with lead for preserving acidulated food. Now, in these cases, the subacetate, or the citrate, is the salt formed; as I have ascertained by keeping stewed apples, butter-milk, and sour milk, in such vessels: but, when vinegar is kept in such vessels, the acetate is formed by the acid corroding the glazing of the vessel, and no deleterious effect follows. In this manner I might review every instance of poisoning by preparations of lead, and demonstrate that they have, in each case, occurred only when the preparation employed has been of a nature to permit the formation of the carbonate, which, it will be found, is in every instance the poisoning agent. One exception only to this rule presents itself. When the dose of the acetate or the sub-acetate is very large, death may result from the acrimony of the salt, as in cases in which acrid poisons are swallowed; but in such instances the specific influence of the salts of lead is not perceived. It is unnecessary to occupy time by further details.

The conclusions endeavoured to be established by the foregoing experiments and reasonings, are of a description likely to lead to an investigation of other poisonous substances, in order to ascertain how far their deleterious properties depend on their direct influence on the animal system, or to changes taking place in the alimentary canal, and the consequent formation of new salts, which are, in fact, the poisonous agents. They also lead to highly beneficial results in the practice of medicine. Thus, by the usual method of prescribing acetate of lead in hæmorrhagies and other diseases, in which it operates by its sedative properties, the smallness of the doses employed requires the use of the remedy to be long continued, and consequently, although the acetate has been combined with opium, yet colic and paralysis have been occasionally produced. Now by giving the acetate in large doses, and protecting it from the action of carbonic acid, by the addition of distilled vinegar, the sedative influence of the remedy is augmented, and the effects anticipated from its employment are secured, without any injurious consequence to the general health. If the theory which I have advanced be correct,—that is, that there is only one direct poison among the salts of lead, and that the other salts become poisonous only when they are converted into that one, the carbonate, it will lead physicians to examine the medicinal properties of the nitrate and the muriate of lead, and to employ them in preference to the acetate and the sub-acetate, provided it be found that they possess sedative properties equal to those of the salts of lead now in use. This theory, if correct, also demonstrates that there is at least one excep-

tion to the hitherto prevailing opinion, that the virulence of salts possessing poisonous properties is in the direct ratio of their solubility.

REPLY TO DR. PHILIP,

By Marshall Hall, M.D. F.R.S. &c.

To the Editor of the London Medical Gazette.

Sir,

Dr. Philip complains of the tone of my notice of his paper: I beg your readers to believe that I had good and sufficient reasons for adopting that tone, and to be content, for the present, without further explanation.

Will Dr. Philip allow me to recal one event to *his* mind? I called upon him, and described the beautiful scene of the capillary circulation in the lung of the toad, *with the fact of the obvious impulse given to the blood in the arteries, capillaries, and veins, at each pulsation of the heart.* What was Dr. Philip's remark? One of admiration at the beautiful phenomena of nature? A wish to behold with his own eyes what he certainly never had beheld? No; but an abrupt, and not very courteous, or even civil, waiving of the subject.

But to the more immediate point before us. Dr. Philip states, "There is no occasion to put the Royal Society to the trouble Dr. Hall proposes. He will find twenty of its members who have seen the capillary part of the circulation; *but he stands alone, I believe, in imagining he can see in it the influence of the individual beats of the heart.*"

This is a most unfortunate paragraph; for though I cannot answer for the *twenty* members of the Royal Society who have seen the capillary part of the circulation, as Dr. Philip has seen it, I can for *one*, and for a second gentleman, who, though not a member of that learned body, is, singularly enough, a particular experimental friend of Dr. Philip himself.

The first of these is no less an authority than Dr. Thomson, of Edinburgh, the well-known author of an inestimable work on Inflammation. Dr. Thomson observes, "*the accelerations from the impulse of the heart can be traced in the veins;* and, indeed, in most of my experiments, THEY WERE MORE VISIBLE IN THE VEINS THAN IN THEIR CORRESPONDING ARTERIES* !"

The second gentleman to whom I have referred is no other than Dr. Hastings. This gentleman gives the following experiment:—

"A frog's foot was brought into the field of the microscope, and the circulation observed for some time. The blood was moving in an uninterrupted current through the numberless vessels in the web. A ligature was thrown round the limb. The motion of the blood became much slower; and a *sensible impulse was given to it in the three orders of vessels at each contraction of the ventricle.* In several other instances a similar result followed."

Dr. Hastings, after detailing other experiments, observes—

"The conclusions to be drawn from these experiments appear interesting. From the first (the one just quoted) we learn, contrary to what has been maintained by Bichat and some other physiologists, that *the action of the heart influences the circulation in the capillaries, and that it even extends to the veins;* for in what other way can we account for the sensible impulse given to the venous blood at each systole of the ventricle, but by supposing that the impulse given to the mass of blood was felt in these tubes?"†

"It *is* very provoking, that no sooner is a physiological point ascertained," (by Dr. Philip) "however carefully, than some, little accustomed to such inquiries," (such as Dr. Thomson and Dr. Hastings!) "are found ready again to perplex it." But, seriously, have these gentlemen so "given proof of their experimental accuracy," that Dr. Philip will feel himself "called upon to reply?" Are they, or is Dr. Philip, of the number of those "who choose to amuse themselves with physiological experiments?" so that "our journals (are) filled with discussions which can only disgrace the science!"

But this is not all. I write this with the very experiment before my eyes. The pulsatory motion communicated to the globules of the blood, synchronous

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* Lectures on Inflammation, p. 80.

† A Treatise on Inflammation of the Mucous Membrane of the Lungs, p. 49.

with that seen in the arteries, and therefore imparted by each systole of the cardiac ventricle, is as plain and easy to be seen as the Monument from London Bridge. It would have been well if Dr. Philip had adopted a similar precaution, before he sent in his antiquated notions to the Royal Society. And it would be well if that, and every society, adopted the plan of the Institute of France, of deputing certain of their body to witness and report upon experiments, before they publish such at least as are disputed at the very moment; otherwise, there must be the obvious risk of publishing the erroneous, and rejecting the true: "*perindè ut in hâc ipsâ re.*"

But besides the precaution of writing with the experiment before me, I have taken the additional precaution, however unnecessary, of shewing it to other eyes. The following gentlemen permit me to say that they have this day seen *the impulse of the heart upon the blood of the veins* as plainly as the very light of the meridian day: viz. Dr. Carswell, Dr. Hope, Dr. A. T. Thomson, Mr. Cox, Mr. Kiernan, Mr. Mollison, &c.

I think, sir, your readers will allow that I had good reason for saying that I thought Dr. Philip's memory must have failed him. I could not imagine that Dr. Philip, although he has not "even seen" my Treatise, had not read those of Dr. Thomson and Dr. Hastings. I had besides met with other instances of Dr. Philip's forgetfulness, for one of which I beg to refer to the first edition of his Treatise on Fevers, vol. iv. pp. 663, 671, 675; and to the last, or third edition, of his Experimental Inquiry, p. 241. In the former, Dr. Philip quotes the experiment of Dr. Whytt and of Fontana, of removing the brain and spinal marrow without arresting the beat of the heart. In the latter, he observes, "the author takes this opportunity of observing, that *after the greater part of the present edition was printed*, a friend informed him that in an old and valuable publication, which is now much less consulted than it deserves to be, *The Edinburgh Medical and Physical Essays*, an experiment is related by the celebrated Dr. Whytt, in which the brain and spinal marrow were slowly destroyed without affecting the action of the heart. This experiment not being mentioned in the works of Whytt, *has escaped attention!*"

There are strange blunders brought together here: first the experiment is fully and repeatedly quoted by Dr. Philip; then it is forgotten by him; then it is brought to his mind by a friend; lastly, it is said not to be mentioned in the works of Whytt; yet let the reader but turn to the well-known work on "Nervous Disorders," second edition, p. 7, note, and he will find a pretty full account of it!

I give you these as specimens of Dr. Philip's mode of proceeding. It was convenient to forget Whytt's experiment, when it was desirable to make such an experiment appear new and original. It was convenient to forget the experiments of Dr. Thomson and Dr. Hastings, when it was wished to make it appear that I "stand alone." If Dr. Philip will read the Treatise he has not even seen, and particularly pp. 87-89, 90, 92, 93, 95-98, 141-154, 163-169, he will find other things worthy of his notice; and although he may also find that he has claimed a good deal more than his share of the additions made to our stock of knowledge, the discovery may not be without its good effect. I will venture to say, that few persons have experimented so unnecessarily, or reasoned so inconclusively. Was it not unnecessary to repeat the experiments of Whytt and Fontana, except for verification? And is it sound reasoning to speak of applying stimulants to the capillary vessels, when, in fact, they are only applied to membranes between which those vessels pursue their course, and when it has not even been proved that such vessels, as distinguished from mere canals, really exist? I do not mean to insinuate that Dr. Philip has not made some addition to our knowledge, "however slender," but I do assert, that the subjects which he has treated still present ample scope for investigation, for those "who choose to amuse themselves with physiological experiments," and especially for those who wish to know the works of nature accurately. A more incorrect statement of phenomena than is contained in Dr. Philip's last paper, does not, in my opinion, exist in the records of any society.

I quite agree with Dr. Philip, that there is no occasion to put the Royal Society to the trouble proposed. I believe some of its members have already changed their opinions since this day last year; and I challenge Dr. Philip to bring a tithe of his twenty, who,

having seen the capillary part of the circulation, *with my experiment before them*, a circumstance most easily arranged, will say that the pulse of the ventricle does not extend to the capillaries, and through them to the veins. Such an experiment is, indeed, the A B C of the investigation; and I repeat, that I challenge Dr. Philip to find two, nay one F.R.S., who, having inspected it, shall remain of his opinion.

Dr. Philip observes, "it is easy to err in reasoning on physiological subjects, and repeating physiological experiments." In this sentiment I fully concur. For example, in the Treatise on Febrile Diseases, ed. i. vol. iv. p. 694, we read, "I may observe that although irritating the brain mechanically produces violent and universal convulsions in the muscles of voluntary motion, both in frogs and rabbits, yet I have found that in neither the one nor the other it affects the motions of the heart*"; whereas, in the Treatise on the Vital Functions, ed. iii. p. 96, we are informed that "it was repeatedly found, both in newly-dead rabbits and frogs, that, after all stimulants had ceased to produce any excitement in the muscles of voluntary motion, both chemical and mechanical stimulants so applied still increased the action of the heart." This is the very *ne plus ultra* of self-contradiction.

It must be admitted that I have two advantages in this discussion over Dr. Philip: in the first place, I repeat my experiments, and that before competent witnesses; in the second place, I have seen Dr. Philip's Treatises, and I think I have proved that I have read them too.

Dr. Philip affects to treat me with indifference. "I can only say," to use Dr. Philip's own words, "that I believe a farther acquaintance with the subject will induce him to change his opinion."

One word more: Dr. Philip says he has never even seen my treatise. This may be so; but did Dr. Philip never see, or hear of, a certain paper read before a certain learned Society? &c. &c. Can it be supposed that Dr. Philip actually *sat in judgment* upon a paper,—call it a Treatise, or by any other name,—which he had "never even seen"? And if not, what does the assertion mean? On the other hand, if Dr. Philip had really never seen my paper or treatise, how could he judge whether its author had, or had not, "made some addition, however slender, to our stock of knowledge?"

In conclusion I beg to repeat, that the tone and manner of my notice, and of this reply, have not been unprovoked; they are, and I intended them to be, firm. Nothing more is requisite when truth is contending against error. They were not intended to offend, except inasmuch as plain truth offends some persons. I would not willingly give offence to any living soul; far less would I have offended one for whom I had long entertained the highest esteem. But if I am slow to give, I am quick in taking, offence, and tenacious of it too, *until the matter is explained*. Having accomplished this point, as far as I think it right to go in this place, I bid adieu to the subject, and, assuring Dr. Philip that although I do not forget, yet I do perfectly forgive, his conduct towards me, with all the injury that it *might* have done me, and which Dr. Philip himself can appreciate better than any one I know,

I have the honour to be, sir,
Your obedient servant,
MARSHALL HALL.

14, Manchester Square,
Aug. 25, 1832.

* "Fontana makes the same observation respecting frogs."

ON THE DISEASES LATELY PREVALENT AT HELIGOLAND.

(Being a Report transmitted to the Army Medical Board.)

AFTER it had been publicly announced that the epidemic cholera had completely disappeared at Hamburgh, and some of the other neighbouring places on the Continent, I was in great hopes this malignant disease would not extend its ravages to this island.

But the Board will have already perceived, from a communication which I had the honour to forward to the Director General on the 28th June, that the pestilence has at length (notwithstanding all the sanitary measures which have been adopted) unfortunately made its appearance amongst us, and that, from the 11th to the 25th June, no less than *eight* well-marked cases of malignant spasmodic cholera have, from some unknown cause, broken

out here; of whom two recovered and six terminated fatally, in a few hours after the commencement of their respective attacks. The first case, to which I was called on the 11th June, was a sailmaker's wife, 45 years of age, of rather delicate constitution, but temperate habits, and residing in the Lower Town. Whether this woman received the germs of the disease from her husband, or from some other endemic cause, I am unable to say; but the former supposition is more likely to be correct, as will immediately be shewn; and as I conceive the case to be one of some practical interest, I feel myself called upon to enter the more into some detail with respect to the particular symptoms and treatment which I employed.

It appears that this woman's husband had only returned from Hamburgh three weeks previously, where he had been to purchase "canvas." He had no ailment whatever until fourteen days after his return, when, being a good deal exposed at sea, and having eaten some unwholesome salt meat, and afterwards drank large quantities of cold water, he was, in the course of the following day, attacked with diarrhœa; but from which he speedily recovered, without medicine. His wife was laid up, in three days after, with the same complaint, but to a far more severe and obstinate degree; accompanied with much nausea, bilious vomiting, and urgent thirst. Her dejections, together with the contents of the stomach, assumed latterly the same rice-water appearance, intermingled with mucous flakes, so peculiar in malignant cases of cholera; while to these distressing symptoms succeeded severe spasms in both legs and thighs, returning at regular intervals of about ten minutes. From the frequency of evacuations by stool and vomiting, this poor woman became at length so much weakened, that, before I had been informed of her critical situation, she had sunk into the last stage of collapse, hurried on, in all probability, by her own imprudence, from having previously immersed her face and hands for some length of time in cold water. On my arrival, at 2 P.M., I found my patient stretched on her back in bed, with both arms fully extended, and seemed *primâ facie* as if she had been rapidly sinking from excessive loss of blood. No pulse at either wrist, or beating over the situation of the heart, perceptible. The extremities were quite cold; skin disagreeably clammy and moist, blue, and much corrugated, particularly about the hands; the abdomen much flattened, and which, as well as the chest, were the only parts found to possess any warmth. The respirations were particularly slow, irregular, and oppressed; countenance much collapsed, and of a cadaverous aspect; eyes sluggish, and sunk in their orbits; lips livid; tongue clean and moist, but, with the breath, unusually cold. Voice so feeble and hollow that she could only express her ailments by signs. Her mental faculties remained, during this mortal struggle, unimpaired.

I was now strongly impressed with the belief that this could be no other than a case of "cholera" of the very worst description, and that no time ought to be lost (however hopeless her state might appear) in using prompt means to rally the system.

Treatment.—The remedies which I prescribed during this stage of the disease, were chiefly composed of opiates, to check the vomiting and discharge from the bowels, spirit camphoræ, ætheris sulphurici, and ammonia, in pretty large doses, combined with mucilaginous substances, warm brandy and water, &c.; and, for external application, the warm-bath, and afterwards hot bottles to the extremities and along the course of the spine; hot-water cloths to the region of the stomach, turpentine embrocations, dry friction, with flannel and additional warm clothing.

The vomiting and purging did not again return after taking the first dose of the medicine, while the spasms were materially relieved, and in a short time afterwards went completely away. But for the comparative relief which my patient now declared she had experienced, more particularly in her breathing, I was disposed (in absence still of all signs of returning circulation and warmth) to view the sudden cessation of these hitherto urgent complaints with some degree of suspicion. I continued the stimulating plan; but finding at my next visit, about ten o'clock that evening, no evident change for the better, and the breathing still much oppressed, I now had recourse to venesection, as recommended particularly by Mr. Annesley, in his work on Tropical Diseases, "from the supposition of the blood

being in a highly degenerated state." Of the correctness of this opinion, and the great benefit derived from this practice, I soon had the most unequivocal proof; for on opening a vein in the right arm, I could only procure a few drops of very thick blood, of a dark colour. I was, however, more successful, and abstracted three ounces of blood from the opposite arm, which was precisely of the same unfavourable appearance as before mentioned; and on standing for some length of time, was observed to be entirely composed of crassamentum. I persevered with the former remedies, and applied a large blister to the epigastrium; when, in about half an hour more, contrary to all my expectations, I had now the pleasure to discover signs of reaction, and returning circulation at both arms; the pulse was hardly perceptible, but gradually acquired more strength; and from this moment the vital and animal functions began to recover, and the secretions of urine and bile (which had been totally suppressed from the commencement of the premonitory symptoms) became in a short time more and more copious.

The patient now, however, in place of deriving relief from so favourable a crisis, appeared only the more alive to all her mortal sufferings. She was now observed to toss about in bed, and to complain particularly of a burning sensation and oppression within the chest, and at the pit of the stomach, with urgent thirst. I now left off the spirituous stimulants and brandy, and substituted the demulcents, saline effervescing draughts, alterative and tonic bitter medicine, from which she experienced instantaneous relief, and in a few days more ultimately recovered.

Some days having now elapsed without meeting with any more cases of a similar description, I was in great hopes that the disease had entirely left us, but I regret to have to make a far different report, for the pestilence reappeared in a more virulent form on the 16th June, when a poor woman, residing in a filthy quarter in the "upper" town, was next attacked with the disease. The symptoms in this case were so violent and sudden, that before her friends could give me timely notice, she sunk into the cold stage, from which she never recovered; and although all the usual means were employed, the case proved fatal in nine hours. After dissolution, the flexor tendons of both arms were observed to be contracted to such a degree as scarcely to admit of the fingers being extended with all my force.

I was prevented from making a postmortem inspection by interment having taken place in nine hours after death, when rapid putrefaction took place. The house was afterwards well ventilated with fresh air and the chloride of lime, and the bedding, &c. ordered to be immediately washed. Notwithstanding, however, all these measures of prevention, this woman's daughter, a healthy girl, 15 years of age, residing in the same house, was in six days afterwards attacked with cholera, which was attended with all the usual symptoms of malignancy, with the exception of spasms.

Having seen the case at an early period, she recovered under depletion, calomel and opium, blisters, and a few doses of alterative medicine. The same girl's grandfather, who had been living at some distance from the upper town, but had been occasionally in attendance on both cases, also fell a victim to the disease on the 24th; and from his constitution being previously much impaired from intemperance, besides pretty far advanced in years, he died in the course of the following day, and was interred the same evening; but previous to which I had a postmortem inspection, when I found the following appearances.

Sectio Cadaveris.—Extensive venous congestion of the abdominal viscera; the mesenteric veins were particularly enlarged; the stomach and bowels contained a great quantity of white mucous fluid, and their internal surface was of an unusual dark red colour, which appeared to me to have arisen more from the effects of congestion than from any primary active inflammation in the parts; the mucous membrane, particularly of the stomach, abraded and softened in various places, and the large intestines were much contracted; the gall-bladder fully distended with dark thick bile; the ducts were pervious; the urinary bladder contracted to a very high degree, and when cut into, it was found to contain only a little white mucus; the structure of the liver, spleen, and lungs, quite healthy; the brain not examined, for want of more opportunity and the necessary instruments.

On the following day, no less than four new cases broke out, two in the same house—a man and his wife, a woman who had been in attendance, living in the adjoining house, and a child, upwards of twelve months old, at a more distant quarter of the upper town. The two women and the child were in the cold stage from the moment I saw them, and the man at the same time pretty far gone: all proved fatal within twenty-four hours, in spite of the usual remedies.

However I may regret the loss of so great a proportion of cases, I must say that I had not the most distant expectation, from first to last, that any of them would recover, under all the disadvantages of their situation; and, moreover, the majority were very elderly people, totally destitute of all means of support, and living in very unwholesome dwellings, with scarcely any covering to shelter them from the cold.

In the present unsettled state of our knowledge with regard to the proximate cause of epidemic cholera, it would be great presumption on my part to offer any observations beyond those which have resulted from my own as yet limited experience of the disease here. It will appear from the foregoing remarks, that no conclusive information has yet been procured to warrant the belief of the disease having been "imported." I can therefore only attribute the development of malignant cholera on this island to some other obscure epidemical condition of the atmosphere, capable of exciting the specific action more or less in constitutions predisposed for imbibing the disorder.

The weather at Heligoland for the last few months has been unusually mild and dry; and the prevailing winds, during May and June, have blown from the north-west; very cloudy, with thunder and lightning, but little or no rain.

I may likewise take notice of the great prevalence of other epidemic and endemic diseases immediately preceding and in conjunction with cholera—I mean more particularly hooping-cough and ague, upon which I took the liberty of offering a few remarks in my previous quarterly report. The former disease disappeared some time ago, with only a trifling mortality; but the ague, particularly the quotidian and tertian forms, began to rage immediately afterwards, with all the characters of an epidemic, and as never had been witnessed on any former occasion here, attacking principally the poorer families—the young as well as the old of both sexes. I have on several occasions had an opportunity of observing a *contagious influence* under its more malignant type. Subsequently to the appearance of cholera, the proportion of intermittent cases has been diminishing, and those at present under treatment I find to be less obstinate of cure, and that relapses recur more seldom.

I was unable to account for so great a proportion of intermittent disease, otherwise than from the great scarcity of fresh water on the island for some time back; but since the cholera has broken out, it is very likely that the same primary remote cause which gave rise to the one, may also to a certain degree have produced the other, under all the circumstances and modifications which usually attend other epidemics—a supposition which I believe to be strictly in accordance with the observations of most authors.

Since I have commenced this report, I regret to have to add two more cases of malignant cholera, which broke out on the morning of the 4th July. The first was a healthy boy, about six years of age; and a married woman. The principal symptoms in both cases were well marked. Having seen the boy at an early period, he was immediately bled, and calomel and opium exhibited, with blisters to the epigastrium; under which treatment he completely recovered in the course of two days. The woman's pulse was too far gone to admit of any benefit from venesection. I therefore placed more dependence on calomel and opium, camphorated spirits, æther, demulcents, tonics, and alterative medicine; and this woman likewise gradually got round, and, I am happy to say, is now out of danger.

The following list will shew the total number of cases of malignant cholera on the island from the commencement, viz. 11th June to the 4th July, the date on which the last cases broke out.

Admitted.	Cured.	Died.	Remaining.
10	4	6	0

I can speak very favourably of the good effects which I have observed to follow the exhibition of the decoction of bark in cases of extreme debility and excessive vomiting. After all other

remedies had failed, the formula which I used was equal parts of the decoction of bark, and emulsio amygdali communis, alone, or combined with camphor, as circumstances might happen. After a few table-spoonfuls, the vomiting, together with the sensation of burning and urgent thirst (so particularly distressing at times in this disease), was materially relieved. I conceive its *modus operandi* in cholera, to be in some respects the same as when given to counteract the effects of any acrimonious poison, particularly the *emetic tartar*, when swallowed in large doses. Besides, it will give additional tone to the stomach, whereby this organ will afterwards be enabled to resist all farther irritation, and which, if not checked early, might have led to a fatal termination. I have had recourse to this medicine in all the cases which have recovered; and, under judicious management, I have no doubt it will be found to produce the most happy effects.

It may not be unworthy of remark, that in opening the body of the man whose case I have already described, I happened to puncture my finger to some considerable depth, but from which I afterwards experienced no particular inconvenience.

These are all the observations which I have at present to offer for the information of the Board. On the first appearance of the disease here, steps were immediately taken by the magistrates and others in authority, for purifying the town, and removing all species of filth, &c.; and I am in great hopes that by a more vigilant observance in this respect, no more cases of cholera will break out on the island.

Should the Board be acquainted with any particular remedy for the cure of "cholera," I should feel particularly obliged by receiving the earliest intimation thereof. Marks of haste will probably be perceived in various parts of this report, for which I beg leave to solicit the Board's indulgence.

DUNCAN MEWINS,
Assistant Surgeon to the Forces.

OBSERVATIONS ON PHLEBITIS.

To the Editor of the London Medical Gazette.

SIR,

MANY cases of phlebitis, of an idiopathic character, have recently come under my observation, which, though they do not lay claim to originality, may do so to interest, and I trust you will give them a place in your valuable journal, if you should deem them worthy of notice.—I am, sir,

Your obedient servant,
G. C.*

Middlesex-Hospital,
July 28, 1832.

Writers on this subject have unquestionably enlightened us much on the pathology of several diseases incident to the human frame, which before were cursorily looked at, and imperfectly understood. Thus, among many instances, we may mention the facts which medical men have received and acted on from the valuable researches of Dr. Lee, Messrs. Hodgson, Arnott, Breschet, and Bouillaud.

Haller was not ignorant of this disease; he recites several cases in which obstruction of the veins in various parts was observed succeeding to inflammation, but he does not throw much light on the pathology of the affection.

In one woman, the inferior cava was obliterated from the iliacs to the renal veins, and the circulation carried on by the right spermatic greatly dilated.

Laennec conceives that the inner membrane of veins is seldom attacked with inflammation; and when obliteration of these vessels does occur, he supposes the process to be begun and completed immediately before death, and that the irregular and imperfect circulation now favours this disposition to coagulation.

It is somewhat surprising he should mention this after having clearly demonstrated the perfect organization of the thickened walls of aneurismal tumors, and yet he doubts that the veins have the same vitality and disposition to assimilate this foreign matter to their own immediate structure; nevertheless, he mentions cases in which he has witnessed the perfect obstruction removed by appropriate remedies.

The inner membrane of veins appears endowed with the same vital properties as serous membranes, following in some measure the same laws under disease; they secrete sero-purulent matter, which, lining the sides of the vessel, and the activity of the inflammation

* The name of the author has been sent to the Editor, with a view of authenticating the facts detailed.—E. G.

being subdued, the serum becomes absorbed, as the natural result of incipient organization, while the pus forms the medium of adhesion with the opposed surfaces of its vessels; its calibre now becomes gradually contracted to half its natural size, eventually giving rise to the derangement of the whole circulation posterior to the obstructed part, and its painful and often fatal results.

There are other forms of dropsy which owe their origin to obstruction and irregularity of action in another part of the circulatory apparatus, induced by a contracted state of one of the openings on the left side of the heart, and thickened membrane of the aorta. Dilatation, and thinness of the walls of the cavities, may exist, and thus give rise to a perpetual regurgitation of fluid, thrown from the ventricle into the aorta. These, and many other causes, establish the worst forms of dropsy that come under the notice of the practitioner.

May not the condition of the capillary system be considered the reverse in the two instances—namely, from venous obstruction and organic disease? In the former, the œdema is the result of turgidity and unequal circulation; fluid is effused into the surrounding cellular tissue, as a natural relief to the over-distended state of the vessels; while in the latter instance, the vis-à-tergo is so reduced, the action of the heart so laboured and oppressed, that they pour out the serous portion of their contents, as being the fluid most ready to escape from them.

If the medium of connexion between the opposite sides of a vein be fully examined, it will be found to present the following appearances, especially if the process of organization is completed:—The external layers are firmly adherent to the inner membrane of the vessel, and are very similar to the inflammatory buff formed on the surface of blood, but they possess more evident firmness when separated. As they are torn from the inner tunic of the vein, numerous red points may sometimes be seen, the remnants of vessels passing from one to the other. The internal layers approach to a yellow colour, are somewhat friable in consistence, and partake of the character of fibrine, though more flabby in nature, and not unlike that found in aneurismal tumors. In such cases the internal layer is so thickened, that on making a transverse section of it, the vessel remains circular, like an artery.

Though these appearances are common, in connexion with such diseases as inflammatory dropsy, simple œdema, phlegmasia dolens, &c., yet in idiopathic phlebitis, or puerperal fever, it is not unusual (if the patient be carried off during the acute stage) to find pus in several veins, or in several parts of one vein, with an injected state of the inner membrane, which assumes a velvet appearance, or the traces of inflammation will be seen, from the mere injection of vessels to the exudation of false membranes.

Perhaps the most prominent and singular feature of this disease is the disposition of the vessels to deposit purulent matter in joints and cavities, as the pleura, pericardium, &c., and in parts not circumscribed, as the cellular tissue between the larger muscles. There is great obscurity about the disease at its origin; and not until fatal consequences have ensued, does the practitioner discover the nature of the attack. I shall not here advert to the opinions of those writers who regard the fatality of the disease as having its origin in the absorption of pus into the general circulation; neither do I deem myself competent to offer any remarks on the doctrine of absorption by veins.

The commencement of inflammation of a vein is ushered in with more or less febrile excitement, which subsiding, a slight pain is first observed in the course of the affected vessel. The vein dilates, and makes on the limb distinct elevations, of a slightly blue colour, which gradually become a pale red. The part is now very painful; and when the finger is placed on the vessel, the patient involuntarily draws the limb away; any motion of it produces great pain, and he usually keeps it in a state of semiflexion; the parts between the veins become soft and bluish; the heat is greatly increased; pulse small, quick, and intermittent; the abdomen generally tense; tongue red, chapped, and dry; pain in the head, with or without delirium; perspiration, diarrhœa, and death.

I trust the following cases may be found interesting. I have recited the fatal ones, to afford an opportunity of also giving the post-mortem appearances, and of throwing some few facts of pathology together.

Case I.—Harriet Davies, æt. 20, admitted into St. Bartholomew's Hospital July 21st, 1831. Anxious countenance; respiration hurried; skin chilly; tongue clean, dry, and chapped in the centre; pulse 120, and feeble; complains only of pain in the left thigh, with inability to straighten it, lying with her knee bent; no pain on pressure behind the trochanter, or in front of the joint, or in the loins; gets no sleep, owing to pain in the thigh; bowels purged.

The history of her complaint was, that she had been ill three weeks, being first seized with rigors, succeeded by heats and pain in the head and side, for which she was bled with relief. The pain then arose in the thigh; from that period she has kept her bed, and been unable to move her limb from its present position. She took the compound ipecacuanha powder, under the idea that it was merely rheumatic; but in the course of the 22d, the leg became quite straight, though pain remained in the thigh and knee. She was remarkably low; her pulse 140, and feeble; and she had constant vomiting. On examining the limb, a dusky blush, with œdema, was observed covering the instep and ankle of the left foot. Purging continues; which is to be checked by Tr. Opii, ♏v. every two hours. Vin. Rubr. ℔j. daily. The opium procured her rest for one night, though the purging remained unchecked. Her abdomen is, nevertheless, patient of pressure, and her countenance improved. State of the limb the same, and motionless; but on the 27th there came on wild delirium, only quieted by opium. Foot became livid, and pain in the knee, with tension of the capsule, arose. She rapidly sunk, and died on the 29th.

Autopsy.—No effusion in the head, and every part of it healthy; firm adhesions in the pleuræ; a portion of middle lobe of the right lung was in a state of pulmonary apoplexy, in which there was a cyst, of the size of a pea, containing pus. This cyst was found constituted by a dilatation of a pulmonary vein, just as it branches off to smaller ramifications: in the vein on the side nearest the heart there was a firm plug, which appeared like coagulated blood. In the left lung were several other cysts, smaller, and containing pus, but not obviously formed by the dilatation of veins, though many were seen running contiguous, having their areas filled with similar plugs. Heart was natural.

In the abdomen, the peritoneal and mucous coats of the intestines were injected; the left kidney was much paler than the right, and had in its pelvis an isolated cyst, containing pus. The abdominal veins were filled with mere coagulated blood. The femoral and profunda on the left side were plugged with coagula, which could be easily drawn out. About four inches of the brachial artery was so contracted as only to admit a very small probe. The venæ comitantes were likewise plugged up, and the plugs were easily separable into laminæ, and were not firmly adherent to the inner membrane of the vessels. Around the shoulder-joint, and diffused through the cellular tissue, was a very large collection of healthy pus. Similar collections were found beneath the psoæ and iliacæ muscles, but they had not penetrated the joint, nor had they any communication with the bones. The pus descended beneath the capsular ligament of the hip-joint, and it was doubtful whether it communicated with a large collection of pus in the joint on the left side. In the acetabulum, the synovial membrane was destroyed, the cartilage exposed, and felt as if eroded. Knee-joint contained a quantity of pus, but no inflammation of the synovial membrane could be traced. A bright yellow serum exuded from the cavity of this ankle-joint. Slight erosion along the cartilage of the tibia on its upper part.

There was another case, which came under observation at the same period, of simple œdema of the leg, but of so peculiar a character that it may be said to form a prominent feature of the affection. On pressing the limb in such a case no pitting readily takes place, as in anasarca: it gives a sensation of elasticity when grasped, is soft, and of a pearly whiteness. Is this effusion into the cellular texture, or a mere distention of the parts consequent on the above obstruction, without extravasation?

Case II.—James Dunford, æt. 23, baker, admitted December 8th, 1831, under the care of Dr. Watson. Countenance heavy; heat and chills frequently alternating; has some pain in the head, and general malaise. Great prostration of muscular power. Bowels open; abdomen is soft: tongue covered with a dirty brown fur, moist; pulse 100, with some power; skin hot; thirst.

He could only account for this attack by being a little wet four days ago, which was followed by rigors, and heats and pain in the head. Conceiving it to be a mild attack of fever only, Dr. W. put him on salines, with calomel and James's powder. He was observed to be very dull and heavy in manner; on the 10th, a large, elevated, circumscribed blotch, extending from the outside of the fore arm along two-thirds of the right arm, was observed, but which he did not appear to know. This remained, though much relieved by poppy fomentations, till the 12th, when a severe rigor set in, lasting for a quarter of an hour; and the following day a dusky-red line was seen running from the front of the elbow-joint to the shoulder,

taking the course of the basilic vein, and here terminating abruptly. It was neither hard or elevated, gave no pain, and he only complained of pain in the middle of the arm and in front of the shoulder-joint, which was greatly augmented on the slightest motion of the limb; he could move the wrist and fingers, but was unable to raise the arm. Was observed to be remarkably drowsy. Tongue now became creamy. Leeches were applied with marked relief to his sufferings, and he was purged with calomel and the senna draught. He was going on favourably till the 25th, when he had another severe rigor, the arms having now recovered some motion. This attack was succeeded by severe pain over the whole abdomen, but more particularly in the pubic region, increased by coughing. Urine passed freely, and the evacuations were more healthy. This was relieved by another application of leeches, and Hyd. Submur. and Opium at night, with small quantities of wine, the pulse having become extremely feeble. The 28th brought several rigors, lasting from five to twenty minutes, and towards the evening he perspired profusely, with relief; his countenance was improved, and he had some motion of the arms. The appearances on the fore-arm were now soon dissipated, but on the 30th a copious green fluid was vomited, and the epigastrium became very painful. He was somewhat relieved by a large blister to the abdomen, and enemata of warm water. Great inability to move the left arm now came on. He frequently had a hectic flush, and was constantly perspiring. His abdomen having become supple, and free from pain, he had a generous diet, with porter and wine, and the acetate of morphia at night, with Ext. Col. C. and Hyoscyam. every morning. The face became more haggard; tongue brown and dry; and sloughing of the back and nates ensued, and he gradually sunk, much emaciated, on the 16th of January.

Autopsy, 24 hours.—An abscess was found between the integuments of the abdomen, over the bladder and the peritoneum, but having no connexion with the cavity. The muscle was dark and sphacelated in appearance, and the pus had a greenish aspect. Beneath the right pectoral muscle was a large abscess, extending into the axilla. The same shoulder-joint greatly diseased, the cartilage of the humerus being nearly absorbed, and the bone becoming carious. The bronchial vein was blocked up completely by a firm clot of a palish colour. This occurred twice in its course, and each coagulum was directly above a valve, adhering most firmly to the walls of the vein. The axillary glands had undergone considerable alteration, being soft and pulpy, while some few had pus in their substances. No morbid appearance seen in the lungs, and the heart was not enlarged in size; but on one of the folds of the tricuspid valve there was a thick, white, circumscribed, cheesy-looking deposit. Similar depositions were also observed on the mitral valve, but they appeared more like the venereal vegetation of Corvisart, or the "vegetation verruqueuse" of Laennec. On the sigmoid valves they had the appearance of small granulations of ulcers; from their projection they rendered the valves rough to the finger. The left wrist was surrounded by pus, diffused among the tendons, but the joint was quite healthy. This shoulder-joint was not examined. The vein was laid open, and the inner tunic was highly vascular, with a deposition here and there of purulent matter. It should have been observed that no inflamed vein could be traced into the abscess of the recti muscles, nor was there any unusual vascularity of the inner membrane of any of the veins between these and the right auricle.

Another case, which I may briefly recite, also came under Dr. Watson's care about the same time, in which the symptoms were inveterate diarrhœa, which succeeded to a distinct inflammation of the saphæna vein, with tenderness and hardness along its course, which passed to the groin and brim of pelvis—œdema of the limb, and inability to move it freely. The purging could only be checked by half-grain doses of morphia, after all other astringents had failed, but the disturbance it produced in the nervous system was so alarming that it was unavoidably discontinued, and she soon fell a victim to the disease.

The chest, on examination, was found diseased; a pleuritic effusion having been recently established, and many tubercles (with which the lungs were loaded) were becoming soft, destroying the adjacent pulmonary tissue.

In the abdomen, the liver and kidneys were perfectly exsanguineous. The only morbid appearance in the alimentary canal was the condition of the mucous coat of the colon, which was studded with black patches, as if stained by ink or charcoal. The left femoral vein, from the knee to its entrance into the common iliac, was quite obliterated, and was like a cord, plugged up by a firm coagulum adherent to its side. The saphæna was similarly blocked up.

In another case, of a woman who came in with a fatty tumor of the breast, inflammation was set up in the calf of the leg, running distinctly in the course of the saphæna vein. Although the alarming symptoms which at first appeared were quite relieved by leeches, fomentations, and aperients, yet she sunk under it the third day from the attack; and on examination, the

saphæna and femoral veins were found to contain pus in many parts. It is, however, gratifying to find the same practice which may occasionally fail succeed in other cases, as the two following will shew, under the care of Dr. Watson.

Case III.—Mary Nicholson, æt. 30, unmarried, admitted January 6th, 1832. Pain in the right leg, especially in the calf; it is easier when warm; pain shooting through the pelvis and loins. On passing the finger along the course of the saphæna and femoral sheath, much pain is produced; there is no distinct hardness here; tongue is pale and moist; pulse 96. She attributed this attack to a cold caught three weeks ago, on which arose pain in the leg, passing up to the pelvis, with a leucorrhœal discharge. The former was nearly eradicated by the acetate of morphia at night, purgatives, and the hot air bath; but on the 10th she had profuse perspiration come on, which dispersed all her pains. Quinine and meat were allowed her; but on the 13th the diaphoresis ceased, and all her pains returned, increased in degree, but more especially in the left calf and thigh, taking also the course of the large veins. This was but slightly relieved by leeches to the part; the leg became swelled, pale, shining, and did not pit on pressure; she, however, obtained marked benefit from leeches to the groin, and a renewal of the hot air bath. On the 10th of February the pain in the groin and course of the femoral sheath only returned at intervals, though it was still very painful on pressure, the œdema diminished, and her general health improved; but she was suddenly seized on the 20th with acute pain in the region of the left kidney, running down the thigh in the direction of the sheath of the vessels. It was much relieved by a sinapism, but more so after she had evacuated a large quantity of urine, loaded with a thick white sediment, like pus. She now rapidly got well and left the hospital, having no pain or tenderness in the course of the femoral sheath, though a cord-like substance was to be felt in its course.

Case IV.—Lucy Hemerton, æt. 29, married, admitted July 31. Surface rather blanched; some tenderness of the abdomen; pain in the groins, particularly the left; swelling of this leg in its whole extent. Great pain is evinced on making the slightest pressure down the saphæna vein of the leg, and she traces the course of the vessel most accurately as the seat of her sufferings; on grasping the limb no pitting ensues; she feels easier when warm, but is unable to put the sartorious muscle into action without screaming, from lancinating pains up the thigh. The right leg is not swelled, but some pain exists on pressure over the femoral sheath, with hardness through its whole course; urine scanty, and loaded with a pink sediment; pulse 100; bowels bound. She states that she has been married eight years, and only became pregnant three years since, when she was confined of a dead child in the eighth month. Three weeks ago she was again brought to bed with a healthy child at the full period; her labour was easy, and she was going on favourably, when she was attacked on the tenth day with pain over the abdomen, legs, and ankles, for which she was bled with relief; but the following day the legs swelled, and have continued to increase slowly ever since, with much pain in the groins. The treatment consisted in evacuating the bowels with ol. ricini, and applying leeches to the groins and course of the femoral sheath. This completely relieved all her pains after two applications; and she had the acetate of potassa, ʒss. ter die, which increased her quantity of urine, and removed the excess of acid deposit in it. On the 5th of August she was free from all uneasiness, and only experienced pain when the finger was firmly passed down either sheath; abdomen supple, and general health much ameliorated. Was now directed to rub in ʒss. of strong mercurial ointment down the course of the vein. On the 8th the œdema had quite disappeared; a faint cord-like substance was felt here and there in the leg and thigh; her gums were slightly sore. She now has a generous diet, and is rapidly approaching to convalescence.

CIRCULATION OF THE BLOOD.

To the Editor of the London Medical Gazette.

Hackney, Aug. 20, 1832.

Sir,

It is remarkable that Dr. Philip, while treating so extensive a subject as the resources of the circulatory powers, should omit so obvious an one as that which I am about to refer to, and which cannot be overlooked, whatever other sources of power there may be. I was happy to find that Dr. Hall had disproved, by experiment, the conclusion at which Dr. Philip had arrived, as it would have proved fatal to the ideas which I now subjoin. But before I had seen Dr. Hall's reply to Dr. Philip, I had made the following note on the experiment, intending to repeat it in another form:—"The ligature on the jugular must bring the sides of the vein in contact, which of themselves they could never have done; and thus the tightening the ligature by compressing the sides must push on the blood. Would the blood be propelled if a divi-

sion were made by a *plane* transversely across the calibre of the vein instead of the ligature, thus leaving the canal patent? The nearest approach to such an experiment would be to use *two* ligatures—one as he did, and one nearer the heart; tighten the last *first*, and in a minute or two tighten the distal one; by which a good deal of blood would be retained between them. Now loosen the upper (nearer) ligature; will *that* portion of the blood be sent on? But this even would be very imperfect; nothing like the division of the calibre by a plane without producing collapse."

In the last week's Gazette, also, Dr. Badham notices another probable source—"the *vita propria* of the blood;" which, as he himself says, certainly is not "an intuitive absurdity." But, apart from all these, there is one cause of progression which neither has noticed, and which appears to me a very fruitful source. I extract it from a little work which I had prepared for popular use nearly twelve months ago, but which I have been dissuaded from publishing; and which goes to shew, that if usual laws act in the maintenance of the circulation as elsewhere, an inherent contractile power of the veins, in any degree, would not only not accelerate or facilitate, but would even *impede* the circulation of the blood. I extract the whole passage, because of the reference of its several parts:—

"A sort of physical reason may be given for the muscularity or contractibility of the arteries. Though the contraction of the left ventricle is so powerful, and gives so great an impulse to the column of blood in the arteries, yet, in consequence of their repeated subdivisions and their gradually increasing aggregate calibre, its effects must greatly diminish towards the extremities of the column—as we see from the disparity in force of the pulse at the wrist and that at the heart; and at the terminations of the column, after the repeated subdivisions and tortuosities of the branches (a considerable *break* to the force being opposed at and *by* the "*fork*" of each subdivision), and the immense increase in the aggregate quantity of their contents (by which the aggregate elasticity of the mass of blood is increased in proportion to the quantity), the force should *now* be barely perceptible, if at all so; and thus we have the uniform flow from the capillary arteries. Thus, then, the power diminishes while the work to be done increases; the contractibility of the arteries making up the deficiency. But, for the very same reason that the arteries require this power to maintain the circulation, *the veins should be without any such.* The blood has to pass from all their peripheral terminations, a roomy space in the aggregate, into fewer vessels, and smaller in comparison with that aggregate. This is a sufficient obstacle for the blood to overcome, without adding any contractile powers of the vessels; and so great is the difficulty presented, that valves are furnished to support the column before them and strengthen their coats, because of the great posterior force they have to sustain. This very difficulty of progression, however, ensures a continued impulse to the anterior part of the column; for it is driven on *wedgewise* by a larger mass behind, constantly acting, into a space which is relatively confined; nor does the force abate. The principles of hydraulics alone, then, would ensure increase of velocity, as well as continued progression, to the venous blood. But this supposes that *their* capillaries are always full—this is a *sine quâ non* to the impulse; and this is secured by the contractile power of the capillary arteries. There would be the same facility for a progression of their blood from their greater to the smaller space, by their construction and position, and relation—and which, in the arteries, would be a retrograde movement; but here, at *the end* of *this* smaller space, where the power can be wholly applied, is the contraction or the ventricle; and, all the way down, the contractibility of the arteries, which is progressively increasing. These are insurmountable obstacles; and the only place for these powers to expend themselves is in the equally roomy space of the capillary and smaller veins, which present no such impediments.

"Thus the circulation would appear to depend more on the vessels themselves, on account of their relations and construction, than on the heart; the contractions of the ventricles (the direction in which this power is applied being wholly regulated by their valves) being required merely to ensure that the increased muscularity of the capillary arteries is acting *in the right direction.* Their subsequent resiliency may do somewhat towards sucking on the venous blood, but it would not ap-

pear to be essential; and the auricles would seem to be of use only to ensure supplies in regular times and given quantities for the ventricles to dispose of. This giving so much power, too, to the vessels themselves (the arteries), would facilitate the explanation of many phenomena which their actions present when influenced by disease; most *general* diseases being much connected with *vascular* action primarily, and that of the heart secondarily; whereas, when the heart is the primary seat of disease, it manifests itself, often for a long time, not much more than locally, except under temporary excitement; and even then the general affection will not amount always to disease."

Mr. Abernethy, in his lectures, used to apply this principle to the progression of the blood *from* the heart; but I never met with any argument by a converse application of the principle, so as to explain its progression *to* the heart. But if it obtain in one instance, it does equally so in the other.—I am, sir,

Your very obedient servant,

GROVE BERRY.

DR. HARDWICKE SHUTE ON COLD WATER IN CHOLERA.

Memorial, No. I. addressed to the Central Board of Health, London.

GENTLEMEN,

IT is stated in your circular, dated 9th August, 1832, that although, under the distressing appearances which characterize the third stage of cholera, there is but little reason for hope, our exertions should not cease. A statement amounting, I conceive, to an acknowledgment, founded on the most extensive observation which the records of the disease in this country afford, that a very great majority of the patients, say forty-nine in fifty, or at least eighteen in twenty, perish in the third stage of cholera. I cannot, under such circumstances, be accused of presumption if I request your particular attention to a plan of treatment which I have found succeed in twelve cases out of fourteen; and I might conscientiously say, in twelve consecutive cases in which my directions were strictly adhered to.

My attention was very early directed to the fact, that no good, or rather, as it appeared to me, that most decided injury was done, by the administration of brandy or alcohol, in any of its multifarious forms, or even of stimulant emetics, where they were retained, as frequently happened in the advanced stage of collapse. In accounting for the deaths which commonly occurred in the course of a few hours, I was most forcibly struck with the marked analogy which exists, as far as the state of the pulse, and diminished animal heat are concerned, between the collapse of cholera and the impaired vital energy which results from starvation, or from long-continued exposure to excessive cold. Now it is an established fact in therapeutics, that the administration of a stimulus, disproportioned in strength or extent to the impaired vital energy of the system, is, under such circumstances, certain death, and that the mortification of a frost-bitten extremity uniformly results from the hasty application of too great a degree of heat, or other stimulants, to the affected part. The treatment which I have adopted in the second and third stages of cholera, is founded on the analogy already mentioned, and on the general principle, that the greater the degree of collapse, or sinking of the vital powers, the greater is the necessity of diminishing, or absolutely withdrawing, all kind of stimulus. The circumstance which particularly directed my attention to the remedy I employ was the thirst—the excessive, inordinate desire on the part of the patient for water, for cold water more particularly, and I may almost say, for cold water exclusively, all other liquids being taken with reluctance, if not absolutely refused. I shall, therefore, state in general terms that the free and unrestrained allowance of cold water, which in the most marked and favourable cases of recovery, was taken to the extent of some gallons in the course of a few hours, is the circumstance to which I desire particularly to call your attention. If I add to this the abstraction of all kinds of stimulus, both external and internal, even to the exclusion of friction, or the application of heat in any form, I have stated generally the whole plan of treatment which, as I said before, has succeeded in twelve out of fourteen, if not in twelve consecutive cases of the third stage of cholera. I am fully aware that the number of cases is too few to justify any thing like a general conclusion; but you will, I think, agree with me that they are not too few to justify a more extensive trial

of the plan proposed; and it is with this view that I now address you, hoping, that through your influence such trial will be made on a more extensive scale than is in the power of any single individual.

I shall now mention some particulars which appear to me calculated to throw additional light on the plan of treatment I propose, it being understood that my observations apply to the second and third, particularly to the third stage of cholera, "when the pulse at the wrist has ceased, or become almost imperceptible."

The windows of the apartments, at the Cholera Hospital in Gloucester, are large and numerous in proportion to the size of the room, and the door which opens immediately into the garden is seldom shut. The windows are open day and night, so that the patient may be considered as living in the open air. I may add, that the fire is purposely kept so low as not to influence the temperature of the room. The covering of the patient is confined to a light blanket and rug; and it seldom happens that some part of the patient, particularly the breast and shoulders, is not constantly exposed. Under these circumstances, a pint of cold water is offered to the patient, and very frequently two-thirds of this are taken at a draught. In what I consider the most favourable cases, vomiting is almost immediately produced, and the patient in two or three minutes again calls for, and eagerly drinks, the same quantity with the same results. This is often continued for hours, until gallons of water have been taken, and the greatest proportion, but I conceive not all, rejected. In other cases the patient is too insensible to ask for water; and under these circumstances, it is offered every ten minutes, or quarter of an hour, and most commonly drank with avidity. If gruel or tea be offered, the patient most frequently refuses it; and, generally speaking, no kind of nutriment is taken in any form until the period of convalescence. I consider it of great consequence that the vital powers should be restored as gradually as possible; and it is of importance to remark that the progress to recovery has been in all the cases extremely gradual and uniform. In the first six or eight hours no amendment can be observed, except the circumstance of the patient not being worse, can be so considered. In the next six or eight hours, no amendment can be observed except some diminution of intensity in the purple hue of the extremities. In the next six or eight hours there is a manifest improvement in the countenance of the patient, but often no restoration of pulse or increase of temperature, but there is increased disposition to sleep. In some cases the pulse has not been perceptible for twenty-four or thirty-six hours. From this period the pulse, the animal heat, and the secretions, are very gradually restored; and at the end of forty-eight hours, on the third day from the commencement of the plan of treatment proposed, the patient is convalescent; and in all cases without consecutive fever. I mention these circumstances particularly, in order that the practitioner may not be impatient: he should, in my opinion, be satisfied, and make not the least alteration in the plan laid down, as long as the patient is merely not getting worse.

It would be inconsistent with the professed object of this memorial to offer any remarks on the pathology of cholera, or the "modus operandi" of the plan of treatment recommended, but I cannot refrain from briefly suggesting, 1st, the importance of knowing, that in the collapsed stage of cholera, cold may be extensively applied to the coats of the stomach without diminution (to use the most cautious term) of the vital energy; 2d, that cold so applied has a manifest tendency to check the serous secretion; or perhaps, more correctly speaking, the excretion or exudation of serum, which characterizes the disease; 3d, the acknowledged effect of vomiting in checking diarrhœa, equalizing the circulation, and disgorging the vessels of the liver, and the consequent importance of not checking a natural action, which has a conservative tendency; 4th, the probable effect of the fluid absorbed from the stomach in restoring the fluidity of the blood, and the presumption that the saline treatment, both as applied to the stomach, and its use by venous injection, may owe its effect as much, probably more, to the fluid itself, than to the ingredients which it contains; 5th, the great importance of the arterial circulation being restored as gradually as possible, and the fact of convalescence taking place without the consecutive fever, which so often disappoints our hopes, and proves fatal in the fourth stage, or the stage of re-action.

The cases alluded to will be published

as soon as my present engagements will admit of my doing so.

The Secretary is requested to acknowledge the receipt of this memorial.

HARDWICKE SHUTE, M.D.

Gloucester, Aug. 21st, 1832.

Memorial, No. II., addressed to the Central Board of Health, London.

GENTLEMEN,

In reply to your first question—viz. since what period I have adopted the water practice—I beg to say, that the first case in which it was tried, and proved successful, occurred about six weeks ago, soon after appearance of the disease in this city. In the four first cases which came under my notice, internal and external stimulants were liberally administered. The mustard emetic and bleeding were also had recourse to, and every patient died in less than twelve (one in six) hours. I determined, therefore, to take the first opportunity of watching the disease uninfluenced by remedies, (having understood from my professional brethren that the saline treatment had equally failed,) with a view of ascertaining, if possible, what were the natural efforts of the constitution, if any, for the relief of the disease. In the four cases mentioned, I had observed that thirst was a prominent symptom, that stimulants were taken with great reluctance, if not positively refused; and in one instance of extreme collapse, when the patient was apparently unconscious of every thing, I was much struck with an earnestly expressed desire that no fire should be made in the room; directions to that effect having been given in her hearing.

Soon after this, I was requested by a medical gentleman of this city to visit one of his patients, a female, aged 15, whose father was at that time lying dead in the house, of cholera. It was a marked case of collapse, attended with constant vomiting and purging of a fluid like rice-water. The pulse at that time (one o'clock P.M.) was scarcely perceptible. I found that calomel and opium had been previously given; recommended the combination of opium with large doses of various stimulant antispasmodics and olive oil, and revisited the patient at seven that evening. The symptoms were at that time decidedly worse. The vomiting and purging unabated. The pulse imperceptible, the countenance cadaverous in the extreme. I recommended, in despair, ten grains of musk to be given every hour, to the extent of two scruples, and left the house with the impression (which was also that of the other medical gentleman in attendance) that our patient would not survive many hours. Having reflected upon the case, and feeling anxious, if possible, to give some relief, I returned in the course of an hour, and ordered some very strong green tea to be made, with the intention of giving it to an unlimited extent. Whilst the tea was in preparation I visited the patient, and to relieve the thirst, which was urgent, presented about a pint of cold water. There was at that time no improvement in the symptoms before mentioned. The greater portion of the water was drank with avidity, and almost immediately rejected. In a few minutes after, the water was asked for, and drank with the same result. Having witnessed a continued repetition of this process for half an hour, without any symptoms of increased collapse, and with the satisfactory circumstance of a change in the voice, which had been strongly characteristic of the disease, I left the patient, with directions that cold water should be given to any extent which she might desire. The tea, which had been occasionally offered, was refused as soon as tasted. I was informed the next morning that she had taken the water to the extent, as it was imagined, of some gallons, and had been constantly sick after each draught; but had not asked for the water, or been sick, for the last hour. The window, which opened immediately upon the bed of the patient, was unclosed, and had been so, I was informed, the whole of the night. At this time there was no perceptible pulse, or increase of animal temperature, but the countenance was, I thought, somewhat improved. The same plan was continued. In the middle of the day there was no manifest improvement, but in the evening the pulse was perceptible. On the following morning reaction was established, and in two days from that period the patient was convalescent without fever. If I have been tediously minute in the history of this case, I trust it will be attributed to the great interest I must naturally feel in a case which terminated so satisfactorily, and which was the foundation of the practice I am advocating.

Soon after this, I met another medical gentleman, who informed me that he was quite in despair—that he had lost *seven* cases of cholera in succession, and had just visited another who would most probably be dead before night; and that he had done nothing, merely ordering small doses of capsicum with camphor mixture. I found this patient (a female, æt. 21) in a very similar state to the last; recommended the unrestrained use of cold water; was informed the next day that she was better, and on the following day found her restored, but with symptoms of too much excitement. This circumstance, however, was accounted for, by the fact of beer and cider, as well as cold water, having been, through the prejudice of the attendant, very freely administered. She was perfectly restored in a few days.

To your second question—viz. "whether that practice has adopted (intended, I presume, has been adopted) by other medical gentlemen, to your knowledge?"—I answer, yes; in one instance to my knowledge, and I have reason to think in another, but in some modified form: in both instances originating in my suggestion. The gentleman with whom I attended the second case already mentioned, has been kind enough to favour me with the following letter, of which I send you a copy:—

"My dear sir,—It is with much pleasure I here enumerate the cases of cholera which have come under my notice since you suggested the cold water treatment; upon the success of which I beg to offer you my most sincere congratulations, and doubt not but the same beneficial effects which I have witnessed will be generally experienced when the practice has become more frequently adopted.—I remain

"Your sincere and obliged friend,

"CHARLES CLUTTERBUCK.

"Gloucester, Aug. 25, 1832."

A. B. (the names are given in Mr. Clutterbuck's letter), female, age 20, the young woman you saw with me, having drank freely of cold water, recovered in a few days; able to go to her usual employment.

B. C., male, boot-maker, age 21, a very hard drinker; took one dose of an aromatic mixture, drank freely of cold water, and could not be prevailed upon to taste his mixture afterwards; perfectly recovered in a few days, to the great astonishment of his friends.

C. D., male, age 5 years. A very severe case. This child drank, during the night, nearly *three gallons* of cold water, and was convalescent; but is now suffering from a severe attack of fever.

D. E., female, age 34. The same plan of treatment, and was convalescent in a few days.

E. F., male, age 32. Treated upon the same plan, and convalescent in a few days.

I have, I conceive, fully answered the questions proposed; but if it was intended to ask me, in the second question, whether, to my knowledge, any other person had previously adopted the same or a similar practice, I answer, no.

My experience, subsequent to the cases mentioned, has been almost confined to the Cholera Hospital here, which was opened a fortnight since, and placed under my superintendance, with the assistance of a resident apothecary. The first four patients sent to the hospital on the first day, but which was not then under my care, were all corpses the next morning. Since that period, of 48 cases there have been 20 discharged cured; eight I consider convalescent; eleven deaths, and nine under treatment. Of the deaths, two were obviously referable to previous treatment; three were in progress towards recovery, and relapsed from over exertion while in a state of extreme debility; two were under the age of three years, and died from cerebral congestion; one was more than fifty years of age; one sixty—the former extremely emaciated, from vomiting or disease; one was so far advanced as almost to have lost the power of swallowing; and one, a notorious drunkard, died with a gin-bottle concealed about her person, a few hours after admission into the hospital. If due consideration be given to these circumstances, the deaths mentioned cannot, I think, throw any discredit on the practice. My experience hitherto justifies, in my opinion, the following conclusions: that, in all cases, the progress towards death is retarded by the cold water treatment; that when the irritability of the stomach remains, the water is taken with eagerness, and almost immediately rejected; re-action will be established in a great majority of the cases, and lead to subsequent recovery without fever, when the natural powers of the constitution are not, from the extremes of age or other causes, unusually debilitated. Any suggestion from those who are much more conversant with the

disease than myself, will be received with gratitude, or any question answered to the utmost of my ability.

I am, Gentlemen,
Your obedient humble servant,
HARDWICKE SHUTE, M.D.

[In a case which recently occurred within our own knowledge, an individual was seized in the night with violent watery vomiting and purging, attended with cramps of the limbs, and against the advice of those about him, insisted on drinking pint after pint of cold water—in the manner and with the effect described by Dr. Shute. The patient represents the relief as having been very great; and his recovery was so complete, that in twenty-four hours he was quite convalescent.—E. G.]

SALINE TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

Eton, August 28, 1832.

SIR,

IN a communication which I received from the Central Board of Health last week, respecting a paper of mine, sent by them to your journal, on the nature and treatment of cholera, it was stated that you declined inserting it on the ground that it did not contain sufficient evidence on the point which it went to advocate—viz. the saline treatment. I regret that I did not send it direct to you, because I have the highest opinion of that treatment, and, from what I have seen, am most anxious for its general adoption, from the belief that it will be a great benefit to mankind.

I have now divested my paper of all theory on the nature of the disease, confining myself to a few remarks on its treatment, with a plain statement of facts in confirmation thereof (for I am aware it is these alone that you require, amid the multiplicity of opinions on the subject), in the hope that you will give it room in your next number.

I have had twenty-nine cases of the prevailing epidemic within the last seven weeks. The first that occurred to me was in the Eton poor-house (which is under my care). The patient had been intemperate in his habits, and was seized with severe vomiting, purging, and abdominal spasms. He was treated on the old system of calomel and opium, which subdued the violence of the symptoms, at the expense of the little remaining strength of the constitution; and he sank of fever and exhaustion, within three days from the attack. All the other cases were treated with saline remedies, and all speedily recovered under its almost magical effects. Two of these were in the poor-house, and the others principally in low, close parts of the town. Three of these were children—one in a state of collapse for some hours, and apparently dying. Nine of the worst cases were bled, with apparent advantage. Six, in whom the head was particularly affected, had one small dose of calomel and rhubarb; and the whole were promptly treated at the outset of the attack, and recovered, for the most part, within 48 hours. The carbonates of soda and potash (preferring the latter, as less nauseous), in solution with camphor julep, and a little syrup, were the only remedies used except those mentioned. In every case (except the children), from ten to fifteen grains of the salt were given every one, two, or three hours, according to the irritability of the stomach, and the vomiting was stopped in a surprising manner, where laudanum would have been instantly rejected. Three-fourths of these cases occurred in one week, at the period when several malignant ones (nine) proved fatal in the neighbourhood. There were five cases in Windsor, and only one of them was cured: this patient was the master of the Windsor workhouse. He had all the most characteristic symptoms of the disease, and was *blue* enough to convince the most sceptical of its identity. He was treated by Mr. Fowler (an Indian practitioner), upon Dr. Stevens's plan; the attack was not followed by secondary fever, and he is now so well that, were Sir David Barry to see him, he would probably deny he had ever had the complaint. None of the fatal cases were treated on the saline plan, or I doubt not that almost all of them, had they been seen sufficiently early, would have recovered; for such is my faith in the efficacy of this treatment, that I would undertake to cure, with the *carbonates alone*, almost every case that occurred, if seen in the *first* stage of the disease; but in the last stage I should certainly adhere strictly to Dr. Stevens's plan. In fact, we have had so many sudden deaths under the old treatment, and so many recoveries under the saline practice, that my

belief is these salts possess a specific influence in neutralizing or destroying the poison of cholera, when diffused in the human system; and I am far from solitary in deeming the discovery of this remedy as one of the most important and beneficial of the age, and its author, not only a real benefactor to mankind in general, but one of the greatest contributors to medical science.—I am, sir,

Your obedient servant,

WILLIAM MOSS.

P.S.—A fatal case of malignant cholera occurred in Windsor on Sunday last: it did not become serious till the evening, and terminated the following morning at six o'clock. The treatment pursued was that of calomel and opium.

CHOLERA IN COLD-BATH-FIELDS.

To the Editor of the London Medical Gazette.

House of Correction for the County of Middlesex, Aug. 28, 1832.

SIR,

BETWEEN my leaving Town on the 11th of this month (Saturday) and my return on Tuesday morning, the 21st, I learned that twenty-three cases of cholera had passed through the infirmaries of this prison; and since that time, this morning, Tuesday 28th, nine cases more; most of whom have been returned to the convalescent yard. Those in the infirmaries are apparently doing well. I lament to say we have lost one man. So that, in addition to my statement, August 1st (under saline treatment, one lost, thirteen saved), I have now the happiness to state, out of thirty-two cases, one lost, thirty-one saved.

I beg leave to express how astonished I am, that, with facts of so overpowering a nature, medical men and others should be so blinded, so incredulous, so obstinate, so infatuated, almost every where, as to run about and tell us, that in this new disease they either know not what to do, or are so wedded to ancient prescription and the authority of some school or other, that they will suffer their patients to die rather than adopt a practice so efficacious in its salutary operation, and almost infallible in its beneficial results.—I am, sir,

Your most obliged humble servant,

JOHN OUSBY,

Chaplain of the House of Correction for the County of Middlesex.

HYDROSTATIC BED FOR INVALIDS.

Extract from the 5th Edition (now in the press) of DR. ARNOTT'S *"Elements of Physics."*

IT is to mitigate all, and entirely to prevent some, of the evils attendant on the necessity of remaining in a reclining posture, that the hydrostatic bed is intended. It was first used under the following circumstances:—

A lady, after her confinement, which occurred prematurely, and when her child had been for some time dead, passed through a combination and succession of low fever, jaundice, and slight phlegmasia dolens of one leg. In her state of extreme depression of strength and of sensibility, she rested too long in one posture, and the parts of the body on which she had rested all suffered: a slough formed on the sacrum, another on the heel; and in the left hip, on which she had lain much, inflammation began, which terminated in abscess. These evils occurred while she was using preparations of bark, and other means, to invigorate the circulation, and while her ease and comfort were watched over by the affectionate assiduity of her mother, with numerous attendants. After the occurrence, she was placed upon the bed contrived for invalids by Mr. Earle, furnished for this case with pillows of down and of air of various sizes, and out of its mattress portions were cut opposite to the sloughing parts; and Mr. Earle himself soon afforded his valuable aid. Such, however, was the reduction of the powers of life, that, in spite of all endeavours, the mischief advanced, and about a week later, during one night, the chief slough on the back was much enlarged, another had formed near it, and a new abscess was produced in the right hip. An air pillow had pressed where these sloughs appeared. The patient was at that time so weak that she generally fainted when her wounds were dressed; she was passing days and nights of uninterrupted suffering, and as all known means seemed insufficient to relieve her, her life was in imminent danger.

Under these circumstances, the idea of the hydrostatic bed occurred to me. Even the pressure of an air-pillow had killed her flesh, and it was evident that persons in such a condition could not be saved unless they could be supported without sensible inequality of pressure. I then reflected, that the support of water to a floating body is so uniformly diffused, that every thousandth of an inch of the inferior surface has, as it were, its own separate liquid pillar, and no one part bears the load of its neighbour—that a person resting in a bath is nearly thus supported—that this patient might be laid upon the surface of a bath over which a large sheet of the water-proof India-rubber cloth were previously thrown, she being rendered sufficiently buoyant by a soft mattress placed

beneath her—thus would she repose on the face of the water, like a swan on its plumage, without sensible pressure any where, and almost as if the weight of her body were annihilated. The pressure of the atmosphere on our bodies is of fifteen pounds per square inch of its surface, but because uniformly diffused, is not felt. The pressure of a water-bath of depth to cover the body, is less than half a pound per inch, and is similarly unperceived. A bed, such as then planned, was immediately made. A trough of convenient length and breadth, and a foot deep, was lined with metal, to make it water-tight; it was about half filled with water, and over it was thrown a sheet of the India-rubber cloth as large as would be a complete lining to it if empty. Of this sheet the edges, touched with varnish, to prevent the water creeping round by capillary attraction, were afterwards secured in a water-tight manner all round to the upper border or top of the trough, shutting in the water as closely as if it had been in bottles; the only entrance left being through an opening at one corner, which could be perfectly closed. Upon this beautiful dry sheet a suitable mattress was laid, and constituted a bed ready to receive its pillow and bedclothes, and not distinguishable from a common bed but by its most surpassing softness or yielding. The bed was carried to the patient's house, and she was laid upon it; she was instantly relieved in a remarkable degree: sweet sleep came to her; she awoke refreshed; she passed the next night much better than usual; and on the following day Mr. Earle found that all the sores had assumed a healthy appearance. The healing, from that time, went on rapidly, and no new sloughs were formed. When the patient was first laid upon the bed, her mother asked her where the down pillows, which she before had used, were to be placed; to which she answered, that she knew not, for that she felt no pain to direct: in fact, she needed them no more.

It may be here recalled to mind, that the human body is nearly of the specific gravity of water, or of the weight of its bulk of water, and therefore, as is known to swimmers, is just suspended or upheld in water without exertion, when the swimmer rests tranquilly on his back with his face upwards. He then displaces water equal to his own body in weight as well as in bulk, and is supported as the displaced water would have been. If his body be two and a half cubical feet in bulk, (a common size) he will just displace two and a half cubic feet of water, equal in weight to his body. If, however, instead of displacing the water with his mere body, he choose to have something around or under him which is bulky with little weight, as the mattress of the bed above described,—when his weight has forced two cubical feet of that under the level of the water around, he will float with four-fifths of his body above the level, and will sink much less into his floating mattress than a person sinks in an ordinary feather-bed. It thus appears that by choosing the thickness of the mattress, and if unusual positions are required, by having different thickness in different parts, or by placing a bulk of folded blanket or of pillow over or under the mattress in certain situations, any desired position of the body may be easily obtained. If the water be about six inches deep, which in general will suffice, the person standing upon any part of the bed, or sitting with the knees raised, will cause the part of the mattress on which he rests gently to touch the bottom, because a narrow end of the body cannot displace water equal to the bulk of the whole, but then the person is standing or sitting on a soft sofa, and in standing or sitting, he naturally prefers the fixed to the floating support; on lying down, however, he as completely floats as if the Atlantic were under him.

This bed is a warm one, owing to water being nearly an absolute non-conductor of heat from above downwards, and owing to its allowing no passage of cold air from below. From this last mentioned fact, however, less of the perspiration, sensible and insensible, will be carried off by the air than in a common bed, and unless the patient can rise, or be lifted daily, to allow the bed to be aired like a common bed, there will be a necessity for using some such means as the following to prevent the condensation of perspiration on the water-sheet below: an oiled silk laid over the mattress; or a blanket, to be occasionally changed, laid under it; or a set of flexible tubes of spiral wire laid under it, with their ends open to the atmosphere, to allow a constant ventilation of the mattress; or similarly placed, and producing the same effect, a layer of cork, cut into square pieces, with spaces left between them to serve as conduits of air. This bed is in itself as dry as a bed can be, for the India-rubber cloth (of which bottles can be made) is quite impermeable to water, and the maker is now preparing cloth expressly for this purpose. Then as Sir Humphry Davy recommended that his safety-lamp should be double, some persons may prefer a double sheet, to obviate the possibility of accident. Unlike any other bed that ever was contrived, it allows the patient, when capable of only feeble efforts, to change his position, almost like a person swimming, and so to take a degree of exercise, affording the kind of relief which in constrained positions is obtained by occasional stretching, or which an invalid seeks by driving out in a soft-springed carriage. It exceedingly facilitates turning, for the purpose of dressing wounds; for by raising one side of the mattress or depressing the other, or merely by the patient's extending a limb to one side, he is gently rolled over, nearly as if he were simply suspended in water; and it is possible

even to dress wounds, apply poultices, or place vessels under any part of the body, without moving the body at all; for there are some inches of yielding water under the body, and the elastic mattress may at any part be pushed down, leaving vacant space there, without the support being lessened for the other parts. Then, with all the advantages which other invalid beds possess, and with those which are entirely its own, it may yet be made so cheaply, that even in hospitals where economy must prevail, it may at once be adopted for many of the bed-ridden. Mr. Earle, within a few days of seeing the first one, had others made for patients in St. Bartholomew's hospital, and has been as much pleased with the results of them as of the first. The bed has since been introduced into St. George's hospital by Mr. Keate, and elsewhere. The author has now seen enough of the effects of this bed to make him feel it a duty at once to publish a notice of it. With it, evidently, the fatal termination called sloughing, now so common, of fevers, and other diseases, need never occur again. And not only will it prevent that termination, but by alleviating the distress through the earlier stages, it may prevent many cases from even reaching the degree of danger. Then it is peculiarly applicable to cases of fractured bones, and other surgical injuries; to palsies, diseases of the hip-joint, and spine; and universally, where persons are obliged to pass much time in bed. And in all cases of curvature of the spine, either actually existing or threatened, it affords a means of laying a patient in any desired position, and with any degree of pressure incessantly urging any part of the spine back to its place. If used without the mattress, it becomes a warm or a cold bath, not allowing the body however to be touched by the water; and in India, it might be made a cool bed for persons sick or sound, during the heats which there prevent sleep and endanger health. There are numerous other professional adaptations and modifications of it, which will readily occur to practitioners sufficiently versed in the department of natural philosophy (hydrostatics) to which it belongs. Before reflection a person might suppose a resemblance between it and an air-bed or pillow, calling this a water-bed or pillow; but the principles of the two are perfectly distinct or opposite. An air pillow supports by the *tension of the surface* which encloses the air, and is therefore like a hammock or the tight sacking under the straw mattress of a common bed, and really is a hard pillow; but in the hydrostatic bed, there is no tense surface or web at all: the patient is floating upon the water, on which a loose sheet is lying, merely to keep the mattress dry, and every point of his body is supported by the water immediately beneath it. To recal the difference here described, and which is of great importance, the bed is better described by the appellation of *hydrostatic bed* than of *water-bed*.

The author has given no exclusive right or privilege to any person to make this bed. He has hitherto employed the carpenter nearest to him, Mr. Smith, 253, Tottenham-Court Road, at the back of Bedford-Square; and the manufacturers of the water-proof cloth, Mackintosh and Co., 58, Charing Cross; but any carpenter or upholsterer may learn to supply them, and he gives free permission to all.

The preceding paragraphs are intended as much to direct in the choice and use of common beds for the sick, as to announce and describe the hydrostatic bed for the cases in which it may be required. At present the medical attendant generally leaves whatever regards the bed to the judgment of friends or nurses; but evidently, he who has been led to reflect how much the course and event of a malady may depend on the patient's being supported, so that no pain shall arise from local pressure, and as little muscular weariness as possible from constrained position, will deem the bed-management worthy of his own attention, and will be able more judiciously both to choose and to use beds. There is a bed constructed of spiral springs, which may be made so as to diffuse the support more equably than any except the hydrostatic bed; and had professional men generally been acquainted with it, it would have been more used than it is, and would have received various modifications, of which it is susceptible, for medical purposes. It has long been known, chiefly however as a mechanical curiosity, or an object of luxury, and was introduced into this country about seventy years ago by Mr. Merlin; but it has been so little known, that a few years ago an English tradesman thought he might appropriate the manufacture by taking a patent for it. It is now made by upholsterers generally; and the same principle is applied in the construction of sofas, chairs, and carriage cushions.

MEDICAL GAZETTE.

Saturday, September 1, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

LAWS AFFECTING THE MEDICAL PROFESSION.

THE recent conviction at Warrington may not be without its use. We do not simply mean that we shall have cholera cases returned with more regularity in future, and obedience better paid to the

mandates of the constituted Boards of Health—though these things in themselves are by no means undesirable—but the transaction we refer to will have conveyed a practical lesson, valuable in so far as example is ever more efficacious than precept. The interest excited among practitioners throughout the country by the news of this Warrington affair has, we suspect, conveyed to many of them the first intimation of the existence of an act of Parliament under which such convictions, with penalties, may take place. This it is to be taught practically — the principal mode, we fear we must add, in which the knowledge of our laws is generally communicated. The Cholera Act certainly was passed with unusual rapidity through its various stages in both Houses, and several months elapsed, without any demonstration, since it became the law. But the Anatomy Act affords, perhaps, a better instance. Here was an important legislative measure carried but the other day, exceedingly brief in its provisions, and much canvassed both in and out of Parliament while undergoing the process of being made a law; yet how little apparently has been known about it! Few as its clauses are, how few of them are distinctly understood, or, at least, how little have they been attended to! A murderer, however, is condemned, and sentenced, according to its provisions, to be *gibbeted*, in lieu of the usual punishment of *dissection*, and immediately all the world is up in amaze at the extraordinary thing done under the new Anatomy Act. They wonder how such a passage in it could have escaped them; yet there the passage was, all the time that the bill was before the legislature; and there it might have remained, no doubt, many a day unnoticed, were it not for the practical lesson conveyed by the sentence and execution of a couple of murderers. Thus it is, doubtless, that the Anatomy Act will gradually become better understood. There are some other points in it still, short as it is, that will most probably need the same sort of palpable commentary, before they are thoroughly brought to the cognizance of the community. But the process is not peculiar to the Anatomy Bill, as we may assure ourselves by what is taking place every day before our eyes. We may observe a very remarkable inability, or an indifference, on the part of the public, to make themselves acquainted with numerous other matters also which it concerns them vitally to know.

The fact is, that an acquaintance with the existing laws of the land, is a matter very curiously provided for by our legislators—that is, if it can be said to be actually provided for at all. It is certain that we have no code that we may take from our pockets, and read as we run. There is not even a popular current digest of our actual laws, so far as we are aware, either for use or ornament. We are obliged, as the saying is, to wade through a river of words for a spoonful of sense; or to wait till we are taught *practically*, by some infliction on our purse or person—or those of our neighbours. The old tyrant who had his scruples, and wished to do every thing according to law, only made an experiment upon the eye-sight of his subjects, and was willing to try at what height they could read, and in how small a letter. There are many of us, we fancy, who would be glad to have even so good a chance. If *our* laws were set up in the same way, what with gas light and the aid of optics, we might make a tolerable shift to come at them; but as it is, the powers of a Lynceus and a Hercules combined might quail at the task. The usual course is to acquire a smattering of legal knowledge at second-hand, by paying learned Pundits, or interpreters, who are well known to have no great claims to infallibility; or we

may profit by our own personal experience. If we were convicted but once for libel, we dare say we should be as intelligent as any one on that most difficult branch of our law; but familiarly as it is talked of in the ordinary course of conversation, who will define it for us?—where is its definition to be found?—who can give us the best information about it? Who but he who has paid most dearly for his knowledge, by his experience—the convicted libeller—the practised hand that has been most frequently exercised in his own defence.

All this, perhaps it will be said, may be true of the laws in general—but how does it particularly apply to medical men, or affect them more than others? Much, we answer—very much. Medical men have not only their common share of the responsibility imposed by the laws upon them, as subjects of the realm, but they have a heap of special legislation to deal with—such as the members of no other profession have to encounter. They are tied and bound by a multitude of particular acts, direct and indirect, of the extent of which few even among themselves are fully aware: and what is still more seriously worthy of their consideration, is, that all those laws so specially affecting them as a body, have been imposed upon them *unrepresented.* Bills, indeed, are introduced into parliament, session after session, sanctioning the infliction of pains and penalties on medical men, and there is not a voice lifted up in either House to protect the rights of the parties so cavalierly treated. Talk of unequal representation after this!

But let us not be misunderstood. We do not mean to say that the legislation to which we allude is attended with peculiar or extraordinary hardship—nay, we believe that in most instances, when bills affecting the profession are in progress through either House, some distinguished medical man or other is consulted, and all due delicacy is observed; but what guarantee have we that it may not be often otherwise? This shew of attention to the interests of the profession, we ought to be aware, is entirely a thing of favour, and perhaps generally owing to personal consideration or private connexions. There is no reason, then, why it might not happen that some measure most obnoxious to us should be introduced, and even carried: considering the materials of which the parliament is composed—made up entirely of lawyers, commercial people, and country gentlemen—this is by no means impossible. It behoves us, therefore, to consider well that we have nobody of "our order," in either House, to represent us; and that this is the lot of the medical profession alone—that profession, the services of which are, upon all trying occasions, so unceremoniously made use of, and so reluctantly and so scantily rewarded. If it be said that the clergy are in a worse predicament, being absolutely *excluded* from the lower House, we ought not to forget that they are still far from being unrepresented, while, in the Lords in especial, they own a most formidable bench.

We were speaking of the unequal severity, and the no small difficulty of coming at a knowledge of the laws affecting our profession. Thanks to Mr. Willcock and his labours, much of the latter complaint has been removed; for in his excellent volume* we find almost every thing that is material on that head. But the same volume will fully bear us out in all that we could say on the question of undue severity. Look at those charters conferring on the existing corporations such a mere mockery of power as constitutes what can be deemed little short of absolute grievance; power which, if it were only attempted to be exercised in any one,

* Laws relating to the Medical Profession, 8vo. 1830.

however important, instance, would render the acting party ridiculous—nay, almost odious—in the eyes of the public. We allude particularly to the pseudo-provisions for the punishment and suppression of quackery. Then the crowd of liabilities to which the medical man is exposed—not merely for imputed want of skill and attention, and so forth, but for non-compliance with numerous civil regulations, in the ordering of which he has not been permitted to have any voice whatever, or control!

The upshot of the matter is this—that in the present growing state of enlightenment and superior civilization, it appears to be every day becoming more and more incumbent on the profession to make themselves thoroughly acquainted with the duties and obligations which their condition in society imposes on them. They will not find it, we rather think, much to their advantage to await the casual, however impressive, lessons of practical experience. They should be in *advance* of their possible difficulties. They should know their rights, and maintain them. Above all, they should exactly understand that position which they hold regarding the existing laws of their country, and should turn an anxious thought to the consideration of the grand question—who are the fit and proper persons who shall be entrusted with the framing of similar laws for their governance in future?

SALINE TREATMENT IN CHOLERA.

A NOTE will be found at page 712, from Mr. Ousby, Chaplain of the House of Correction, in which he accuses the members of the medical profession of *blindness, incredulity, obstinacy,* and *infatuation,* for not adopting the saline treatment of cholera; constituting (as he says it does) a remedy "almost infallible." We have given immediate insertion, as we pledged ourselves we should do, to all the evidence which might be transmitted to us on this subject, whether for or against the treatment alluded to; and we must say, in answer to our very respectable correspondent, that, up to the present time, the bulk of evidence is decidedly against the saline treatment. The question never can be decided by a reference to the Cold-Bath-Fields Prison, because all connected with it are regarded as partizans; and if they would produce any decidedly favourable impression on the public mind, it must be by adducing the testimony of others, unconnected with that establishment, who have tried it on a large scale, with success. If this can be done, why is it not? If it cannot, then is the saline treatment reduced precisely to the same level with many others—patients occasionally recover when it is used, but whether in consequence of its employment or not, is another question*.

PAPERS ON CHOLERA.

WE have frequently laid before our readers, papers on cholera with which we had been favoured by the Central Board of Health. We are requested by the Board to state, with a view of preventing any misapprehension, that, in transmitting such papers for publication, they do not intend to convey any expression of approbation or disapprobation regarding them; nor do they wish to be considered responsible in any way for the doctrines of their correspondents.

CONTAGION OF CHOLERA.

IT will be recollected that the greater number of the physicians of Paris subscribed, all of a sudden, to the doctrine of the non-contagion of cholera. M. Velpeau was one of those who set their names to the celebrated declaration published immediately on the outbreak of the epidemic in the French capital: this gentleman now candidly confesses the over-hastiness of that proceeding,

* We hear that the saline treatment has been tried at the hospitals in Abchurch-Lane, Greville-Street, and other places: why have we no reports from those quarters?

and has published, in the *Archives de Médecine*, a paper containing a narrative of facts, and the results of above eighty cases closely examined, which supply a chain of evidence of the most conclusive nature as to the positive existence of contagion.

COUNCIL OF THE COLLEGE OF SURGEONS.

Mr. Stanley has been elected into the Council of the College of Surgeons, in the room of Mr. Hawkins, resigned.

MODE OF PROCURING THE ANATOMICAL LICENSE.

The party applying for a License to practise Anatomy must make a written application to the Secretary of State, signed by him with his christian and surname at length; in which application he must state, he applies for a License to practise Anatomy, in pursuance of the Act passed for regulating the Schools of Anatomy. He must state also the place of his residence, and the place where he is about to carry on the practise of Anatomy; and must further state, that he is, according as the case may be, (a Fellow or Member of a College of Physicians or Surgeons,) *or* (a Graduate or Licentiate in Medicine,) *or* (a person lawfully qualified to practice Medicine, in England, Scotland, and Ireland) *or* (a Professor or Teacher of Anatomy, Medicine, or Surgery,) *or* (a Student attending a School of Anatomy.)

This application to be countersigned by two Justices of the Peace acting for the County, City, or Borough, wherein the party applying resides, and so describing themselves, certifying the place of residence of the party, and also the place where they believe him to be about to carry on the practice of Anatomy.

CHOLERA IN LONDON.

The cholera has undergone a decided and very considerable increase during the last ten days,—the cases are not only more numerous, but are upon the whole more rapidly fatal. Even the Bills of Mortality (see our last page) give us 274 deaths last week, being an increase of 158; while the increase during the week of burials is so great as 477!

DR. PRICE.

Dr. Charles Price, of Brighton, has been appointed Physician Extraordinary to the King.

CLINICAL OBSERVATIONS

Delivered in the General Dispensary,

By Mr. Coulson,

August 13th, 1832.

Icthyosis, of several years standing, cured.

Gentlemen,—I will first direct your attention to a case of icthyosis, which you have recently seen cured by very simple means. The boy, Walter Scott, aged 8, was admitted under my care on the 13th of July, with an affection of the skin, which he has had for several years, I believe from his infancy, and a prominence of the chest. The upper arms and the legs were the parts most affected, and these presented the appearance as if an innumerable quantity of very small brown pebbles had been glued to the skin. There was nothing like fish-scales to be seen, from the supposed similarity of the eruption to which the disease has derived its name, and on rubbing the fingers over the part, a rough, or grating sensation was communicated. The skin covering the back and chest was of a dirty brown colour, and had a rough feel. Independently of this disease, the child had a prominence of the chest, on the symptoms and treatment of which I will make a few observations after I have disposed of the treatment which was adopted for the affection of the skin. Dr. Clutterbuck, who was present when the child first applied, suggested that no internal medicines whatever should be given, but that local applications alone should be used, and of these he conceived that a lotion, composed of the liquor hydrarg. oxymuriatis, undiluted, to be as good as any. This lotion was used twice a day; it made the skin feel very stiff, but it certainly had a beneficial effect. I changed this application after a week, and ordered a liniment, composed of half an ounce of the ung. hydrarg. nitrat. and an ounce of olive oil, well rubbed down. The liniment was applied to the affected parts twice a-day; and on Friday last, when I saw the child, the complaint had completely disappeared.*

* Rayer in his work on Diseases of the Skin (Vol. II. p. 304), mentions that in summer this disease will sometimes disappear, and return in autumn.

The brownness and roughness of the skin still remain, but all the pebble (or, as it would be generally termed, *scaly*) appearance is quite gone. I have no doubt that any mercurial preparation in this case would have done good. The preparation which is employed in this institution with great success in most cases of porrigo, in the milder cases of lepra and psoriasis, as well as pityriasis, consists of an ointment, composed of one drachm of calomel and six of spermaceti ointment, or lard. It has a decided effect on the affections which I have mentioned; and as I have to my share alone on the average more than two hundred skin diseases here in the course of the year, it is very convenient to have one simple remedy applicable to the treatment of so many of them.

The prominence of the chest, for which the child now only remains under treatment, is a complaint of not very unfrequent occurrence in weak and delicate children. The sides of the chest are very much flattened, one side being sometimes more depressed than the other; the sternum projects, like that of a chicken or pigeon, from which circumstance, persons with this affection are called pigeon or chicken-breasted. The sternum, however, is not always so prominent as it at first sight appears to be, the prominence being formed by the sternal extremities of the ribs, and the sternum itself being either flat, or a little concave at its lower, and projecting at its upper part. Children with this affection, as in the child whose case we are now considering, breathe with difficulty; the tonsils are enlarged, and their sleep is very restless and disturbed. In fact, a peculiar symptom in this affection is the unpleasant noise which children make during their sleep. The treatment to be adopted consists in the application of pressure to the chest, and the use of those exercises which tend to expand the chest. The child should be placed against the wall, or laid on a table, and the hand should be pressed against the sternum, the pressure being made during expiration, and suspended during inspiration. The pressure should be made frequently during the day, and be continued for a few minutes each time. This is the plan which we are trying on this boy, and I have no doubt whatever of its success.

The next case I will speak of is one of iritis, with papular eruption. The patient, Mary Knight, aged 30, was admitted with an affection of the iris of the right eye; and the face and arms were covered with a papular eruption, which turned out to be *venereal lichen*. The patient on being questioned as to her having had any affection of the genital organs, strongly denied it, and attributed the origin of her complaint to a cold. The cause of the complaint would not have made any difference with me as to its treatment, for at all events I should have given her gentle doses of mercury until the affection was relieved. But I was anxious to satisfy my mind on the point; and, on examining the woman, she was found to be labouring at that time under gonorrhœa; and growing from the internal surface of the vagina there were large crops of warts, such as usually follow neglected excoriations. The warts were touched with the nitrate of silver; and a wash, composed of a solution of the nitrate of silver, was ordered for her; the compound decoction of sarsaparilla, and Plummer's pill, were given internally. Under this treatment the patient rapidly recovered. I mention this case to you because it illustrates two or three points to which Mr. Carmichael first drew the attention of the profession; it shews the kind of eruption which follows gonorrhœa and excoriations; next, it shews the kind of eruption which accompanies venereal iritis. Mr. Carmichael, whose essay on venereal diseases contains a greater fund of original observation than is to be found in any other work on the subject, says, that with a single exception, during the whole of his experience, he has found the eruption which accompanies venereal iritis of the papular form.

The following case was very obscure at its commencement, and terminated fatally a few days ago; it is a case of inflammation and suppuration of the psoas magnus and iliacus externus muscles of the right side. The poor man, Francis Green, fifty years of age, and by calling a porter, was admitted under my care July 25th, 1832. For five or six weeks prior to this time he had been confined to bed, and had been under medical treatment. When I saw him, he complained of a dull, aching pain, a little above the right groin, and at the upper part of the thigh. There was no swelling, with the exception of a slightly œdematous state of the thigh. He was unable (and had been during the whole of his illness) to lay with his body and the right lower extremity extended in a straight line; he could lay down provided his right thigh were drawn up at a right angle with his body, but not without. He had no power to bend the thigh without help, being obliged to raise the limb with the assistance of his hands. There was no pain whatever in his back. I entered the case as one of inflammation of the psoas and iliacus internus muscles. The man's strength was much reduced; his pulse was small and quick; appetite bad, and rest disturbed. As the pain was so great about the upper part of the thigh, blood was ordered to be taken from the part by leeches and cupping, and warm poultices to be constantly applied. The severity of the pain was lessened by these means; and after a few days (3d of August) a swelling made its appearance above Poupart's ligament, which left no doubt on my mind of its containing pus. An incision

was carefully made, about an inch in length, a little below, and to the outer side of the internal ring: this gave exit to a large quantity of thin greenish fœtid pus. My colleague, Dr. Roberts, who saw the patient with me, ordered him to take the quinine mixture every four hours, a grain of opium at bedtime, and as much nourishment as his stomach could bear. The wound continued to discharge copiously the same ill conditioned pus; his strength gradually sunk; and on the 8th he died. I omitted to mention, that for more than twenty years this man laboured under a very bad stricture of the urethra; and that his water came away in drops from him, or was obliged to be drawn off by means of a very fine gum-elastic catheter, which he could do for himself.

On the following day I examined the body. The right iliac fossa contained more than a pint of this offensive matter; the psoas and iliacus muscles were in great part destroyed; there was no affection whatever of the vertebræ. The matter had insinuated itself in one direction beneath the iliac fascia, in front of the transversalis fascia, and pointed to the spot where the opening was made. The matter also took another direction, some of it having passed down the thigh, not in front of the psoas magnus or iliacus internus (as is usual in psoas abscess,) but behind these muscles, to the back of the limb. No matter had gone into the pelvis. I removed the bladder and urethra from the body, and as there are several cases of stricture now under treatment, it will be instructive for you to examine this preparation, and see the changes which have taken place. There is a stricture just anterior to the bulb, which will scarcely admit a fine cat-gut bougie: and close to this stricture there appears to be a false passage; it is merely, however, a cul-de-sac, in which the instrument must have frequently caught, in its passage into the bladder. The more common seat of stricture is just behind the bulb. When it is situated anterior to this part, the complaint is more difficult to manage; whether this arises from the action of the accelerator urinæ muscle on this part of the canal or not, I cannot say, but you have had frequent opportunities of seeing the difficulty experienced in the treatment of bad strictures anterior to the bulb. The membranous portion appears to be small; but you should bear in mind, for it is an important fact, that the diameter of the urethra varies considerably in different parts. At the bulb of the urethra, in the healthy state, the diameter is three-fifths of an inch; and, as the circumference of the circle, by the nearest mathematical approximation, is three times its diameter, we find the bulb is nearly two inches in circumference. The membranous portion is seven-twentieths of an inch in diameter, or about an inch in circumference. You perceive then that the urethra is twice as wide in one part as in another. The complaint with which the man died, was not connected with the stricture; the latter complaint, as I said before, he had laboured under for more than twenty years; the former complaint about two months. He might have been easily cured of the obstruction in the urethra, if he had taken proper advice for it. The remainder of the canal was sound, with the exception of a slight ulceration of the mucous membrane, in the prostatic portion; and the application of the armed bougie a few times to the stricture, would have removed the complaint.

WEEKLY ACCOUNT OF BURIALS,

From the "Bills of Mortality," Aug. 21, 1832.

Abscess	6	Hooping-Cough	6
Age and Debility	108	Inflammation	64
Apoplexy	10	Inflammation of the Bowels & Stomach	36
Asthma	8	Inflammation of the Brain	3
Cancer	3	Lungs and Pleura	2
Childbirth	8	Insanity	4
Cholera	274	Jaundice	2
Consumption	127	Liver, Diseases of the	5
Convulsions	70	Measles	19
Croup	2	Mortification	8
Dentition or Teething	3	Paralysis	7
Diarrhœa	3	Rheumatism	2
Dropsy	28	Small-Pox	26
Dropsy on the Brain	23	Spasms	5
Dropsy on the Chest	2	Stone and Gravel	1
Epilepsy	1	Stricture	1
Erysipelas	1	Thrush	6
Fever	11	Tumor	1
Fever, Scarlet	3	Unknown causes	56
Fever, Typhus	3		
Gout	1	Stillborn	29
Hæmorrhage	1		
Heart, Diseases of	3		
Hernia	1		

Increase of Burials, as compared with the preceding Week 477

METEOROLOGICAL JOURNAL,

August 1832.	Thermometer.		Barometer.	
Thursday . 23	from 47	to 63	29·77	to 29·85
Friday. . . 24	43	67	29·93	29·99
Saturday . 25	47	63	29·89	29·78
Sunday . . 26	43	61	29 76	29·69
Monday. . 27	39	61	29·72	29·52
Tuesday . 28	41	59	29·26	29·18
Wednesday 29	43	57	29·28	29·35

Wind variable, S.W. prevailing.

A very dull, wet week; rain every day except the 24th; the last three days unseasonably cold.

Rain fallen, ·95 of an inch.

During the past week the barometer has made rather a rapid fall; on the 28th inst. it was lower than it has been at any period since the 2d of February last.

Charles Henry Adams.

NOTICES.

Mr. Bossey's valuable report has just been received.

We regret that we cannot make room for Mr. Parkin's paper.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 8, 1832.

ESSAYS ON DIAGNOSIS.

BY

MARSHALL HALL, M.D. F.R.S. L. & E. &c.

ESSAY I.—*concluded.*

III.—ON THE DIAGNOSTIC ARRANGEMENT OF DISEASES.

IT is said that there are more than fifteen hundred distinct varieties of the rose. It seems almost impossible that such a fact should be established; for when the number of objects and their similarity are so great, the distinction, identification, and enumeration of them, must be a matter of extreme difficulty. This difficulty is diminished almost infinitely by the simple means of bringing the objects together, and placing them *vis-à-vis* to each other, so that they may most readily be compared and contrasted.

Such is the design, such the object, of the diagnostic arrangement of diseases. Diseases which are similar, are, of course, apt to be confounded; the diagnosis can only arise from careful comparison and contrast: this is most readily accomplished by arranging such diseases, as it were in parallel lines.

It may frequently occur that the same disease, as inflammation and hysteria, may, in their different forms, resemble different diseases. In this case, the same disease cannot be placed in more than one part of the arrangement; perfection of classification being made to give way to practical utility.

The diagnosis and identification of diseases are, in this manner, greatly facilitated. This effected, and not otherwise, our knowledge of the pathology becomes available.

I.—DISEASES OF THE SYSTEM.

I.—FEVERS.
- 1. *Continued.*
  - *The Complications.*
- 2. *Periodical.*
  - *The Complications.*

II.—ERUPTIVE FEVERS.
- *The Complications.*

III.—
- 1. *Irritation.*
  - *The Complications.*
- 2. *Exhaustion.*
  - *The Complications.*
- 3. *Delirium Tremens.*
- 4. *Erethismus Mercurialis.*

IV.—
- 1. *Acute Dyspepsia.*
- 2. *Chlorosis.*
- 3. *Hysteria.*
- 4. *Some Nervous Diseases.*

V.—INFLAMMATION.
- 1. *Simple.*
  - *Its varied effects.*
- 2. *Modified.*
  - 1. *Furunculus.*
  - 2. *Carbunculus, &c.*
- 3. *Specific.*
  - 1. *Rheumatic.*
  - 2. *Arthritic.*
  - 3. *Syphilitic, &c.*

VI.—SCROFULA; TUBERCLES.

VII.—HÆMORRHAGIA.
- 1. *From Constitutional Causes;*
- 2. *From Mechanical Causes:*
  - 1. *Intra-vasated, or congestion.*
  - 2. *Extra-vasated.*
- 3. *From the Skin;—Purpura.*
- 4. *From the Mucous Surfaces, or the Hæmorrhages.*

5. *In the Substance of Organs, or the Apoplexies.*

VIII.—Scorbutus.

IX.—Melanosis.

X.—Encephalosis.

XI.—Scirrhus; Carcinoma.

XII.—Syphilis.

II.—DISEASES OF ORGANS.

I.—Diseases of the Brain and Spinal Marrow.

i. *The Sudden.*
ii. *The Acute.*
iii. *The Insidious.*
iv. *The Chronic.*
1. *Of the Brain.*
2. *Of the Cerebellum.*
3. *Of the Medulla Oblongata.*
4. *Of the Medulla Spinalis.*
The Complications.
v. *The Maniæ.*

II.—Diseases of the Organs of Respiration.

i. *The Insidious.*
ii. *The Acute.*
iii. *The Chronic.*
1. *Of the Larynx and Trachea.*
2. *Of the Bronchia.*
3. *Of the Lungs.*
4. *Of the Pleura.*

III.—Diseases of the Heart and Aorta.

i. *Diseases of the substance of the Heart.*
ii. *Diseases of the Valves.*
iii. *Diseases of the Surface.*
iv. *Diseases of the Aorta.*
v. *Nervous Affections.*

IV.—Diseases of the Alimentary Canal.

i. *The Acute Diseases.*
ii. *The Insidious Diseases.*
iii. *The Chronic Diseuses.*
1. *Of the Stomach.*
2. *Of the Small Intestines.*
3. *Of the Colon.*
4. *Of the Rectum.*

V.—Diseases of the Liver, Spleen, and Pancreas.

VI.—Diseases of the Urinary Organs.

1. *Of the Kidney and Ureter.*
2. *Of the Bladder.*
3. *Of the Urethra.*
4. *Of the Prostate.*
5. *Of the Penis.*

VII.—Diseases of the Uterine System.

1. *Of the Uterus.*
2. *Of the Ovarium.*
3. *Of the Mamma.*
4. *Of the Vagina and Pudenda.*

VIII.—Diseases of the Fauces.

1. *Of the Tonsils.*
2. *Of the Velum Palati*
3. *Of the Epiglottis.*
4. *Of the Pharynx and Œsophagus.*

III.—DISEASES OF CERTAIN REGIONS.

I. Diseases of the Neck.

II. Diseases of the Groin.

III. Diseases of the Lumbar Region.

IV. Diseases of the Spine.

V. Diseases of the Hip.

IV.—TOPICAL INFLAMMATORY DISEASES.

I. Diseases having no single seat.

1. *Phlebitis.*
2. *Inflammation of the Absorbents.*
3. *Erysipelas.*
4. *Furunculus.*
5. *Carbunculus.*
6. *Pustule Maligne, &c.*

II. Cutaneous Diseases.

1. Acute.
2. Chronic.

V.—TOPICAL NERVOUS DISEASES.

1. Painful.
2. Spasmodic.
3. Paralytic.

This sketch of the diagnostic arrangement will give a tolerably just view of the extent of the subject of the diagnosis of those diseases which generally fall under the care of the physician, exclusively of surgical diseases, and even of puerperal diseases, and of the diseases peculiar to children.

It will be sufficiently obvious, that an accurate knowledge of diagnosis can only be attained by great attention and assiduous study. This will be still more obvious, when the numerous forms and complications of diseases are clearly

apprehended, as will be seen even on a cursory consideration of the subsequent Essay, on the diagnosis of fevers.

ESSAYS ON DIAGNOSIS.

BY

MARSHALL HALL, M.D. F.R.S. L. & E. &c.

ESSAY II.

ON THE DIAGNOSIS OF FEVERS.

The present Essay will afford an ample exemplification of the manner in which it appears to me that the subject of diagnosis should be studied and treated. I am quite of the opinion of M. Louis—" il n'arrive probablement jamais que des individus qui meurent d'une maladie dont le siége est bien déterminé, n'offrent de lésions que dans l'organe primitivement affecté*." And if this be true generally, it is still more especially so in fevers. In fact, our task of diagnosis is only half performed, when we have ascertained the case to be fever—a special form of fever. The complications may, mediately or immediately, be the cause of death. If they be undistinguished, or undetected, the first part of the diagnosis will be unavailing. In the course of fevers, the early detection of a complication is therefore of the utmost moment. This will appear very obvious on reading the subsequent pages. It will also appear of the greatest importance to cultivate a habit of watching and of renewed examination, for such complications.

Before I proceed to the actual diagnosis of fevers, it may be well to present the reader with such an arrangement of the different kinds of fever, as may conduce to the object we have in view.

OF FEVERS.

I. OF CONTINUED FEVERS.

1. *Common Fever.*
   1. *The Acute Form.*
   2. *The Protracted Form.*
2. *Typhus Fever.*
   1. *The Milder Form.*
   2. *The Severe Form.*
   3. *The Sinking Form.*

II. OF PERIODIC FEVERS.

1. *Intermittent Fever.*
   1. *The Quotidian,*
   2. *The Tertian,*
   3. *The Quartian,*
   4. *The Reduplicated,*
   5. *The Remittent, Forms.*

1. *Common Fever.*

This fever assumes two forms—the acute and the protracted.

I. *The Acute Form.*

I. *The History.* — The morbid affection which I propose to designate the common fever, occurs from fatigue, anxiety, and watching, as in unremitted attendance on the sick; from long exposure to cold or rain, as in taking long journeys, or, as I have often seen, in the labours of the harvest: from extreme errors in diet, &c. It usually comes on immediately after exposure to one of these causes, with chilliness, febrile heat, flushing, &c. Its duration is from ten to twenty days.

II. *The Symptoms* enumerated more fully are the following: — Redness, flushing, and tumidity of the countenance, injection of the conjunctiva, and heat, softness, and tumidity of the skin generally; the tongue is loaded, white, generally moist, swollen, and indented; the breath tainted. There are aching pains, lassitude, and muscular debility; headache; intolerance of light or sound, and, in the erect posture, vertigo or faintishness. The respiration is hurried; the pulse frequent, full, and soft; there are anorexia and constipation.

III. *The Complications* usually seen in this affection are,

I.—1. *Herpes Oris,* and
   2. *Herpetic Sore Throat;*

but besides these, there is occasionally,

II.—1. *Encephalic,*
   2. *Thoracic,* or
   3. *Abdominal Inflammation.*

IV. *The Effects of Remedies (?)*

V. *The state of the System* is such as to admit of the flow of a moderate quantity of blood without syncope.

VI. *The Morbid Anatomy* of the acute form of common fever is unknown; uncomplicated common fever never proving fatal.

II. *The Protracted Form.*

I. *The History.*—This form of common fever comes on more slowly, and after a still more protracted exposure to the causes already enumerated; from disappointment and grief; from want and poverty, &c. Its duration is fre-

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* Recherches de Gastro-Entérite, t. i. p. 419.

quently protracted through six, eight, ten, or even twelve weeks. The patient may then slowly recover, or glide into a state of phthisis.

II. *The Symptoms.*—The countenance, occasionally flushed at first, becomes shrunk, wan, sallow, and tremulous; the general surface shrunk, dry, harsh, and exfoliating; the hands are rough and harsh; frequently a circle of redness and burning is observed extending round the palm; there are muscular tremor and debility, then headache or vertigo, delirium or coma; the pulse becomes frequent and small; the respiration and the articulation are tremulous; the tongue becomes brownish and dry in the centre, or morbidly red, smooth, and dry; there is sometimes vomiting or diarrhœa; the urine usually deposits a copious pinkish sediment.

III. *The Complications* most frequently seen in this form of the common fever, are,

1. *Aphthæ of the mouth and throat.*
2.—1. *Cephalic,* 2. *Thoracic,* and 3. *Abdominal* } *Inflammation.*
3. *Tubercles.*

IV. *The Effects of Remedies (?)*

V. *The State of the System (?)*

VI. *The Morbid Anatomy* of protracted common fever, as distinguished from typhus, is altogether unknown. *Is there ulceration of Peyer's glands?*

II. *Typhus Fever.*

This fever appears under three forms; the mild, the severe, and the sinking.

I. *The History.*—Typhus is sometimes epidemic and sometimes endemic; its causes are contagion (?) malaria (?) the air of crowded cities; deficient and unwholesome food. It usually begins rather insidiously, and gradually assumes one or other of the forms just mentioned and about to be described. It attacks the young chiefly.

1. *The Mild Form.*

I. *The Symptoms.*—The mild form of typhus usually begins with pallor, languor and tremor, muscular debility, chilliness, alternating with febrile heat, and perhaps perspiration. There are headache and vertigo; the pulse is rather frequent; the tongue is whitish, and apt to be dry; there are anorexia, and frequently relaxed bowels. This condition may continue for a fortnight, and gradually subside.

II. *The Complications* consist of

1. *Cephalic,*
2. *Thoracic,* } *Inflammation.*
3. *Abdominal,*

III. *The Morbid Anatomy* of *this* form of typhus, is, I believe, similar to that of typhus in its severe form, varying only in degree.

IV. *The Effects of Remedies.*—There is early syncope on abstracting blood in the erect sitting posture; generally on the flow of about *ten* ounces.

II. *The Severe Form.*

I. *The First Symptoms* of the severe form of typhus are chilliness and febrile heat, early and peculiar muscular debility, and mental depression; the countenance expresses languor and anxiety, and is either pallid or slightly flushed; the articulation, the manner of protruding the tongue and of holding out the hand, and every muscular motion or effort, is attended with a peculiar tremor; there are headache, vertigo in the erect posture, delirium, and somnolency; the temperature of the general surface it only slightly augmented, and there are not unfrequently coolness and moisture. The tongue is whitish, and apt to become brown and dry; there is complete anorexia, and frequently a degree of griping and diarrhœa, and the alvine evacuations are frequently mingled with slight portions of mucus, or blood.

II. *The Subsequent Symptoms* are shrinking of the countenance, with dryness of the lips and sordes over the teeth, and suffusion of the eyes. Every thing in motion and posture denotes extreme muscular and nervous debility; the articulation is indistinct, the hand is held out with difficulty, the tongue is protruded with effort, and is often not drawn in again, from mental torpor; the tremor passes into subsultus, or spasm; the patient falls into the most prone position, unable to support himself even on the side, and is perhaps constantly occupied in picking the bed-clothes. There are delirium, or somnolency, or alternations of these two states, or violent delirium, or deeper stupor. The tongue becomes encrusted, deeply fissured, brown, and excessively dry; the lips are also frequently fissured, and bleed, and there is frequently epistaxis. The skin is various—sometimes cool and moist, sometimes of slightly elevated

temperature, frequently beset with miliaria, especially over the neck and thorax, and with petechiæ more generally. The pulse is slightly frequent, and easily compressible; there is frequently a sonorous rattle, with or without cough or mucous expectoration; there are generally intestinal pain and distention, and diarrhœa, with dark, offensive, flatulent, mucous, bloody, involuntary or unconscious evacuations. The urine is frequently partly retained with distention of the bladder, and partly passed unconsciously.

Comparisons.—Typhus fever is somewhat similar to the following diseases, with which, therefore, it must be carefully compared and contrasted:—

1. *Phlebitis.*
2. *Encephalic disease.*
3. *Delirium tremens.*
4. *Muco-Enteritis* *.

The diagnosis of these affections from typhus fever, will be best effected by carefully comparing and contrasting their characters respectively in every point. This plan will also avoid the necessity for much repetition throughout this work, and form one of the most useful exercises. I shall, in this place, only observe that no disease except typhus *conjoins* chilliness, febrile heat, early vertigo, somnolency or delirium, muscular debility and tremor, the peculiar state of the tongue, of the skin, of the bowels, &c.

Phlebitis is generally traced to a local wound or injury, except it occurs as a puerperal disease. There are a peculiar violence of rigor, anxiety of countenance, appearance of sinking, delirium, frequency of the pulse, hurried respiration, vomiting, diarrhœa, &c.

In encephalic disease there is generally none of the symptoms really peculiar to typhus: the muscular strength is unimpaired; the pulse, the tongue, the general surface, the state of the bowels, are comparatively little affected, and there are more simply the symptoms of local affection of the brain.

Delirium tremens, notwithstanding the two symptoms implied in its designation, is very different from typhus: the tremor is less accompanied by debility, the delirium less attended by stupor; there is, on the contrary, considerable activity and constant wakefulness, the tongue and skin are moist, the breath tainted by some spirituous liquor, and the disease is readily traceable to its cause.

In muco-enteritis there is less febrile action, less debility, and more nausea, vomiting, and diarrhœa. This disease occurs in subjects of every age, frequently from some known cause: there is none of the peculiar state of mind, muscle, tongue, skin, &c. so characteristic of typhus.

III. *The Morbid Anatomy* of typhus fever seems to consist in a diminished cohesion of the particles which constitute the solids and fluids of the body: hence we find,

1st. Softening of the parenchymatous substance of all the organs—the brain, the heart, the liver, the spleen, the kidney, &c.

2dly. Softening, thinness, and ulcerations of the mucous membranes—of the epiglottis, larynx, trachea, pharynx, œsophagus, stomach, intestines, &c.

3dly. Rupture of the textures constituting the skin, and the serous and mucous membranes, and hence petechiæ, vibices, and effusions of blood, of bloody serum, &c.

4thly. Want of cohesion in the blood itself; the coagulum of which is soft, uncupped, and occasionally covered with a buff of the consistency of mere jelly.

5thly, *That change of structure which alone is constant, or nearly so*—is *inflammation and ulceration of Peyer's glands, especially occupying that part of the ileum situated near the cœcum,* but extending over a considerable part of the intestines. This point seems to be established by the labours of Roederer and Wagler*, Prost†, MM. Petit and Serres ‡, M. Louis §, M. Cruveilhier‖,

* The first of these morbid affections, compared with typhus fever, seems clearly to indicate the share of a morbid condition of the *blood* in inducing the symptoms. The second denotes the similar influence of a morbid condition of the blood itself and of its circulation, upon the nervous system.

* De Morbo Mucoso; Goettingæ; 1762.

† Médecine éclairée par l'Observation et l'Ouverture des Corps; Paris, 1804, pp. lv. &c. It is an extraordinary work for the period at which it appeared. The author observes—" M. Bayle m'associa à ses travaux: dès-lors j'espérai du succes."

‡ Traité de la Fievre Entero-Mésentérique. Paris, 1813.

§ Recherches du Gastro-Entérite, 1829. A work which will constitute an ERA in the science of medicine, by introducing numerical precision into its data.

‖ Anatomie Pathologique. Paris, 1830.

Dr. Bright*, Dr. Carswell†, &c. It has been long disputed whether this affection be the *cause*, the *effect*, or a mere *complication* of typhus fever. It cannot, I think, be justly said to be any one of these. It is a part—an almost essential part—of this fever, and appears to bear the same relation to the entire disease which the rash and sore throat do in scarlatina, and the rash and the bronchial affection in rubeola.

These ulcerations appear under various forms, being granular, pustular, fungous, gangrenous, &c.

With these ulcerations are conjoined enlargement and softening of the corresponding mesenteric glands.

IV. The principal *Functional Complications* are,

i. *Encephalic;*
1. *Headache.*
2. *Stupor.*
3. *Delirium.*
4. *Subsultus.*

ii. *Thoracic;*
1. *Cough.*
2. *Expectoration.*
3. *Rattle.*

iii. *Gastric and Intestinal;*
1. *Pain and Sickness.*
2. *Pain and Diarrhœa.*
3. *Melœna.*
4. *Tympanitis.*
5. *Symptoms of Perforation of the Intestine.*

V. The *Structural Complications*‡ are—

1. *Encephalic,* consisting of—1, effusion upon the arachnoid; 2, injection and softening of the cortical and medullary portions of the brain; and, 3, of similar affections of the cerebellum. This complication is slighter in degree, and less frequent in its occurrence, than is supposed.

2. Effusions of lymph, and ulcerations of the epiglottis, the larynx, the trachea, the pharynx, the œsophagus, &c.

3. *Thoracic,* generally slight, and consisting of—1, adhesions, or effusion of bloody serum into the pleura; 2, hepatization, or splenization of the lung; 3, reddish mucus in the bronchia; 4, a livid red colour, thinness, and softening of the heart, denoted generally by irregularity and feebleness of the pulse.

4. *Abdominal;* these are—1, softening, thinness, ulceration, and the mamelated state, of the mucous membrane of the stomach; 2, softening of that of the intestines, with constant ulcerations of the clustered glands of Peyer, and occasional ulcerations of the solitary glands of Brunner; 3, enlargement and softening of the mesenteric glands; 4, softening of the substance of the liver, spleen, kidney, &c.

5. *Perforation of the Intestine.*

The *symptoms* of perforation of the intestine are generally *sudden* pain and tenderness diffused over the abdomen, nausea and vomiting, sunken countenance, smallness and feebleness of the pulse, cold perspirations, with pallor over the whole surface, and rapid failure and sinking of the powers of life.

6. The *integuments* covering the sacrum are apt to ulcerate and slough from pressure, and those of parts covered with blisters, from irritation, in a degree which becomes somewhat diagnostic. There is also occasionally erysipelas.

6. *The Effects of Remedies;* and,

7. *The State of the System.*—There is, comparatively with health, and still more, comparatively with inflammation, little tolerance of loss of blood; syncope is early produced on opening a vein in the erect sitting posture.

III. *The Sinking Form.*

1. *The Symptoms.*—In the sinking form of typhus, or that designated the congestive, a form little seen in hospitals, there is early coldness of the face and general surface, with a feeble pulse, stupor, deep breathing, extreme debility of the muscular system, so that articulation and all attempts to move are abortive; the eye is sunken, the voice husky, the evacuations perhaps involuntary.

This form of typhus is noticed in this place, in order that nothing practi-

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* Reports of Medical Cases. London, 1827.

† By the liberality of Dr. Carswell, I, as well as many others, have repeatedly seen his incomparable drawings, amounting nearly to *two thousand*; and I have as repeatedly contemplated this gentleman's labours with unmingled admiration. They will long be the ornament of the London University. I rejoice to know that Dr. Carswell is at length engaged in preparing Elements of Morbid Anatomy, with plates, for publication: this work must infinitely surpass every thing of the kind published in this kingdom.

‡ Throughout the whole of these essays I understand by the morbid anatomy, such changes as are *essential* to the disease; and by structural complications, such as are only of more or less frequent occurrence.

cally useful may be omitted, and that the student may be aware of a form of disease not of frequent occurrence. It can scarcely be mistaken for any other disease.

2. *The Morbid Anatomy;* and,

3. *The Effects of Remedies* appear to be unknown, or, rather, involved in hypothesis.

II. Of Periodic Fevers.

1. *Intermittent Fever.*

I. *The History.*—The *Causes* of intermittent fever in its first and subsequent attacks, are the miasmata of marshes, stagnant water, and humid localities, and the north-easterly winds. The disappearance of intermittent fevers from London and its neighbourhood, and from other localities in which they formerly prevailed, is ascribed by Dr. Willan, and by Sir Gilbert Blane, to the practice of draining, and other improvements in agriculture. The *Course* is marked by successive distinct, cold, hot, and sweating stages; and these are recurrent every second, or every third day, or at other intervals, giving origin to the designations, quotidian, tertian, quartan, &c.

1. The *Quotidian* has an interval of twenty-four hours, a paroxysm of moderate severity, but of long duration, beginning with a slight cold stage, generally in the morning. It is apt to assume the remittent form. It occurs principally during the spring.

2. The *Tertian* has an interval of forty-eight hours, a severer cold stage, a shorter paroxysm, recurrent generally about noon, and followed by much perspiration. This is the most frequent form of intermittent, and is observed to be milder in spring than in autumn.

3. The *Quartan* has an interval of seventy-two hours, a short paroxysm, and a long intermission. The paroysms usually occur after noon, with a long and severe cold stage, a gentle hot stage, and slight perspiration. The quartan intermittent fever occurs chiefly in autumn, is apt to prove obstinate, without having any tendency to assume the remittent form.

4. Intermittent fever sometimes assumes the *Reduplicated*, or merely *Remittent* forms; and sometimes every kind of *irregularity* in form, and in the intensity of its paroxysms, or of their different stages.

The recurrence of the paroxysm may not be always so accurate in point of time and hour in different cases, as I have mentioned; yet attention to this point, in the same case, is a very important means of diagnosis in obscure cases. The rigors in suppuration and in phthisis have not such sustained regularity of return.

II. *The Symptoms.*—The paroxysms of intermittent fever begin with yawning and languor, and a sense of creeping along the back; the patient then shivers with cold; the countenance and the general surface are pale, shrunk, and cold; there is that state of the skin termed 'cutis anserina,' and the nails assume a livid hue; the respiration is sibilant; the pulse is small and frequent, and perhaps irregular; there are anorexia and thirst; the tongue is dry and clammy; the urine is limpid.

The cold stage gradually subsides, and the countenance becomes flushed and tumid, and the eyes injected, whilst the general surface is turgid, hot, smooth, and dry; there are frequently acute pains of the head, throbbing of the temporal arteries, intolerance of light and sound, and delirium; the respiration is frequent, but less anxious; the pulse strong, full, and frequent; there are urgent thirst, with continued dryness of the tongue; the urine becomes high coloured.

In the sweating stage the countenance assumes nearly its natural appearance; the skin loses its tumidity and heat, and becomes covered with perspiration. The head is relieved, and sleep often supervenes; the respiration becomes free, the pulse nearly natural; the urine deposits a degree of sediment*.

The paroxysm over, the patient is left somewhat pale and languid, and there are headache and anorexia. In the commencement of intermittent fever, the apyrexia is, however, sometimes almost free from indisposition.

III. *The Complications* of intermittent fever are frequently, like the fever itself, periodic,—intermittent, or remittent; and sometimes, without fever, there are similar paroxysms and intermissions, or remissions, of local affections.

The principal of these are—

1. *Hemicrania.*

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* The observations made upon the urine by the older writers on Intermittents, are confirmed by M. Andral, in the Clinique Médicale, Ed. 1, t. i. p. 479.

2. *Pain of the Eye-brow.*
3. *Thoracic Pain.*
4. *Splenic Pain and Tenderness.*
5. *Pain of the Testis.*
6. *Other Topical Pains.*

These affections sometimes assume a more aggravated form, and there are—

1. *Headache, Delirium, Coma, or Amaurosis.*
2. *Thoracic Pain, Cough, Asthma, or Syncope.*
3. *Colic, Cholera, or Diarrhœa.*

These local affections may precede, accompany, or follow intermittent fever; or they may exist variously in the intermittent or remittent form, independently of febrile symptoms. They will be particularly noticed hereafter.

The principal permanent complications are—

1. *Enlargement of the Spleen.*
2. *Anasarca.*

IV. *The Morbid Anatomy* of intermittent fever seems really to be little known. The spleen is the organ chiefly and most frequently affected; it becomes enlarged. This enlargement is discovered during life by recurrent pain, dulness of sound on percussion of the false ribs of the left side, and, at length, on examining the region of the spleen by pressure. The spleen may remain enlarged, ascend, or descend, and constitute a mode of ascertaining the existence formerly of intermittent fever, without materially affecting the health.

V. *The Effects of Remedies.*—The influence of the quinine in intermittent fever, pains, &c. is so marked, as to be at once diagnostic of the disease, and suggested for all cases of distinctly intermittent character.

CROTON OIL IN CHOLERA—RECOVERY UNDER THE SALINE TREATMENT.

To the Editor of the London Medical Gazette.

Wolverhampton, Aug. 27, 1832.

Sir,

Holding the office of Secretary to the Board of Health in this town, I think it a duty I owe to the public to communicate any plan of treatment or remedy which, from my own experience and that of others, I consider to have been useful in cases of cholera. I beg to premise that the disease has shewn itself in this town and neighbourhood to be of a highly contagious nature; almost every case having been easily traced to that cause, and the medical men and nurses having been particularly affected.

The remedy which I wish most particularly to recommend to further trials is the croton oil, in the *perfectly collapsed stage*, when the pulse is as it were extinct. In most of the cases where I have used it, the effect has been to arrest, for a time, the vomiting and purging, and, after a few hours, to produce the dark-green stools as described by Dr. Teggart, in his lately published letter. I was led to try the remedy in my two first cases (a woman and her daughter, both collapsed, but the pulse of the former just perceptible), by the patients' refusing all medicine. The mother died eight hours afterwards, but without a return of the vomiting and purging; the child passed the green stools in a few hours, and is still in the hospital slowly recovering. The next case was a child of four years—cold, blue, and pulseless, but still able to walk (a very common circumstance, I find); to whom I gave two drops rubbed up with sugar and two oz. of water (a teaspoonful being taken every half hour), with the effect of rendering the little sufferer warm, flushed, and bright-eyed, after the usual evacuations, in about eight hours. Other cases treated in the collapsed stage are still under cure; others have died. I can only speak from my own experience as to the stage in which the remedy has been used in this township; but Dr. Macann, of the Central Board (now staying here to direct our proceedings), informs me that experiments are now being made with the croton oil in other places. I do not mean to claim any specific power for croton oil in cholera, but I think no suggestion should be disregarded when we are publicly told, by one of the most distinguished physicians of this country, that "a person in the blue stage will have nearly the same chance of recovery, if wrapped up in blankets and laid in bed, as with all the remedies hitherto proposed administered to him *."

* See a late number of the Lancet—Dr. Elliotson's Lecture.

With respect to the saline treatment, I may state that a boy in decided collape, six of whose family have died in this town, of cholera, in the last week, recovered solely under that plan of treatment; the powders, as prescribed by Dr. Stevens, being diligently administered every quarter of an hour, for some hours. I may also state, that when the muriate exceeds the carbonate of soda, as in some of the powders recommended, I find they almost always increase the vomiting and thirst; both which are rather allayed by these containing sodæ mur. ℈j., soda carb. ʒss., chlor. pot. gr. vii. The former I now always use, allowing no other drink to the patient.

The disease, I am happy to state, is almost confined to the outskirts of the town, among the colliers and ironfounders; except in one filthy court, in which the family mentioned above resided.

That this letter may be instrumental in saving even one life, is the hearty desire of, sir,

Your obedient servant,

T. Ogier Ward, M.B. Oxon.

COMPARISON OF VARIOUS MODES OF TREATMENT IN CHOLERA.—EFFICACY OF SALINES.

To the Editor of the London Medical Gazette.

Convict Hospital Ship,
Woolwich, August 27, 1832.

Sir,

The profession is deeply indebted to you for having opened the pages of your valuable journal for the discussion of the merits of the saline mode of treatment in cholera.

In prosecuting an inquiry of such importance to the welfare of the community, and of such deep interest to the profession, it cannot be too generally lamented, that decisive and final opinions have been hastily promulgated, by some extolling the practice as in almost every case infallible, and by others declaring it totally inert and injurious.

Considering that its employment has been hitherto by far too limited to warrant its general assumption or rejection, the object of this paper is to add a few facts to those already published, and thereby promote the laudable purpose you have attempted.

Cholera appeared in this establishment about the middle of March, but occurred in isolated cases until the 5th and 6th of May, when its irruption became general and the cases numerous.

Of about 800 individuals, of whom this establishment was then composed, a very great majority were affected with the premonitory symptoms. Many facilities being offered, the utmost vigilance was employed to secure their early detection. All the men were medically inspected three times daily: if any individual had three evacuations while at labour, he was placed under observation, and his dejections examined. By this means the cases of sudden, profuse, and serous diarrhœa, amounted for some time to at least 30 daily. Some of these had vomiting, faintness, and cramps. Most of them were attacked in the morning, and were treated by a single dose of 5 or 6 grains of calomel combined with 1, 2, or 3 of opium, followed by two ounces of the saline mixture every hour; and, if the purging continued, a starch enema, with 2 drachms of the carbonate of soda, 4 of common salt, and 1, 2, or 3 of the tincture of opium; with gruel for diet. Under this regimen, with subsequent small doses of rhubarb and castor oil, most of these persons recovered, and were never admitted or reported as cholera.

Satisfied that as much was effected as the most unremitting and assiduous *nursing*, and the steady employment of remedies, could accomplish, the following table and remarks are offered, as affording the results, carefully and impartially collected.

Treatment.	Cases.	Deaths.	Recov.
By Bleeding and Stimulants.	13	5	8
By Stimulants only	4	1	3
By Bleeding and Salines.....	56	11	45
By Salines only	65	9	56
Hospital Patients.....	10	10	0
Total	148	36	112

The treatment described as "Bleeding and Stimulants," consisted of venesection to the amount of 6 or 8 ounces; the administration of salt and mustard emetics; of hot salt water enemata; hot air bath; bottles of hot water and mustard cataplasms externally; and of brandy, ammonia, and Cayenne pepper, in liberal and oft-repeated doses internally. No opium was given, as these

were the earliest cases, and we were desirous to observe the disease unobscured by its effects. Thirteen were subjected to this treatment, five of whom were lost in periods of 5, 9, 10, 11, and 24 hours from admission; and of the eight who recovered, three were moderately and five severely collapsed. Only one of these had the insensible purging, said to be a very fatal symptom; and he appeared to derive most benefit from the mustard poultices, which were extensively applied for several hours.

The cases treated by "Stimulants only," were seen early. Two had severe premonitory symptoms; one was moderately collapsed; and one died in 72 hours of muco-enteritis.

Until reading the letter of Mr. Wakefield, published in the Medical Gazette, on the efficacy of the saline mode of practice, the principal dependence had been placed, in collapsed cases, upon cautious bloodletting, salt and mustard emetics, and mustard poultices. The hot bath, although always at command, had been found in many cases impracticable; and the hot air bath decidedly injurious. Having prepared a mixture, every two ounces of which contained one of the powders used at Cold-Bath-Fields, with a small quantity of brandy*, all future cases were treated (after the above preliminary measures had been practised) to the effect of this dose, repeated every half hour, until reaction was accomplished, when small doses of calomel and opium were given, every two hours, till the gums were slightly sore, and healthy secretions established; after which the sulphate of quinine and mild aperients usually completed the cure. Fifty-six were thus treated, of whom eleven died, at the following periods after the commencement of the treatment:—

In six hours . . .	1 (a relapse.)†
— twelve do. . .	5 (1 a relapse.)
— fifteen do. . .	1
— eighteen do. .	2
— twenty-six do.	1
— four days . . .	1 (a relapse.)
	11

Of the recoveries, seven had severe premonitory symptoms; fourteen were moderately, twenty-five severely collapsed; fifteen had insensible serous purging; and in several, the pulse at the wrist and the secretion of urine were absent for two or three days. A few had smart inflammatory affections of the abdominal viscera, and one or two had partial dropsy during their recovery.

Under the treatment described as "Salines only," are included all those cases in which, during collapse, the following constituted the whole treatment:—A salt-water emetic, followed by a dose of the saline mixture every quarter and half hour; by an effervescing draught, with an excess of soda, every hour; by the hourly administration of an enema, composed of starch, carbonate of soda, and common salt, and occasionally the Tr. Opii; by saline beverage, consisting of barley-water, given *ad libitum*, to every pint of which two drachms of carbonate of soda had been added; and by mustard poultices.

Of sixty-five cases so treated, nine were fatal, the period of decease being respectively,

In nine hours from admission .	4
— twelve hours	2
— twenty-four do.	1
— thirty-six do.	2
	9

In the case fatal in twenty-four hours, there was partial reaction and relapse; and the patient who died in thirty-six hours was admitted early, and treated by saline injection into the veins.

Of the fifty-six recoveries, sixteen had premonitory symptoms; ten were moderately, and thirty severely, collapsed; seventeen had insensible purging.

Under every variety of treatment, the vomiting and hiccup continued occasionally obstinate during recovery, and a few had partial dropsy; but it was only requisite to employ leeches in four cases; venesection in one (a month after, for anasarca); and of the whole number of cases (148) only two were fatal after re-action: one of these was an hospital patient upwards of 70 years of age, who was treated by salines only, lived a week, and died of apoplexy; and the other the case of enteritic inflammation already mentioned.

The hospital cases were such as

* Although in Dr. Stevens's practice stimulants were entirely prohibited, a small proportion of brandy was added to this mixture, because most of the patients had previously led irregular lives, and been long accustomed to smoking and dram-drinking.

† These relapses were all previously recovered from every appearance of danger, but are included as recoveries.

would have been fatal under any acute disease, being patients far advanced in phthisis, fever, and in one or two the arteries were ossified from extreme old age, and death arose from congestion during re-action.

Leaving these facts without comment for the present, I beg to subscribe myself, sir,

Your most obedient servant,
P. Bossey, M.R.C.S.

REPORT
OF
CHOLERA AT WARRINGTON.
SUPERIORITY OF THE SALINE TREATMENT.

(From a Correspondent.)*

We have been favoured with the perusal of a manuscript containing a list of cases of cholera, which have occurred in and in the neighbourhood of Bank-Street, Warrington, which has been the principal seat of the disease in the town. We may add, that the manuscript has been drawn up by a gentleman of great respectability, and one of the oldest practitioners in that place.

There had been some suspicious cases in Warrington previous to the 18th of June, but the inhabitants generally continued healthy until that date. On the evening of that day, however, a woman named Elizabeth Mills, aged about 30, was attacked in the market-place with evident symptoms of cholera. This woman had walked that morning from Manchester, where her husband had been attacked with cholera, on the evening of the 16th; he died on the 17th, at five o'clock P.M., and was buried early on the morning of the 18th. The widow was attacked about eight o'clock in the evening, and was taken to a house appointed for the purpose of lodging such persons as are conveyed by passport through the town. She recovered after a severe illness; but her child, whom she had carried from Manchester, and who had been taken to the workhouse, was seized on the 20th, and died on the 23d.

On the same day that this child died, Philip Inghram, a travelling vender of medicines, arrived from Manchester, with his wife and family. They went to reside in Bank-Street. The following morning one of the children was attacked with cholera, and died on the 28th, of the consecutive fever. On the 26th Inghram himself was attacked: at noon he fell suddenly into a state of collapse, and died at midnight.

The family of Inghram was lodged in a house belonging to a Mr. Lawless. This house was remarkable for its cleanliness; but on the 24th Mr. Lawless was taken ill, and others of his family were subsequently attacked. From this house the disease spread to the neighbourhood; and from that time to the present the disease has continued its ravages, and still exists in several parts of the town. We think it the less necessary, however, to follow up the subject of its introduction, as we observe, by a small tract which Dr. Kendrick has published on cholera, that Mr. Glazebrook, the Secretary to the Warrington Board, is about to draw up an account of the rise and progress of cholera in that town.

The manuscript to which we have referred contains the history of one hundred and eight cases, in and in the neighbourhood of Bank-Street. Various plans of treatment were tried. The following is an outline of the result, up to the time that the statement is made out.

	Cases.	Deaths.	Recoveries.
Where the patients obstinately refused to take any medicine whatever	4	4	0
Where the patients were either very old previous to the attack, or neglected, and seen by the physicians so late, that no treatment was of any use	30	30	0
Treated by Morrison's purging pills	3	3	0
Treated by bleeding, followed up chiefly by the antiphlogistic treatment, as recommended by Broussais	13	13	0
Treated with opium and stimulants	28	28	0
Treated with bleeding, and the saline treatment ...	7	0	7
Treated by the saline remedies, without bleeding...	23	2	21
	108	80	28

* The names of the parties have been sent to us.—Ed. Gaz.

In one of the very worst cases which was under the saline treatment, where collapse came on almost instantaneously, a piece of lint, about four inches square, was immersed in a liquor ammoniæ; this was applied to the skin. When the cuticle had separated, it was taken off, and the oxymuriate of mercury was applied to the abraded surface. The saline powders were given regularly; reaction came on. In ten hours the patient perceived the mercurial taste in the mouth, and recovered, after having been several hours in a state of complete collapse.

TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

It is much to be regretted, that persons holding a respectable rank in our profession should condescend to give vent to angry feelings in a scientific journal, and still more so that the cause of truth should be injured, by being involved in a personal dispute. With every allowance for the excited feelings of Dr. Stevens, arising from the genuineness of his cases of cholera being called in question, he should not have wandered from that point, nor allowed himself the use of such ungnarded expressions as unsuccessful and unscientific, applied to opium and calomel. If he could, or would, lay aside for a moment his prejudices in favour of the exclusive use of his salines, and examine the numerous cases in which success has attended the exhibition of *small doses* of opium, in conjunction with calomel, and followed by mild purgatives, under the direction of practitioners, to say the least, as capable of judging as himself, he would be convinced that these medicines were entitled to some consideration; and farther, if the evidence be sufficient to induce him to give this plan a trial, he will find that, on the one hand, as a progressive deterioration in the quality of the alvine discharges is one of the great characteristics of cholera, which may be said to date its commencement from the first deviation of these discharges from their natural state; so, on the other hand, he may observe that the return to health may be dated from the moment that the calomel begins to act upon the secretions. The dejections become less frequent, and the increase in their colour and consistence, which is to be regarded as the index of the operation of the calomel, is as progressive towards recovery, and attended with an abatement of all the other symptoms. He will find, moreover, that, *provided brandy, ammonia, mustard emetics, &c. be abstained from,* there will scarcely be a symptom worthy of the name of febrile, attendant upon re-action; and the average number of days that patients subjected to this plan of treatment require attention, will be less than six. Now this appears to be the very same result which attends his own practice, in cases seen soon after the commencement of the attack, and is clearly referrible to the same principles; consequently, such a plan is neither unsuccessful, nor unscientific; and we must either conclude, that Dr. Stevens has never observed its effects, or does not understand the principles upon which his own practice is founded sufficiently to enable him to see how other medicines, capable of producing the same results, may be equally, and even *more efficacious.* This I say advisedly, because by how much calomel has a greater power of exciting secretion than salines, by just so much will it be successful up to a later period of the disease. We must still, therefore, under these circumstances, be content to place our confidence in opium and calomel, until some substances shall be found *to produce the same effect more speedily.* The same principle appears in the readiness with which this disease is relieved by purgatives in children.

There is one observation of Dr. Stevens in which we must all agree. He says, "Every day I have additional reason to believe, that the mortality will be greatly diminished when the principle of the practice comes to be more generally understood, and where it is tried by practitioners who know how to vary the treatment according to circumstances;" for until it is, we cannot deny that our profession justly deserves the ridicule which necessarily attaches to it, on account of so many opposite medicines having each at dif-

ferent times been brought forward as the one thing needful.

I have the honour to be, sir,
Your obedient servant,
J. W. Earle.

14, Old Broad-Street,
Sept. 1, 1832.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

The Effects of Arts, Trades, and Professions; and of Civic States and Habits of Living, on Health and Longevity, with Suggestions for the removal of many of the agents which produce Disease, and shorten the Duration of Life. By C. Turner Thackrah, Esq. Second Edition.

We have before admired the great industry and ability displayed in this work, and we continue to regard it as a most valuable contribution to political medicine. The author informs us that in the present edition he has given the results of his examination of about 120 additional employments, and that he is still busily engaged in a field in which much remains to be done—in which, indeed, little or nothing had been done before he took the subject in hand. The work, as we presume most of our readers are aware, is of an essentially practical nature, and abounds with facts which supply ample material for grave reflection. The remarks which Mr. Thackrah has supplied are generally brief, but pregnant; any thing rather than book-making seems to have been his object. It will require much preaching and persuasion to abolish the evils, especially in manufacturing towns, of which he so pithily complains; but if any thing effectual can be done in that way, it will be, in the first instance, by the method adopted by the author; that is, by the publication of a book full of curious matter of fact, and suited to the perusal of every class of readers.

There is an amusing and very sensible passage which just presents itself to us for extraction. Mr. Thackrah, it will be seen, while treating of the modes of life peculiar to artists, tradesmen, and professional persons, has not neglected the calling of another considerable class, who deserve to be noticed *per se*—we mean the *bons vivans*. Our author is evidently not an abstinence-society man; he proceeds to say:—

"The proper culture of the stomach is certainly not only to be allowed, but enjoined. A celebrated Greek (Aretæus) heads his chapter on gastric disease with the remark, that the stomach presides over our pleasures and our pains. And assuredly no practitioner of medicine who closely observes his patients—no man, in fact, who closely observes himself—can hesitate in frequently ascribing the source of irritability, anger, and despondency, or, on the other hand, of cheerfulness, hope, and benevolence, to the state of the digestive organs. The kinds and quantity of food, therefore, and the modes of its preparation, afford a study by no means unworthy of science; and *works like Kitchener's deserve a place in every library*. But assuredly the art is carried by many to a lamentable extreme. Cookery becomes the minister of gluttony. The palate is stimulated to excess; the stomach is consequently gorged; its powers are weakened; and venous congestion of the abdomen, with all its attendant evils, is established. The disposition and power for muscular exertion are greatly reduced; the brain, and the whole nervous and vascular systems, suffer from the improper quantity and quality of the circulating blood."

The following little notice of the primitive simplicity of our forefathers may not be uninteresting:—

"The evil of refined and excessive eating is not new. Livy complained that in his day cookery had become an art—a noble science; that cooks were gentlemen—*venter Deus*. Another ancient remarks of the Rhodians, that they built houses as if they were immortal, but they feasted as if they meant to live but a little while. Seneca justly observes, *Multos morbos multa fercula fecerunt;* and again, *Innumerabilis esse morbos miraris? Coquos numera!* No medical man of the present day could have given a more dismal picture of the effects of excess, than the 95th epistle of this philosopher. But without referring further to the faults of other nations, or to the monstrous excesses of individuals, like Soliman the Calif, and Maximus the Roman Empe-

ror, I would remark on the character of Britons. The English, it seems, have always been remarkable for full living. Chaucer, in his Persone's Tale, arraigns their 'divers meats and drinks, bakemeats and dishmeats, brenning (burning) of wild-fire.' *Ampliter viventes*, says Polydore Vergil, *in prandiis et in cœnis*. 'Banquets, rere-suppers, and juiceries betwixt meals,' are mentioned by Bishop Fischer as the common excesses of his countrymen. And in Scotland, it appears from Holinshed, that a law was made in 1433 'for the restraint of superfluous diet.' We afterwards find Jeremy Taylor inveighing against the luxurious tables of his day:—'Strange that for the stomach, which is scarce a span long, there should be provided so many furnaces and ovens, huge fires, and an army of cooks, cellars swimming with wine, and granaries sweating with corn; and that into one belly should enter the vintage of many nations, the spoils of distant provinces, and the shell-fishes of several seas!'"

A Letter addressed to the Central Board of Health, written with the view of establishing rational principles for the Treatment of Cholera; and shewing the Danger of the Mode of Practice at present generally followed. By John George French, Surgeon to St. James's Cholera Hospital, and Resident Surgeon to the Infirmary of St. James, Westminster.

The circumstance of our having published, last week, two interesting papers on the treatment of cholera by means of cold water, induces us to take notice of the pamphlet before us. The opinion of the author may be very shortly explained. He thinks "that the alimentary canal becomes subject to a process which altogether supersedes digestion, and by this process a large quantity of fluid is produced as an excretion, which rapidly diminishes the bulk of the blood: with the mechanism of this production, we are as yet unacquainted."

And again:

"That in some instances the disease continues its progress till the death of the patient; but, in the very large majority of cases, when left to nature, the disease ceases when it has produced a state of collapse, varying in intensity."

Mr. French is farther of opinion (in which we entirely concur with him), that brandy and opium, as hitherto too often administered, do harm; and that, on the contrary, the extreme thirst which attends the collapse is an indication on the part of nature for fluids—especially cold water, which is the most grateful to the patient. The following short paragraph contains what farther of Mr. French's views we deem it necessary to give.

"As long as there is thirst, with a sensible diminution of the evacuation of the bowels, there is every rational hope that reaction will take place by the efforts of nature, if no mischievous interference be permitted. The situation of the patient is now too critical, while such a process as I have described is going on, to hazard the chance effect of medicines which the speculations of the ingenious may suggest; though I may take occasion to observe, that Dr. Stevens's saline mixture is harmless at the least, if not beneficial; the thirst may be by this probably increased; and the kidneys may be probably stimulated: but if it should be observed that the respiration becomes exceedingly hurried, and the alæ nasi flapping, and pain and considerable distress is experienced in the chest, the action of an emetic, and the local stimulus of a sinapism, may be extremely useful. The *feelings of the patient* with regard to temperature must be attended to. It is unwise to add to the oppression of the nervous system, by subjecting him to a disagreeable degree of heat, which is so commonly done."

The most remarkable part of the brochure before us, is the extremely limited data with which the author furnishes us for opinions so much opposed to those generally received: only three cases are given in illustration, and of these one only was treated by the cold water as the sole internal remedy; the first patient having been put upon salines (muriat. and carb. of soda and potass) and calomel, and the second, till after re-action had commenced having refused every thing, drink included, owing to a determination to take no medicine. The third had sinapisms to the spine and stomach, hot water to the feet, and cold water for drink, *ad libitum*. All three recovered.

The coincidence of this paper with that of Dr. Shute is remarkable, and makes it desirable that farther trial should immediately be given to the abandonment of opium and stimulants.

It would be very acceptable if Mr. French would favour the profession with tabular or other very abbreviated views of his cases—for we cannot suppose that the doctrines adduced, and which are really important, have been given to the public without extensive means of observation.

M. BROUSSAIS' ACCOUNT OF HIS OWN DOCTRINES;

In a Letter to the Académie des Sciences, August 6, 1832.

Gentlemen,

A physician who has passed the best part of his life in labouring for the advancement of the science which he cultivates, formed long ago the design of laying before the Académie an account of his labours, and of the changes which he has seen effected in the healing art.

It was blameable, no doubt, to have deferred till now the accomplishment of that design; nor can any better apology be offered for the delay than the desire which the author had of rendering those propositions more convincing which he wished to infer from his observations, and the changes of which he is about to speak. He comes, in short, gentlemen, to request a brief audience; for he has felt the want of your support in seconding his efforts, and those of his co-operators, in a work which he believes to be useful to society. Anxious not to waste the time which you so usefully devote to the progress of knowledge, he will at once broach the subject upon which he is desirous of your consideration.

Medicine, as every one knows, is the science which teaches us to recognize and to treat the diseases of living beings; but we shall confine our remarks to those of the human species. Medical men, then, are, as it has been said, the ministers of nature; men devoted to acts of benevolence and mercy; men whose great object is the doing good to their fellow-creatures. Nothing, consequently, is more natural than that they should be ever desirous of the means.

While yet a youth, filled with these ideas, the individual who has the honour to address you felt himself (even from the year 1804) unpleasantly affected, from his imperfect ability in the military hospitals, to perform the delicate duty which the government had imposed on his conscience. Was it his fault that he was not more successful in the practice of his profession, or the fault of the system in which he had been brought up? He worked incessantly for five years, and in 1809 appeared his *Histoire des Phlegmasies Chroniques.*

Remote from Paris, where, indeed, he was little known, and a stranger to all manner of intrigue, he had no opportunity of setting forward this work at the time of the *concours* for the decennial prizes, in 1811. He obtained, however, an honourable notice on the occasion — an encouragement which had a powerful effect in supporting his zeal and redoubling his exertions.

The History of Chronic Inflammations is a work wholly experimental. At the time it was written those diseases were scarcely known. Pugol de Castres (of whom scarcely any one dreamt, but who was speedily exhumed upon the appearance of this work) had treated only of suppurations of the visceral cavities: those slow and insidious inflammations which have their seat in the membranous tissues in the chest and abdomen were completely overlooked by the physicians of the age. Pinel had given them no place in his Nosography—there was nothing in lieu of them but certain *organic derangements.* Corvisart, who so eminently possessed the art of exploring disease in the functions, had arrived at no correct notion of them. He knew how to determine the seat of a tumor in the viscera, but he could give no account of its nature, if it was not connected with pulmonary consumption, or malady of the heart; he paid no attention to what was commonly called organic derangement, while he saw no cause for the slow but gradual decay of the patient, except in a state of debility or cachexy—terms which conveyed to the mind nothing, but which at the same time, unfortunately, supplied wrong indications for the treatment.

The History of Inflammations threw light upon all those points hitherto obscure; it shewed how inflammation was principally instrumental in the origin of those adventitious masses which developed themselves among the viscera; it exhibited how, under another form, the same inflammation invaded insensibly the tissue of their membranes, and brought on that state of emaciation which was usually attributed to the feebleness of the solids and the depravation of the liquids; it did more; it proved that those weaknesses and depravations were often curable; it determined the period of their being so, and the mode of subduing them.

From that moment, science had a new face: *organic diseases,* so vaguely understood before, had now a sense that every medical person could comprehend. The great business, then, was to palliate their direful effects—to prevent them, when their germs became visible, and practice assumed a rational form in respect to this important section of our physical ailments.

The History of Inflammation was, however, merely the first step towards that reform of which practical medicine stood so much in need. The class of fevers was in no more satisfactory condition to the under-

standings of men of sense, than those of the cachexies had been. Continued fevers presented themselves in general to practitioners under two very different aspects; the one they attributed to the inflammation of some particular organ; the other, which they called *essential*, was deemed independent of all local affection. The cause of the first was found in inflammation of the brain, and went by the name of *encephalitic*; or in that of the lungs, or of the abdomen, with a phlegmonous form—that is to say, with a pulsating tumor and burning heat; or in that of the peritoneum; or, finally, in any or all those inflammations which their situation at the surface rendered appreciable to the sight and touch of the practitioner. I say that all these febrile movements accompanying palpable inflammations were apprehended as they should have been—being attributed to their true cause. But *essential* fevers were supposed to have no *locale:* nobody knew to what to refer them; and in this state of ignorance it was attempted to characterize them, either after their predominant symptoms, or other data still more obscure. Was the serosity of the bile predominant, they were called *bilious fevers:* was there apparently a superabundance of mucous in the fæcal matter, they were called *mucous*, or *pituital* fevers: was the heat remarkable, they were called *hot* fevers: the body cold—*cold* fevers: and if the patients at the same time complained of a raging heat within, they gave them another name.

When the powers were sunk in fevers, they were called *asthenic* or *adynamic:* if the body exhaled a repulsive fœtid odour, they were styled *putrid fevers*, although many physicians of the best sense rejected with contempt such a denomination, aware that putridity was incompatible with life. Other fevers were *nervous* or *ataxic*, and others called after the country or place where they most prevailed: thus we have had the camp, the prison, the hospital, the Hungarian, the Low country, fever: we have had, in short, from the skin affections also, the fevers denominated the petechial, the miliary, the nettle-rash, &c. &c.

In some instances the name and character were derived from a supposed unknown and perfidious agent, which was ever deceiving the vigilance of the physician, and throwing him out of his calculations. Hence the *insidious* form. And when a better name could not be found to describe the danger of the complaint, we had the *pernicious* fever.

Let us not be misunderstood as attempting to depreciate the labours of those who have given us these results, for of such are the materials employed by modern writers for the structure of the evidence of science; and our gratitude and veneration are due to the laborious men who have supplied us with them. Our object is simply to shew in a brief sketch the progress of the human mind in the acquisition of medical knowledge, and to describe the actual state of our art at the period in question. But we may offer our opinion.

And what, gentlemen, can you see of philosophy in the proceedings of the old school regarding these supposed essential fevers? Can you see in them a *science?* Alas! there is nothing there but chaos and confusion; nothing but an exhaustless source of controversy, not merely on the nature, but, what is far more serious, on the treatment of disease. In fact, any thing like agreement was rare; for in one and the same disorder, some would take their indications from the biliary or mucous secretion, while others would have recourse to the nervous system, or the debility, or putridity, of the patient.

Such was the state of medical science when, in 1816, was published the first edition of the *Examen des Doctrines Medicales.* This work, the fruit of a more extended experience, went far beyond its predecessor the *Histoire:* it inveighed strongly against the vagueness, the contradictoriness, and the insufficiency of the prevailing doctrines. It preached up the necessity of following another method in appreciating the symptoms of disease, both chronic and acute: it counselled not to proceed any longer in medicine by the formation of groupes of symptoms. It shewed, in short, that nothing was less reasonable than to call a groupe of ten or twelve symptoms the *cause* of the material alterations which were found in the organs after death.

The work in question proposed to consider fevers, as inflammations are considered; to determine the seat of the latent irritation which gives rise to the febrile state—a state which is itself but an excess of irritation, caused principally by the heart; it traced fever to that latent local irritation in the viscera, as its cause, and suggested to take for the basis of the proper treatment, the influence of external agents on the *mobile* of the febrile condition, and consequently on the fever itself.

This method was eminently simple, unique, and consequently philosophical. Its novelty could not but raise a violent storm against it; but, entrenched behind an imposing mass of facts, it stood its ground, and the history of the latter periods of French medicine can give a good account of its success.

One very remarkable circumstance deserves to be mentioned. In 1812 began that work which was presented to the world as a monument of French medical science—the great *Dictionnaire des Sciences Médicales.* Up to 1817 it bore the uniform colouring of Pinel's doctrine. From that time forth it became mottled with a mixture of the principles set forth in the first *Examen.* Scarcely

is it finished, when forth comes the *Dictionnaire Abrégé*, in which those principles predominate to such a degree, that they absolutely make up the greater part of its bulk. In the great Dictionary, fevers are still *essential*; they are but symptomatic in the Dictionary abridged; and the Dictionary in 18 volumes, which comes next in order, reduces those fevers considerably, and, moreover, every where displays the banner of the doctrine which the *Examen* advocates.

[M. Broussais then proceeds to notice other works of his, and among them his recent tract on the Cholera; after which he goes on:—]

But, gentlemen, it is time to give you a summary and comprehensive view of the method we follow in the distinction and treatment of diseases, which we shall now attempt to do in the most succinct manner we are able.

This method has for its guides two phenomena which never abandon it at the bedside of the sick—motion and sensation. In fact, as long as the individual is alive, his animal substance will be affected by the influence of external agents, and hence will result, under given circumstances, certain perceptions for his consciousness. The sick man suffers; but as sure as he suffers, observation descries in his suffering organs movements different from those of the sound state! The sick man takes a remedy which does him a service—his sufferings diminish; and *vice versâ*: but in the former case, his organs will be less disordered—they will more nearly approach the *hormal* rhythm; while in the latter, they will tend to more and more disorder, and the disturbance will spread from the first organ to several others.

This being settled, the bases of medicine are settled also. No disease is ever in the first instance general; it always begins in some one organ, and often in a single tissue of that organ, even when it depends on a cause which has effected an alteration in the fluids—as in the case of small-pox. If, then, the practitioner make use of his senses, and find out the primitive seat of the disorder, and if especially he ascertain the exciting cause of this rising disturbance, he succeeds most usually in arresting it, and the malady is stifled in its cradle. It is thus that the new French method has reduced, in a manner truly wonderful, the number of severe fevers, or rather of those bad symptoms which are indeed now seldom met with, except where assistance has been tardily given, or where it has been entirely rejected. This is a fact well known; it is attested by all practitioners who have to deal with fever in the hospitals, civil and military. It is rare now to find among them any general or essential fevers; they are all reduced to affections simply local.

But what particularly distinguishes this method is, that it rejects no means, how empirical or powerful soever they may appear. We do not become bound to employ only one kind of remedy, for we believe that all kinds have their proper uses; but we take pains to appreciate their effect, and to accommodate them to the susceptibility of the organs disturbed. The action of the modifiers of the constitution is our constant study, and their effects on motion and sensation, our guide in estimating their value. Whatever is injurious to the case in hand is thrown aside; but we do not reject its possible use in other cases.

Thus we have no system *à priori*, no preconceived ideas, no oath *in verba magistri*. If we have adopted for our guide the irritation and ab-irritation of the tissues, it is because we cannot by any possibility find others better.

We intreat you, gentlemen, that you will each individually reflect upon this subject, and ask yourselves how you generally judge that the prescription of your physician is or is not suited to your complaints: if you feel more fever, more agitation, less repose, and more suffering, you say to him, "your remedy, sir, does not appear to me to be suited to my case;" if you feel, on the contrary, more calm, less agitation, and less suffering, you say to him just the reverse, and express all your gratitude. Well, then, gentlemen, these modifications, which you have each of you experienced, resolve themselves ultimately into the simple facts of motion and sensation, (*le mouvement et le sentiment*) and the system which we pursue is nothing more than the interpreting their indications in maladies. But perhaps you will say, have we not had this system from the earliest times, and has it not been practised by all the sects? Common sense would suggest such a question: the truth, however, must be told—it has *not* been so. In a large number of cases it was usual to say to the sick, "have patience, it is the remedy that is operating." In others, as in the gout, for example, the expression was, "I can give you no relief—your sufferings are necessary for nature's purpose, and you must bear them." In divers acute diseases, where the remedies only augmented the fever, and the other bad symptoms, in place of soothing him, the practitioner would congratulate the patient, and tell him that it was necessary to keep up the natural powers, in order to effect a salutary crisis. How often have unfortunate creatures, parching with thirst, and dying for cold drinks been obliged to gorge themselves with hot draughts, which they rejected with horror! This has been the practice, gentlemen, not very far away from you. Before the cholera reached France, it was treated in this way: it was only with the greatest difficulty, and by dint of undeniable success, that the physicians of the

north and east consented to cool their patients.

There are still many physicians who, in costive and painful states of the digestive organs, prescribe stimulants under which the stomach suffers, and who bid the patient be of good cheer, as he will be benefitted by his sufferings in the end. Others there are who have more regard for their patients' troubles, but do nothing more than change the remedy, substituting one mode of punishment for another, (always preserving the principle) but never rendering them any real relief.

No, gentlemen; the art of sparing the sufferings and tortures of patients is not so ancient as you might suppose: it is a modern art, and an art which has made but little real progress, except under the happy influence of the method which we cultivate.

This method, gentlemen, is called the *physiological*, for it observes and deals with life in the abstract, the life of the organs, and *in* the organs, with reference to the agents which can exercise any influence upon them.

I have now laid before you, gentlemen, the principles of the *physiological* medicine—that medicine which good sense approves, and which keeps pace with the intelligence of the age—that medicine which has necessarily been adopted by all the ablest members of the profession, and by all whom their vocation or their circumstances induce daily to come amongst us. It remains for you, the *élite* of our men of science, to extend to it your encouragement. Only deign to consider it well, and you will be convinced that here is no chimera—that it has a real existence, and is of a nature to extend itself, and to attract the regards of all men who think, and love to contemplate the advancement of all the predictions of the human mind.

BROUSSAIS.

CHOLERA TREATED BY COLD WATER*.

To the Members of the Central Board of Health, London.

GENTLEMEN,

THE perusal of Dr. Hardwicke Shute's communications induces me to transmit an account of a most severe case, successfully treated, some weeks back, almost on the same principles.

On the 6th of August, at 7 P.M. I visited a young man, æt. 20, who was in the confirmed collapsed stage of cholera, attended with the following symptoms: general coldness and purplish colour of the skin, from every pore of which issued copious cold perspiration, or rather cold water. Breath and tongue cold, the latter feeling like a frog; no pulse at the wrist; cramps in almost every muscle; and the vomiting and purging of transparent fluid excessive. Urine suppressed; the countenance, if at first in any degree shrunken, became, in less than an hour, completely collapsed, and the voice decidedly a choleric whisper. On the 7th, the pulse had not returned, yet the diarrhœa was considerably checked, but an immoderate flow of urine for a short time was observed. The skin was dry, though still purplish and cold; cramps not so urgent; tongue no longer cold, slightly crusted; vomiting quite as frequent and copious. On the 8th there was some improvement; some warmth of skin, but no urine. On the 9th, a slight tinge of bile in the evacuations; no urine; pulse returned, but beating slowly. On the 10th, motions decidedly bilious, and the last feculent: skin warm; pulse better. No urine till the 11th, from which time he gradually improved, was out on the 16th, and resumed his labour on the 24th, having had scarcely any consecutive fever.

The treatment was as follows:—A mustard plaister was applied to the spine, from the occiput to the sacrum, followed by a blister spread on adhesive plaister. An enema of starch and laudanum was immediately administered, repeated in three hours, and then every twelve hours for four days; but after every loose motion, three-fourths of a pint of starch alone was thrown up the rectum. No medicine of any kind, but cold water as much and as frequently as the patient desired.

A medical friend who saw this case with me on the 11th, afterwards treated a pulseless old man, æt. 70, on the same plan, except giving forty drops of laudanum in chalk mixture, and speedy recovery ensued, re-action being so decided as to allow of bleeding.

I gave cold water *ad libitum* to the first patient I attended with cholera, and withdrew all stimulants; and succeeded. But many other remedies were also tried, and it was not till I lost two cases, and witnessed many deaths under other plans, that in the cold stage I adopted the simple method above de-

* For this paper we are indebted to the Central Board of Health. We take the opportunity of hinting to our contemporary, that when he received it on Monday he was not instructed to detain it till Thursday at 4 P.M.—E. G.

tailed. Experience alone can decide whether the counter-irritation be or be not essential, in addition to the cold water.

I am, gentlemen,
Your obedient servant,
JOS. B. WHITING.

King's Lynn, Sept. 2, 1832.

In premonitory cases, the astringent and opiate plan recommended to the medical practitioners of this town by my brother, Dr. Whiting, has been marked by the most decided success in arresting the progress of the disease, of which there has certainly been *three*, if not five, importations.

MEDICAL GAZETTE.

Saturday, September 8, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

STUDY OF ANATOMY AS AFFECTED BY THE "ACT."

WE have, on various occasions, directed the attention of our readers to the general questions involved in the Bill for regulating the supply of bodies for dissection, which was recently brought before parliament. That Bill has now become an "Act," and constitutes part of the law of the land; and as we had occasion to allude last week to the slow and gradual manner in which a knowledge of the legal enactments affecting themselves seem generally to be acquired by the members of our profession, we have devoted a considerable portion of the present number to lay before them an exact copy of the "Anatomical Act*," which has just issued from the government press; so that it will not be our fault if our brethren do not know—till taught by having some penalty to pay—that there exists in it sundry provisions touching them with which it behoves them to be especially acquainted. Those who undertake to teach anatomy have several new duties to perform; the first of which is to give notice of the place where it is their intention to practise dissection, at least one week before they begin to do so; and the second, to admit no subject without setting forth various particulars which are specified; thirdly, to enter the said particulars in a book, to be always forthcoming at the demand of the Inspector; and, lastly, to have the remains of the body decently interred—a certificate of which, by the Act, is required.

After a little time, we have no doubt but that the machinery in general will work well enough; the part most likely to produce a little jarring is the burying clause, but even with respect to that the difficulties are not insurmountable.

So much for those who undertake to teach, and who, we trust, will have no reason to complain. With respect to the student in anatomy, the case is somewhat different. That he who provides apartments for dissection should be obliged to take out a license, and should be strictly under the eye of the government, is but just and reasonable; but that the student, who is to frequent the school thus licensed, should be hampered, does not appear quite so obviously necessary; at all events, nothing should be required of him but what is easy of accomplishment. However, our object at present is rather to explain what the Act really requires, than to discuss its fitness. Pupils, therefore, ought to be aware, that, before they can take up a scalpel, they must have a license from the Secretary of State for the Home Department, to be applied for in the manner described in our last number—stating his christian and surname at length, his residence, and the school he is about to attend; all which is to be countersigned by two justices of the

* See page 749.

peace. Now all this will require some little time; and inasmuch as gentlemen, on arriving in town, have no "residence," it would be desirable for them to betake themselves to the seat of their studies a few days earlier than usual, and before the courses actually commence.

So far there is nothing to do but comply with what is required of them: but we have now to allude to a demand which we advise them most strenuously to resist—namely, that of a fee on the part of the chief clerk at the Home Office. Our attention was directed to this by the following letter:—

To the Editor of the London Medical Gazette.

London, September 1, 1832.

Sir,

There is a condition exacted by government from those who apply for a license to practise anatomy under the new Act, besides those enumerated in the Medical Gazette of this day's date, to which I should wish to direct the attention of the profession. Previous to the delivery of the license, a fee of 2*l.* 2*s.* 6*d.* is demanded by the chief clerk of the Home Office, from those whose claims to practise anatomy have been approved of by his superior. Should such fee not be forthcoming, the license is, for the present, withheld. This "tax upon knowledge," as a teacher of anatomy, and in the interest of my pupils, I have felt it my duty to resist. No precedent can be pleaded in its favour—it is not in any way authorized by the Act—in principle it is illiberal—and to the student of anatomy, subjected as he is by the new statute to the same restrictions and penalties with his teacher, it might be rendered exceedingly oppressive. Should I find, on further application to the Home Office, that this fee is still insisted on, I shall again bring the subject under your notice.

I am, sir,
Your obedient servant,
A Physician.

We have carefully perused the Act, and find, with our correspondent, that nothing of any such sum to be paid for the license is therein mentioned. "It is not in the bond;" and as it is obviously a very heavy and disproportionate demand, we hope to see it effectually resisted. We do not believe that it is legal to refuse the license to those who decline paying a price for it of which no specification is made, and we feel satisfied that when it becomes known in the proper quarter, and is considered how heavy a tax it would prove to many, the grievance will be immediately abated, if not entirely removed. It is quite clear that the license should be easily obtained, and liberally granted, but that all offence to the public, bringing scandal on anatomy, should be prevented.

With regard to the duties of the Inspector, it is only necessary for the teachers and students to keep in mind that he is bound to see the provisions of the Act fulfilled, and that he shall personally visit the schools from time to time. It is satisfactory to perceive that gentlemen have been appointed who are intimately acquainted with the details of the dissecting-room, and the general circumstances of the case, and who otherwise are not likely to give any trouble which can with propriety be avoided;—a fussy inspector might make himself an intolerable nuisance.

SIR EVERARD HOME.

Sir Everard Home died at Chelsea on Friday last, the 31st ultimo, in the 77th year of his age. Sir Everard was of Scotch extraction, and his connexion with the Hunters brought him into notice at an early period. At one time he enjoyed high consideration, and the more substantial advantage of an extensive practice. He had the good fortune to be appointed one of the surgeons to St. George's Hospital, which, added to various works which attained a considerable degree of celebrity, contri-

buted to extend his reputation, the advantages of which he lived nearly half a century to enjoy. Sir Everard Home was the author of Practical Observations on Stricture, of Lectures on Comparative Anatomy, and of various papers in the Philosophical Transactions. In 1813 he was created a baronet by his late Majesty: he was also sergeant-surgeon to the king, and surgeon to Chelsea Hospital. For several years before his death, Sir Everard had retired from public life, and we believe that his habits, together with his advanced years, had led to considerable bodily infirmity.

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PUBLIC HEALTH IN LONDON.

Rather a remarkable variation has occurred in the mortality of the metropolis during the past fortnight; and it seems to have been connected, or at all events was simultaneous, with a sudden and considerable change in the atmospheric pressure. On Tuesday the 28th ultimo the barometer had fallen to 29·18, being lower than at any time since February; and the period of seven days gave an increase in the number of deaths within the Bills of Mortality, as compared to the preceding week, of 477. Since the 28th the barometer has been gradually recovering its former elevation, and on Tuesday last reached 30·21;—the Bills of Mortality up to which day give a decrease in the number of deaths of not less than 497. We are happy in being able to add, that among these the diminution in cholera is from 274 to 157. This cannot, it is true, be regarded as the total number of deaths by cholera, but we are enabled to state, that taking the whole metropolis, the disease has undergone no increase during the week just concluded.

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ANTI-CHOLERA BRANDY.

Advertisements are constantly appearing in which various nostrums for cholera are recommended, and among others we have anti-cholera brandy. We have been led to make some inquiries on the subject, which have satisfied us that brandy, either neat, or variously prepared and disguised, is very extensively used—and by those, too, who might have been expected to know better—as an antidote to the present epidemic. It is impossible to calculate the mischief which accrues from this most pernicious practice, and we earnestly intreat our readers to use their influence in counteracting it. Among the few points connected with cholera about which the intelligent part of the profession in this country are agreed, we may state one to be that the method of treatment by stimulants, however plausible in theory, has *totally failed in practice.* In fact, the opposite principles, of regarding the vomiting and purging as a sanitory effort on the part of the system to relieve itself of the poison, and the necessity in the low state of vitality to which the patient is reduced of avoiding over-stimulation, as we would shun the fire in a case of frost-bite, are daily gaining ground. Let the bowels be freed by a moderate purgative at the beginning, and let brandy, laudanum, and every thing analogous, be laid aside, either in the prevention or cure of cholera, except in very minute quantities, and under particular circumstances, and the probability is, that the rate of mortality will be greatly diminished. At all events let the dram bottle, rendered yet more poisonous by spices and other fiery additions, be forthwith discarded, by all those who are not tired of their lives.

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SURGEON TO CHELSEA HOSPITAL.

The valuable appointment of surgeon to Chelsea Hospital, which was held by Sir Everard Home, is to be conferred on Mr. Keate.

SERGEANT-SURGEON.

We understand that Mr. Brodie is to be Serjeant-Surgeon to the King, in the room of Sir E. Home. It is said that his late Majesty had made this appointment a matter of special request to his royal brother, then Duke of Clarence, when it was all but certain that he would be his immediate successor to the crown.

ENERVATION PRODUCTIVE OF CHOLERA.

Dr. Legros, of Paris, has published a note, in which he urges strongly the fact of *any* kind of nervous debility rendering the system peculiarly exposed to an attack of cholera. It is, however, to one source of exhaustion that he particularly alludes. The same observation has, indeed, been made by others; but as it has not been sufficiently impressed upon the public, we think it right to bring it more pointedly under the notice of our readers. A case will render farther explanation unnecessary. A young man of twenty-seven, when on the point of marriage, was seized with cholera: his illness was severe, and his convalescence slow; however, having regained his health to a considerable extent, he would consent to delay his happiness no longer. He was married; and, at four o'clock next morning, was attacked with cholera, having had no premonitory symptoms, and died in seven hours, notwithstanding the most diligent employment of remedial means. Again, an old general left his house in good health, and was brought back in a dying state in two hours afterwards. What follows will read best in the original:—

"Interrogé sur ce qui avait précédé son indisposition, il nous apprit qu'il était bien le matin, qu'il n'avait pas de dévoiement, que cependant depuis environ trois mois, il suait plus souvent, plus abondamment et non plus de facilité que de coutume, puis, après beaucoup de tergiversations, il avoua qu'il était allé avec une femme, et que c'était pendant de vains efforts pour exercer le coït, qu'il avait été pris de tremblemens de sueurs froides et de vomissemens."

He died in the course of a few hours, having neither purging nor cramps; but with severe vomiting and mortal collapse. It is in the case of convalescents and elderly persons that the attack, under the circumstances alluded to, has been chiefly met with.

CASES OF HERNIA,

Occurring at the Middlesex Hospital; with Clinical Observations,

By Sir Charles Bell.

Gentlemen,—In the present and the preceding month you have witnessed the operation for hernia performed five times. As the occasion of doing your duty in these cases comes unexpectedly upon you, and especially requires decision, I am very desirous that you should not lose the advantages of these examples, but that, by mature reflection upon them now, you should be prepared, when it becomes your duty, to act with promptitude. It is one benefit of clinical instruction, that, by conversing with you as we pass round the wards, I learn on what to dwell when we are met here. I think I have observed that you mistake very much the importance of tobacco injections. That practice has arisen in a physiological error, and is, I am of opinion, wrong in every view we take of it. The effect desired to be accomplished through it, is to produce debility, with the view of removing "spasm" from the stricture, and to withdraw the intestine by exciting the action of the bowels within. Now, before I proceed to my argument, I must acknowledge that the highest authorities in the profession, both now and heretofore, are in favour of the practice; which will make you weigh my opinions and yield only to conviction. In the first place, there is no such thing as spasm in the neck of a hernial sac. When you perform the operation with the knife, it is not muscle which you cut, and therefore it is not muscle which prevents the reduction of the bowel. As to exciting the action of the intestines within, you must observe that there is just as much danger to be apprehended from this practice as from too great pressure applied from without. If you have attended to the condition of an incarcerated and strangulated gut, you will know that there is a

portion of the intestinal canal which is in danger from the sharp edge of the stricture pressing upon its reflected angle, and that there are these various causes of failure in hernia* :—1st. Abdominal inflammation, excited by the writhing and distention of the intestines above the strictured part. 2dly. Mortification of the intestines within the sac. 3dly. Rupture or ulceration of the gut opposite to the line of the stricture. All of these, but particularly the last, are sufficient reasons against augmenting the violent excitement of the bowel within.

Experience convinces me more and more, that the surgeon's practice in hernia must be determined, not by symptoms, but, after having ascertained that the distress of the patient does arise from hernia, by the touch—that is, by feeling the roundness, the fulness and hardness of the tumor, and the narrowness or *pinch* of the neck. You have here the reports of four operations performed in succession, and all attended with success. Now in each of these it was a small portion of the intestine that was down, and the stricture was, in all the cases, very close; in short, after the surgeon's duty was performed, and the state of the parts ascertained, the opinion was universal, that nothing could have succeeded in reducing the intestines except the edge of the knife. Happily the patients were sent into the hospital early, without violence having been done by the taxis, and there was no delay in performing the operation after they were received. But, unfortunately, it often happens thus: the patient is conveyed to the hospital after a surgeon has done his best out of the house; the house-surgeon makes his attempts, is unsuccessful, and sends for the surgeon of the week; he next examines the tumor, attempts reduction, then orders a large bleeding, the warm-bath, stimulating clysters, and a consultation to be called. Now, with all this, there is too much delay. I hold it to be the duty of the surgeon to make his attempt to reduce the hernia with patience, gentleness, and perseverance: during this time, if he be an intelligent practitioner, and his experience be grounded on the anatomy, he will be better able to determine upon the propriety or the necessity of operating than twenty surgeons standing round the patient's bed, observant merely of the symptoms. And here is the advantage of character in an institution like this—that the patient does not object to the operation, or delay giving his assent, after the opinion of the surgeon has been declared.

To return to the subject of tobacco injections. One advantage I certainly see in that practice: it makes the patient very sick, and low and cold, and he and his relations acquiesce readily in the surgeon's decision, seeing that there is all the appearance of approaching death. Nor must we omit to observe, that death has, on many occasions, been the consequence of the use of tobacco. The tobacco smoke is exceedingly unmanageable; sometimes producing no effect, but sometimes unexpectedly bringing on lowness of pulse, fainting, cold sweats, sickness, and tossing and anxiety. In regard to the tobacco infusion, you will find that our authorities differ very much as to the quantity that is proper. Sir Astley Cooper has seen one drachm, in infusion, prove fatal; while others employ two drachms, or even more, in decoction. But my grand objection to the use of tobacco is the procrastination, and the admission that there are any means to be trusted to, compared with the taxis, or any thing to be done when that fails but the operation with the knife.

I must remind you, however, that in hernia, and especially in large herniæ, there is sometimes an opposition to reduction from the condition of the bowels within the abdomen. It is in such cases that the apothecary's practice—clysters and purgatives—may be of advantage: but when there is a small hard knob in the groin, let nothing come in competition with the surgeon's hand.

But let us proceed to our cases.

Femoral Hernia.

Case I.—A. D., 64 years of age, mother of nine children, was admitted into Bird's Ward, on the afternoon of July 5th, with symptoms of strangulated hernia. A swelling was found in each groin; that in the right large, soft, of irregular form, yielding to the pressure of the hand with a croaking noise, but not entirely disappearing, and receiving a great impulse on coughing; that of the left side, the size of a large walnut, tense, incompressible, and receiving no impulse

* See a Clinical Lecture on Hernia, by Sir C. Bell, in Med. Gaz. vol. iii. page 104.

from coughing.—(No. I.) Neither swelling was painful: the first mentioned had existed for fourteen or fifteen years, she thinks it has never gone entirely up, and she has not suffered inconvenience from it: the last first showed itself seven years ago when she was making some exertion, and has occasionally come down, but never until now having been more than an hour and a half down, but going up on pressure, and sometimes not reappearing for four or five weeks. Last night the swelling in the left groin reappeared while she was ironing, and as usual when it descended she was attacked with sickness and vomiting, which, as she did not succeed in reducing it, continued during the night: this forenoon she sent for a surgeon, who ordered her to come to the hospital.

For three hours subsequent to her admission she had no symptoms; then she was attacked with vomiting, and soon after began to complain of pain in the belly. The taxis being tried without effect, twenty ounces of blood were taken from the arm; and the reduction being again attempted without success, the operation was resorted to, twenty nine hours after the descent of the rupture.—(No. II.) The sac contained a little reddish serum, a portion of omentum, and underneath this a piece of bowel, the size of a large cherry, of a dark red, almost chocolate colour.—(No. III.) The stricture being divided, the gut and part of the omentum were returned. Immediately after the operation, a large enema of warm water was given, and two hours subsequently, ten ounces of blood were taken from the arm. Early in the following morning, the patient began to take small doses of sulphate of magnesia, and at noon she had seven grains of extract of colocynth, two of calomel, and three of extract of conium, in pill, which produced free evacuation from the bowels. In the evening, twenty leeches were applied to the abdomen. From this time, with the exception of some pain she one day experienced from an error in diet, and which yielded to one application of leeches and a dose of castor oil, she had not an unfavourable symptom.

(No. I.)—I shall not detain you long here, but just remind you that you have, in this description, the distinctions marked which have been, rather incorrectly, called chronic and acute hernia. You see at once, that what is called the acute hernia results entirely from a mechanical cause: it is a small, round, hard tumor, which receives no impulse from within, owing to the closeness of the stricture; and you distinguish it from the large lax tumor, with an open or wide neck, through which the abdominal impulse is conveyed.

(No. II.)—"Twenty-nine hours after the descent of the rupture."—Gentlemen, you must put no value upon this fact; it informs you of nothing. The mischief may be done in an hour as well as in nineteen hours. The lesson you receive is, that there must be no delay after the proper means have been tried to reduce the tumor.

(No. III.)—This small cherry-like portion of intestine marks the condition of the greatest danger in hernia. After the constriction of the veins has produced this dark colour, secretion into the interior of the gut and effusion between the coats come on very rapidly, complete the strangulation, and mortification must immediately follow.

Inguinal Hernia.

Case II.—James Turner, æt. 50: August 1, 1832. He has been subject to hernia for many years, but has always been able to return it when it came down: he has generally worn a truss, but latterly has not, on account of the spring being broken.—(No. IV.) The rupture came down yesterday afternoon, since which he has not been able to return it. He was admitted into the hospital at twelve o'clock this morning, previously to which he had seen some medical man, who tried the taxis two or three times, but without avail. The tumor was acutely painful, so that very little time was spent in trying the taxis: he said he had vomited frequently before he came to the hospital, but did not do so afterwards. His bowels had not been open since the rupture came down. There was pain in the abdomen, in the region above the hernia. A consultation was called, and the operation decided upon, and was immediately performed. An incision was made over the tumor, about three inches in length; and, after some dissection, the sac was laid open: it was found to contain a large quantity of fluid; some also came from within the abdomen.—(No. V.) The portion of intestine that had descended was about six inches in length, and of very good colour. The stricture was remarkably small: it was divided upwards, and inclining rather outwards. One circumstance not attending the operation in general was, the acute pain which this patient suffered during the whole operation: dividing the different layers of membrane gave severe pain, as also did the division of the stricture. There was no omentum in the sac. The edges of the skin were brought together by two sutures, and a compress and bandage applied.—(No. VI.) A large enema of warm water and gruel was administered, which returned mixed with fæcal matter. The bowels were twice open afterwards. He also took a calomel and

opium pill every six hours; and a drink of barley water, Epsom salts, and lemon juice. Towards evening there appeared to be more pain in the belly, above the hernia, for which twenty leeches were applied, which relieved him.

2d.—He passed a good night; the pulse was 64, and it has not risen since the operation; the bowels have been open four times to-day; the tongue is white, but quite moist. Towards evening, his pulse had increased greatly in strength, though not in velocity; there was also a jerk in it; he was bled to ℥xx. after which it became soft and quite compressible. He complains of more thirst than he did.

3d.—His pulse has remained quiet since the bleeding; the tongue is moist; the bowels have been freely open to day; in the morning there was a slight increase of pain in the abdomen, above the hernia. This was relieved by the application of sixteen leeches. The wound was dressed to-day; there was a good deal of swelling and induration about the edges, extending down the spermatic cord.

4th.—There is no increase of pain in the abdomen; his pulse remains quite soft, and tongue moist. The bowels have not acted very freely, and he had a dose of castor oil, after which they were properly opened. He did not sleep very well last night, so he had Opii ♏xx. Aq. Menth. Pip. ℥iss.

5th.—He did not sleep well. This morning he has more uneasiness in the wound; he has no pain in the abdomen; the pulse is quite soft; his bowels are freely open. The dressings were discontinued, and a linseed poultice was applied to the wound.

6th.—He is doing quite well. There is no pain in the abdomen; his bowels are freely open; the wound looks better.

After this he had not a bad symptom. The wound continued healing daily, and by the 24th had completely cicatrized.

(No. IV.)—You will remember that a person who has worn a truss, to the effect of nearly closing the ring, comes into a condition of great danger when the gut does descend. There is a narrowness of the passage, and a sharpness of the edge of the stricture, which gives to the accidental descent of the intestine all the characters of the "acute" hernia.

(No. V.)—Your prognosis will be very much directed by this secretion. A certain quantity of serous effusion is a necessary consequence of incarceration; and when this is limpid, it is favourable. But we must distinguish the effusion into the sac from the serum which flows from the abdominal cavity after the reduction of the intestine. The latter implies that the viscera within the abdomen have suffered high excitement, and when it is in large quantity it is a very unfavourable symptom. A case occurred during the summer, which no doubt you recollect, in a woman: when the finger was withdrawn, after reducing the intestine, the serum flowed as water from a cask. She died, and yet the intestine within the sac promised a favourable result. I remember a gentleman standing by, saying, "Well, if that intestine does not recover, I do not know when we are to expect success;" and I then pointed out to him the quantity of serum, as the very worst symptom.

(No. VI.)—As to this pain from the cutting of tendinous parts, we cannot depend much on the expression of the patient. He certainly complained a great deal during the operation, but he was as remarkably merry after it. I do not remember to have seen a patient operated on, who was in actual danger, as this man was, of forcing all down again by a hearty fit of laughing.

Femoral Hernia.

Case III.—S. B., 29 years of age, walked into the hospital on the afternoon of Sunday, August 12th, complaining of having had severe sickness and vomiting the two preceding days. Admitted by the apothecary into one of the physician's wards; that gentleman, on investigating her case, detected a small swelling in the right groin; and suspecting that this might have some connexion with her complaints, he requested the surgeon of the week to see her. From her statement it appeared that she had noticed a small kernel in the situation just mentioned, for the last three years; that, on the afternoon of Friday last, whilst she was occupied in her avocations as housemaid, she felt it get larger, and that soon afterwards she was attacked with severe twinging pain in the belly. A calomel pill, and some gruel which she took, were rejected by vomiting. In the course of the night she had a scanty scybalous motion. On the morning of Saturday some pills and a draught were administered, but not retained; and some castor oil, taken at six in the evening, had the same fate. Since then she has not vomited nor had sickness; her countenance is good, and betrays no suffering, and her pulse is natural. On examining the swelling, it was found to be little larger than a hazel nut, firm and unyielding, and receiving no impulse on coughing; free from tenderness, but occupying precisely the situation of femoral hernia; there was no pain or tension of the abdomen. The taxis being tried with-

out avail, a large clyster was administered, and another attempt made to reduce the swelling, but with as little success. Although, therefore, no urgent symptoms existed, it was now resolved to operate, from the circumstances revealed in the history of the case.

On reaching the hernial bag, or fascia propria, this was so small in size, and some blood-vessels were so distinctly ramified upon it, as to occasion a doubt if it was not the bowel. (No. VII.) On opening the peritoneal sac a little bloody serum was evacuated, and then there was perceived a portion of bowel, not larger than the tip of the finger, of literally a black colour. The stricture was very tight, and on its being divided, about a couple of ounces of serum flowed from the cavity of the abdomen. The piece of gut was now returned within the abdomen, but it did not recede from the ring, being retained there by adhesions all round, which were not disturbed. A compress and bandage were put over the parts in the usual way, it being determined, however, that if clear evidence of mortification should take place, these should be removed, and the bowel probably opened. As this, however, had not taken place, and as very violent inflammation existed, blood was taken from the arm immediately on the patient being replaced in bed; and as the pulse rose under this, twenty ounces were allowed to flow. A large clyster of warm water was ordered forthwith, and two grains of calomel every two hours. Five hours after the operation, the countenance for the first time showed some anxiety; twelve ounces of blood were taken from the arm, and some extract of colocynth, with calomel and conium, in pills, ordered.

In the morning of the 13th the bowels had not been acted upon; the belly was soft, and free from pain; yet, as there was some sharpness of pulse, ten ounces of blood were taken by venesection, and fifteen leeches applied to the abdomen. The colocynth pills were repeated, and again at 2 P.M. At 6, half an ounce of castor oil was given, which at 10 o'clock—that is, 30 hours after the operation—produced the first motion from the bowels. Copious evacuations followed. The case proceeded favourably; but on the morning of the 17th the lint covering the wound was observed to be tinged yellow, and on removing this, a small quantity of a similar coloured fluid was seen to come from the wound. Firm pressure was made upon the aperture by a compress and roller; half an ounce of castor oil was taken, and a large enema given. If any severe griping succeeded the exhibition of the castor oil, the compress was directed to be removed; but this was not called for; the patient had a free evacuation per anum.

On the 18th bilious fluid in increased quantity had flowed from the wound, owing in a great measure to the compress having shifted, and the difficulty of applying pressure by means of a roller. To remedy this, and as there was evidently a free passage in the natural course of the intestinal canal, a truss was applied over the compress, and by this means the discharge of bilious matter from the wound was effectually prevented. The truss was continued until the 26th, the bowels acting freely per anum on the exhibition of castor oil; when, owing to some redness and vesication of the integuments, it was left off, and large poultices applied over the wound. The discharges of bilious matter now recurred, and in considerable quantity; but in a day or two this subsided, and under the chalk dressing the integuments have assumed a healthy character. The sore is now granulating, healing, and contracting; there is still some weeping of a bilious fluid, small in quantity, and increased upon the taking an aperient, which is requisite to get the bowels to act per anum. (No. VIII.)

(No. VII.)—You will remember that the anatomy of hernia cannot be completely studied by the dissection of the natural parts. The true sac assumes a very different appearance in the varieties of hernia. We have no time to enter fully upon this subject: yet I must remind you that the peritoneal sac of the femoral hernia of women is so exceedingly thin, and the colour of the intestine shines so distinctly through it, that you are very apt to mistake it for the intestine, and to reduce it along with the gut. The circumstance noticed in this case, of the resemblance of the fascia to the bowel, strengthens the same misapprehension. You distinguish the sac by the mode in which the vessels run upon it; and taking the tumor betwixt the finger and thumb, you can discover that it contains a fluid, and that in that fluid there is a nucleus. The dissection, however, is a very nice one.

(No. VIII.)—Of the five cases which are read to you, this is by far the most interesting. I mentioned three sources of danger; and that where there was a small portion of intestine and a narrow stricture, the coats were apt to be partially cut, and afterwards to ulcerate. You see that, in this case, the general disturbance or injury to the intestinal canal within the abdomen, did not bear a proportion to the injury of the portion included in the hernia; and you accordingly observe that recovery is almost certain. You will especially mark, that when the intestine within the sac is

thus exposed to ulceration and mortification, it does not lie loose; for, as inflammation accompanies the injury, it is agglutinated by coagulable lymph to the peritoneum behind the stricture. You ought not to undo this adhesion; and, on the whole, the practice here pursued by Mr. Arnott I conceive to be just what you ought to follow. You will observe that there has not been a sloughing of the intestine, but an ulceration where it was pinched; and you will do well to mark this distinction when feculent matter is discharged after hernia.

I am inclined to believe, that in this case the whole diameter of the gut was not included in the stricture. If so, this is a very favourable circumstance, and will facilitate the process of cure, and lead us to hope that there will be here no anus at the groin. This is a subject which we shall take up at more leisure.

Femoral Hernia.

Case IV.—Rebecca Meeking was admitted July 12th, at 7 o'clock in the evening. She stated that at six o'clock this morning she was seized with vomiting and pains in the bowels, when her attention was directed to a tumor in the right groin, which she says she had not observed before. She was visited shortly after by some medical man, who found that she had rupture, and endeavoured to reduce it, but without success; he also tried again this evening, but could not succeed. He then advised her to be brought to the hospital. On her admission there was found to be a femoral hernia of the right side; the tumor was about the size of a walnut. One portion of it was soft and lay over Poupart's ligament, and could be easily brought down; there was another portion much harder, situated to the inside, and below the ligament. This portion was irregular, while the other felt smooth and even. She says that she has vomited frequently during the day. The bowels were open yesterday, but not to-day. The tumor is now very painful on pressure, and offers great resistance, all attempts to reduce it proving ineffectual. There is no pain in the abdomen; the pulse is small and weak. She had an enema, part of which returned immediately, and the remainder came away about half an hour afterwards, mixed with hardened fæces.

The surgeon of the week came to the hospital about 10 o'clock, and tried for a short time to reduce the hernia, but could not make any impression on it; it was accordingly found necessary to operate. An incision was made to the extent of about two inches over the tumor. The irregular mass felt through the skin was now exposed, which proved to be fat, and two or three small glands. About the centre there was seen to be a large vein, running up under the skin of the abdomen; this was carefully avoided. (No. IX.) After a little dissection the sac was exposed, which appeared very thin, being quite transparent; it was opened, and found to contain about ℥ss. of fluid. The portion of intestine was now exposed, and about the size of a large hazel nut; it was in a very good condition, though of a darker colour than natural, not having lost its shining appearance. The stricture was divided upwards and inwards, and the intestine easily returned; no omentum was contained in the sac.

The edges of the incision were brought together with adhesive plaister, and a compress and bandage applied. An enema of warm water and gruel was given, and ℥xiv. of blood taken from the arm.

13th.—She has not vomited since the operation; she got some sleep during the night; her bowels have been open once this morning; she had pain in the lower part of the abdomen, which was relieved by the application of leeches; she was ordered to take a calomel and opium pill every six hours, and a mixture containing castor oil. In the evening there was an increase of pain, and she had nine leeches applied, after which she was relieved.

16th.—She has gone on quite favourably since last report; the bowels have been open daily by taking small doses of castor oil.

After this her improvement was rapid, and she had not a bad symptom. The wound required to be poulticed for about a week, after which it was dressed, and gradually healed. She was discharged from the hospital August 14th. (No. X.)

(No. IX.)—The greatest advantage of experience in a lecturer, is when he recollects what were the things that puzzled him in his early practice. In just such a case as this was my first operation performed. I well remember the difficulty I had in distinguishing the sac, and opening it. The femoral hernia, as I have had frequent occasion to observe to you, is generally very small: and over it lie the glands of the groin, with much fat, and sometimes, indeed not unfrequently, there are sacs, containing fluids, or sort of hydatid tumors, making the mass altogether very irregular. I have just observed that the layers covering the peritoneum are very smooth, and very like the proper sac; so that this forms a piece of dissection which requires some dexterity, together with a perfect knowledge of

the distinct characters belonging to the parts. You lay the glands aside, in such a manner as to make it unnecessary to divide the lymphatics of the thigh. The wounding of the vein, here spoken of, is of no consequence, further than that by covering the surfaces with blood, it may make the necessary dissection a little more difficult.

(No. X.)—The only further observation that I will make on this case, is in regard to the direction of the bistoury in cutting the stricture. It may have appeared to the by-standers that the cut was made obliquely upwards and inwards; but certainly the surgeon's intention was to cut directly upwards. In these small herniæ, it is of very little moment; for all you have to divide is just the sharp edge of that tendinous arch which stretches across the upper part of the neck of the sac: and you would do well to observe with what intention this is done; which will afford you a measure of the extent of your incision. In such cases, you do not think of forcing in the point of your finger; but only the directory. Then passing the straight bistoury (which you see is blunt to half an inch from the point, and cuts only at one small part of its edge,) you raise the hand, separating the instruments; and you do not draw the bistoury, as in common cutting, but raise it like a lever, the point resting on the groove of the directory. By this means, you cut only what strongly resists; that is, the firm edge of the ligament, and, of course, a small portion of the neck of the sac. Now, when this is done, you are not at once to push up the intestine; but rather draw it a little down, and compress it, and empty it; and always be very careful in the mode of reducing it, avoiding, as the most dangerous practice, the thrusting in of the finger into the stricture, whilst the tender part of the gut remains in its original place.

Direct Inguinal Hernia.

Case V.—On the 14th of July, at the hour of visit, the attention of the surgeon was called to the case of a man between fifty and sixty years of age, who for the last two days had had bilious, and now stercoraceous vomiting, and from whose bowels there had been no evacuation per anum for a week. He had for years had a rupture in each groin. The hernial tumor of the right side was found to be large, soft, and in its contents were felt formed fæces. With some difficulty it could be entirely reduced, descending again immediately on the pressure being withdrawn; that of the left side was about the size of a hen's egg, tense, but yielding in some measure to pressure, which was attended with a gurgling noise; but the swelling could not be reduced. The skin over the latter was reddened, the abdomen was distended, and the skin cold and clammy. It was feared mortification of the contents of the left hernial sac had taken place; and on getting into the sac, this was found to be the case with a portion of bowel which it contained. The stricture, which was not tight, was divided directly upwards, the bowel was opened, and the edges of the incision in it attached by ligature to those of the skin. A free and copious discharge of the contents of the intestinal canal immediately took place, the tension of the abdomen subsided, but the patient survived only 30 hours.

On examination of the body, extensive inflammation of the peritoneum, with effusion of lymph, was found to have existed. The hernia operated upon was discovered to have been a direct one, passing immediately from the belly through the external ring. The epigastric artery passed upwards, on the outside of the neck of the sac; and again, on the outside of this vessel, was seen the internal ring dilated, and with a pouch of peritoneum propelled a little way through the inguinal canal; so that, if the man had lived, it seemed as if he might have had a hernia passing in the usual way through this canal, in addition to the direct one operated upon, the epigastric artery passing up between the openings of the two sacs.

The hernial tumor of the right side was formed principally of the caput cœcum; it was not included in a sac; and although a sac did exist, this lay on the anterior part of the bowel only, and contained nothing. The posterior and inferior parts of the gut were covered by loose cellular substance. The intestine contained in the direct hernia of the left side, had been the lower part of the ilium, close to the cœcum.

This case shewed the advantage of the rule of operating in inguinal hernia, by dividing the stricture directly upwards, which had been done in this case, and yet the incision had not touched the epigastric artery. (No. XI.)

(No. XI.)—We have been observing that the incision of the stricture should be made directly upwards. In this hernia (the bubonocele) there is an additional reason for this practice, since it is not always clear whether the case be one of direct or oblique hernia: and you have had here demonstrated that in the one the artery lies on the outside, and in the other on the inside; while in both, it is close upon the neck of the

sac; so that if, in the direct hernia, the surgeon had cut obliquely upwards and outwards, the artery would have been in danger: and the same danger would have been incurred if, in the other, he he had cut obliquely upwards and inwards. Thus you have the reason of the rule to cut directly upwards in every case.

During the ensuing season, we shall have frequent occasion to resume this subject of Hernia.

AN ACT FOR REGULATING SCHOOLS OF ANATOMY;

Being the 2d and 3d of William IV. cap. lxxv.

Whereas a knowledge of the causes and nature of sundry diseases which affect the body, and of the best methods of treating and curing such diseases, and of healing and repairing divers wounds and injuries to which the human frame is liable, cannot be acquired without the aid of anatomical examination: and whereas the legal supply of human bodies for such anatomical examination is insufficient fully to provide the means of such knowledge: and whereas, in order further to supply human bodies for such purposes, divers great and grievous crimes have been committed, and lately murder, for the single object of selling for such purposes the bodies of the persons so murdered; and whereas therefore it is highly expedient to give protection, under certain regulations, to the study and practice of anatomy, and to prevent, as far as may be, such great and grievous crimes and murder as aforesaid; be it therefore enacted by the King's Most Excellent Majesty, by and with the advice and consent of the Lords spiritual and temporal, and Commons, in this present parliament assembled, and by the authority of the same, that it shall be lawful for his Majesty's principal secretary of state, for the time being, for the home department in that part of the United Kingdom called Great Britain, and for the chief secretary for Ireland in that part of the United Kingdom called Ireland, immediately on the passing of this act, or so soon thereafter as may be required, to grant a license to practise Anatomy to any fellow or member of any college of physicians or surgeons, or to any graduate or licentiate in medicine, or to any person lawfully qualified to practise medicine in any part of the United Kingdom, or to any professor or teacher of Anatomy, medicine, or surgery, or to any student attending any school of Anatomy, on application from such party for such purpose, countersigned by two of his Majesty's justices of the peace acting for the county, city, borough, or place wherein such party resides, certifying that, to their knowledge or belief, such party so applying is about to carry on the practice of Anatomy.

II. And be it enacted, that it shall be lawful for his Majesty's said principal secretary of state, or chief secretary, as the case may be, immediately on the passing of this act, or as soon thereafter as may be necessary, to appoint respectively not fewer than three persons to be inspectors of places where Anatomy is carried on, and at any time after such first appointment, to appoint, if they shall see fit, one or more other person or persons to be an inspector or inspectors as aforesaid; and every such inspector shall continue in office for one year, or until he be removed by the said secretary of state or chief secretary, as the case may be, or until some other person shall be appointed in his place; and as often as any inspector appointed as aforesaid shall die, or shall be removed from his said office, or shall refuse or become unable to act, it shall be lawful for the said secretary of state or chief secretary, as the case may be, to appoint another person to be inspector in his room.

III. And be it enacted, that it shall be lawful for the said secretary of state or chief secretary, as the case may be, to direct what district of town or country, or of both, and what places where Anatomy is carried on, situate within such district, every such inspector shall be appointed to superintend, and in what manner every such inspector shall transact the duties of his office.

IV. And be it enacted, that every inspector to be appointed by virtue of this act shall make a quarterly return to the said secretary of state or chief secretary, as the case may be, of every deceased person's body that during the preceding quarter has been removed for anatomical examination to every separate place in his district where Anatomy is carried on, distinguishing the sex, and, as far as is known at the time, the name and age of each person whose body was so removed as aforesaid.

V. And be it enacted, that it shall be lawful for every such inspector to visit and inspect, at any time, any place within his district, notice of which place has been given, as is hereinafter directed, that it is intended there to practise Anatomy.

VI. And be it enacted, that it shall be lawful for his Majesty to grant to every such inspector such an annual salary, not exceeding one hundred pounds, for his trouble, and to allow such a sum of money for the expenses of his office as may appear reasonable; such salaries and allowances to be charged on the consolidated fund of the United Kingdom, and to be payable quarterly; and that an annual return of all such salaries and allowances shall be made to parliament.

VII. And be it enacted, that it shall be lawful for any executor or other party having lawful possession of the body of any de-

ceased person, and not being an undertaker or other party intrusted with the body for the purpose only of interment, to permit the body of such deceased person to undergo anatomical examination, unless to the knowledge of such executor or other party, such person shall have expressed his desire, either in writing at any time during his life, or verbally in the presence of two or more witnesses during the illness whereof he died, that his body after death might not undergo such examination, or unless the surviving husband or wife, or any known relative of the deceased person, shall require the body to be interred without such examination.

VIII. And be it enacted, that if any person, either in writing at any time during his life, or verbally in the presence of two or more witnesses during the illness whereof he died, shall direct that his body after death be examined anatomically, or shall nominate any party by this act authorized to examine bodies anatomically to make such examination, and if, before the burial of the body of such person, such direction or nomination shall be made known to the party having lawful possession of the dead body, then such last-mentioned party shall direct such examination to be made, and, in case of any such nomination as aforesaid, shall request and permit any party so authorized and nominated as aforesaid to make such examination, unless the deceased person's surviving husband or wife, or nearest known relative, or any one or more of such person's nearest known relatives, being of kin in the same degree, shall require the body to be interred without such examination.

IX. Provided always, and be it enacted, that in no case shall the body of any person be removed for anatomical examination from any place where such person may have died, until after forty-eight hours from the time of such person's decease, nor until after twenty-four hours notice, to be reckoned from the time of such decease, to the inspector of the district, of the intended removal of the body, or, if no such inspector have been appointed, to some physician, surgeon, or apothecary residing at or near the place of death, nor unless a certificate stating in what manner such person came by his death shall previously to the removal of the body have been signed by the physician, surgeon, or apothecary who attended such person during the illness whereof he died, or if no such medical man attended such person during such illness, then by some physician, surgeon, or apothecary who shall be called in after the death of such person to view his body, and who shall state the manner or cause of death according to the best of his knowledge and belief, but who shall not be concerned in examining the body after removal; and that in case of such removal, such certificate shall be delivered, together with the body, to the party receiving the same for anatomical examination.

X. And be it enacted, that it shall be lawful for any member or fellow of any college of physicians or surgeons, or any graduate or licentiate in medicine, or any person lawfully qualified to practise medicine in any part of the United Kingdom, or any professor, teacher, or student of anatomy, medicine, or surgery, having a license from his Majesty's principal secretary of state, or chief secretary as aforesaid, to receive or possess for anatomical examination, or to examine anatomically, the body of any person deceased, if permitted or directed so to do by a party who had at the time of giving such permission or direction lawful possession of the body, and who had power, in pursuance of the provisions of this act, to permit or cause the body to be so examined, and provided such certificate as aforesaid were delivered by such party together with the body.

XI. And be it enacted, that every party so receiving a body for anatomical examination after removal shall demand and receive, together with the body, a certificate as aforesaid, and shall, within twenty-four hours next after such removal, transmit to the inspector of the district such certificate, and also a return stating at what day and hour and from whom the body was received, the date and place of death, the sex, and (as far as is known at the time) the christian and surname, age, and last place of abode of such person, or if no such inspector have been appointed, to some physician, surgeon, or apothecary residing at or near the place to which the body is removed, and shall enter or cause to be entered the aforesaid particulars relating thereto, and a copy of the certificate he received therewith, in a book to be kept by him for that purpose, and shall produce such book whenever required so to do by any inspector so appointed as aforesaid.

XII. And be it enacted, that it shall not be lawful for any party to carry on or teach Anatomy at any place, or at any place to receive or possess for anatomical examination, or examine anatomically, any deceased person's body after removal of the same, unless such party, or the owner or occupier of such place, or some party by this act authorized to examine bodies anatomically, shall, at least one week before the receipt or possession of a body for such purpose at such place, have given notice to the said Secretary of State or Chief Secretary, as the case may be, of the place where it is intended to practise Anatomy.

XIII. Provided always, and be it enacted, that every such body so removed as aforesaid for the purpose of examination shall, before such removal, be placed in a decent coffin or shell, and be removed therein; and that the party removing the same, or causing the same to be removed as aforesaid, shall make provision that such body, after undergoing anatomical examination, be decently interred

in consecrated ground, or in some public burial ground in use for persons of that religious persuasion to which the person whose body was so removed belonged; and that a certificate of the interment of such body shall be transmitted to the Inspector of the district within six weeks after the day on which such body was received as aforesaid.

XIV. And be it enacted, that no Member or Fellow of any College of Physicians or Surgeons, nor any Graduate or Licentiate in Medicine, nor any person lawfully qualified to practise medicine in any part of the United Kingdom, nor any professor, teacher, or student of anatomy, medicine, or surgery, having a license from His Majesty's Principal Secretary of State or Chief Secretary as aforesaid, shall be liable to any prosecution, penalty, forfeiture, or punishment, for receiving or having in his possession for anatomical examination, or for examining anatomically, any dead human body, according to the provisions of this act.

XV. And be it enacted, that nothing in this act contained shall be construed to extend to or to prohibit any post-mortem examination of any human body required or directed to be made by any competent legal authority.

XVI. And whereas an act was passed in the ninth year of the reign of his late Majesty, for consolidating and amending the statutes in England relative to offences against the person, by which latter act it is enacted, that the body of every person convicted of murder shall, after execution, either be dissected or hung in chains, as to the court which tried the offender shall seem meet; and that the sentence to be pronounced by the court shall express that the body of the offender shall be dissected or hung in chains, whichsoever of the two the court shall order; be it enacted, that so much of the said last-recited act as authorizes the court, if it shall think fit, to direct that the body of a person convicted of murder shall, after execution, be dissected, be and the same is hereby repealed; and that every case of conviction of any prisoner for murder, the court before which such prisoner shall have been tried shall direct such prisoner either to be hung in chains or to be buried within the precincts of the prison in which such prisoner shall have been confined after conviction, as to such court shall seem meet; and that the sentence to be pronounced by the court shall express that the body of such prisoner shall be hung in chains, or buried within the precincts of the prison, which ever of the two the court shall order.

XVII. And be it enacted, that if any action or suit shall be commenced or brought against any person for any thing done in pursuance of this act, the same shall be commenced within six calendar months next after the cause of action accrued; and the defendant in every such action or suit may, at his election, plead the matter specially, or the general issue not guilty, and give this act and the special matter in evidence at any trial to be had thereupon.

XVIII. And be it enacted, that any person offending against the provisions of this act, in England or Ireland, shall be deemed and taken to be guilty of a misdemeanor, and, being duly convicted thereof, shall be punished by imprisonment for a term not exceeding three months, or by a fine not exceeding fifty pounds, at the discretion of the court before which he shall be tried; and any person offending against the provisions of this act in Scotland, shall, upon being duly convicted of such offence, be punished by imprisonment for a term not exceeding three months, or by a fine not exceeding fifty pounds, at the discretion of the court before which he shall be tried.

XIX. And in order to remove doubts as to the meaning of certain words in this act, be it enacted, that the words "person and party" shall be respectively deemed to include any number of persons, or any society, whether by charter or otherwise; and that the meaning of the aforesaid words shall not be restricted although the same may be subsequently referred to in the singular number and masculine gender only.

XX. And be it enacted, that this act shall commence and take effect from and after the 1st day of August in the present year.

XXI. And be it enacted, that this act may be altered or amended during the present session of parliament.

NOTE FROM DR. WILSON PHILIP.

To the Editor of the London Medical Gazette.

Cavendish-Square, Sept. 4th, 1832.

Sir,

You will oblige me by giving the following a place among the notices in the next number of the Medical Gazette, for the purpose of informing Dr. Hall that the removal of the brain and spinal marrow is an experiment of a wholly different nature from the destruction of these organs; and therefore that the grave charge he brings against me is unfounded.

The second apparent inconsistency stated by him, depends on facts with which he appears to be equally unacquainted. In my first experiments I found it impossible to excite in the heart, by stimuli applied to the brain and spinal marrow, the irregular actions so readily excited by them in the muscles of voluntary motion; but I discovered by experiments, made many years afterwards, that although this is the case, the action of the heart could be readily influenced, in its frequency, both by stimulants and sedatives applied to those organs.

I certainly was not aware from Dr. Hall's account of his Treatise, that it and his paper

were the same. Now that this gentleman has disclosed the cause of his anger, I can assure him it is imaginary. There was not one concerned who would not have been happy to have regarded his paper in the light that would have been most agreeable to him.

Dr. Hall must submit to a careful study of the present state of physiological knowledge before he can hope to make any addition to it. It is unfortunate for physiology that experimentalists are more apt to pursue their own fancies, than to undergo the labour of acquiring a knowledge of what has been done by others; yet on this foundation, in general, must all experimental progress be founded. Dr. Hall says he has given proof of being well acquainted with my Treatises, at the very moment that he demonstrates his ignorance of the leading points maintained in them; for it is impossible for a person either to have read them, or the accounts which have from time to time been published of the repetition by others of the experiments contained in them, without knowing that the removal and destruction of the brain and spinal marrow influence the animal economy in different, and in some respects, even opposite ways.

I shall take this opportunity of endeavouring to correct what I believe to be a common error, even among those who are well acquainted with physiology—I mean the opinion that the beating of all the arteries is the immediate effect of the contraction of the heart alone. All the facts on the subject, as far as I am capable of judging, assure us that the beating of the arteries, with the exception of those which receive their blood immediately from the heart, is the combined effect of the contraction of the heart and that of the preceding part of the artery. Hence it is that causes which make no impression on the heart often greatly influence the beating of distant arteries, and regulate the distance from the heart to which the beating extends, which, in the healthy state of the arteries, is never as far as the capillaries. In diseased states, the beating of the vessels, like other phenomena of the animal body, is influenced in various ways—a circumstance which Dr. Hall wholly overlooks *. In external inflammations, for example, of too small extent to affect the heart, a pulsation, as every one knows, is often perceived which has no existence in the part at other times; the consequence of the debility of the inflamed vessels and of the arteries, which in the course of circulation precede them, being excited to increased action.—I am, sir,

Your obedient servant,

A. P. W. Philip.

* One of the chief points to be attended to in physiological experiments, is to distinguish the phenomena which depend on the healthy action of the part, from those which are the consequence of the state into which it is brought by the experiment. Dr. Hall reasons respecting the phenomena observed in a limb with a ligature thrown round it, which, affecting more the motion of the blood in the veins than the arteries, from the former being more pliable, as well as many of them more superficial, necessarily occasions more or less morbid accumulation of blood in the limb, and variously influences the action of its vessels, precisely as he would do were the limb in its natural state.

MEDICAL APPOINTMENT.

Dr. J. Wyatt Crane has been elected Physician to the St. George's and St. James's Dispensary, in the room of Dr. Somerville resigned.

WEEKLY ACCOUNT of BURIALS,

From the Bills of Mortality, *Sept.* 4, 1832.

Abscess	5	Gout	1
Age and Debility	56	Hæmorrhage	4
Apoplexy	8	Hooping Cough	5
Asthma	7	Inflammation	31
Cancer	3	Inflammation of the Bowels & Stomach	24
Childbirth	5	Inflammation of the Brain	1
Cholera	157	Insanity	1
Consumption	49	Liver, Diseases of the	4
Convulsions	33	Measles	13
Dentition or Teething	6	Mortification	5
Diabetes	2	Paralysis	3
Diarrhœa	2	Rheumatism	1
Dropsy	7	Small-Pox	15
Dropsy on the Brain	16	Spasms	2
Dropsy on the Chest	4	Stone and Gravel	1
Epilepsy	1	Thrush	2
Erysipelas	2	Tumor	2
Fever	15	Unknown causes	2
Fever Intermittent or Ague	1		
Fever, Searlet	5	Stillborn	8
Fever, Typhus	3		

Decrease of Burials, as compared with the preceding Week 497

METEOROLOGICAL JOURNAL.

August 1832.	Thermometer.		Barometer.	
Thursday . 30	from 44	to 59	29·44	to 29·51
Friday. . . 31	42	63	29·68	29·72
September				
Saturday . 1	42	63	29·69	29·52
Sunday . . 2	44	63	29·82	29·98
Monday. . 3	41	65	30·02	30·17
Tuesday . 4	40	63	30·21	30·16
Wednesday 5	39	64	30·05	30·00

Wind variable, S.W. prevailing.

The 30th ult. and 1st inst. cloudy, with frequent heavy rain; otherwise generally clear.

Rain fallen, 1 inch and ·15 of an inch.

Charles Henry Adams.

BOOK RECEIVED FOR REVIEW.

Practical Observations on Midwifery, with a selection of Cases, Part II. By John Ramsbotham, M.D. &c. Highley.

LITERARY INTELLIGENCE.

Nearly ready, a Treatise on Inflammations, containing their Pathology, Causes, Consequences, and Treatment; with their Effects on the various Textures of the Body: being an extension of "A Dissertation on Inflammation of the Membranes," to which the Jacksonian Prize for the year 1828 was awarded by the Royal College of Surgeons in London. By George Rogerson, Surgeon.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 15, 1832.

ESSAYS ON DIAGNOSIS.

BY

MARSHALL HALL, M.D. F.R.S. L. & E. &c.

ESSAY II.—*continued.*

ON THE DIAGNOSIS OF FEVERS.

I HAVE reserved, for a continuation of this essay, one of the most important discussions which can occupy the physician in regard to fever. It is that of the questions, 1st. whether the affection of Peyer's glands be constant in typhus? and, 2dly, what is the nature of those typhoid diseases in which this affection of Peyer's glands is not found?

I propose to begin this discussion by laying before your readers two propositions upon the subject, from the pen of M. Andral.

1. "Parmi les individus qui succombent avec l'un ou l'autre des groupes morbides appelés fièvres essentielles, quelques-uns ne présentent dans l'intestin grêle aucune lésion appréciable.

2. "Chez le plus grand nombre, l'intestin grêle présente une lésion speciale qu'on ne trouve à-peu-près exclusivement, à son état aigu, que dans les fièvres dites essentielles, et qui consiste dans la tuméfaction inflammatoire," (&c.) "des follicules intestinaux."

M. Louis, on the other hand, asserts: "Chez *tous* les *plaques elliptiques* étaient plus ou moins altérées, et leur altération d'autant plus profonde qu'elles étaient plus voisines du cœcum," &c.

In addition to these dissentient statements, made by the two most eminent observers of Paris, there is no less discrepancy between the conclusions of the physicians of London, Edinburgh, and Dublin; especially Dr. Tweedie, Dr. Allison, and Dr. Stokes.

In the midst of such difference of opinion, two methods of proceeding may be adopted: the first is, to examine anew the evidence already before us; the *second*, to observe anew the facts as they are presented by the sick themselves.

The object of the present communication is to present to such as are disposed to undertake these tasks, the various considerations and reflections which may guide them in their useful labour.

1. Is it really true that all fevers—all the fièvres essentielles—are the same disease? If not, it is certain that the same morbid change is not to be expected in all. We should no more expect to find the "*dothinenterite*" in scarlatina, than we should expect to find the pustules of variola in that disease. Let us suppose a disease similar to scarlatina, with the exception of the contagion and the rash, and we at once conceive a disease very similar to typhus, —so similar as not always to be readily distinguished from it; in which we should not expect to find the dothinenterite. Is there such a disease?

In the first place, Huxham and Willan take great pains to describe a febrile affection, really different (however similar) from typhus, or malignant fever. By the former it is designated slow nervous fever; by the latter, synochus; and, as it occurs in summer, synochus biliosus. Were these observers entirely deceived?

In the second place, do we not sometimes, nay, frequently, observe the anxious attendants on the sick become, through this very anxiety, through fa-

tigue and watching, the subjects of fever—of *typhoid* fever? Have we not frequently to watch several individuals in succession, through such symptoms, from such a cause? Does not a house, from having one anxious case, frequently become an hospital? Have we not to go from room to room, as from ward to ward?

Now, typhoid as such cases may be, is it not rare to see such a symptom as *petechiæ*, and the other symptoms designated by the older observers as *putrid* and *malignant*? At this moment I am watching no less than two such cases, not without fear for the result, in which, however, I regard not the disease as typhus; and now the important question presents itself, should I, if they prove fatal, find the "plaques elliptiques," the "dothinenterite," the peculiar morbid anatomy of real typhus?

There is a circumstance, the importance, and the signification of which has, I think, escaped the attention of our first pathologists. It is remarked by all that typhus chiefly occurs in the *young* between the ages of 15 and 30*. But it is particularly observed by one, M. Andral, that the disposition to typhus again returns in *old age*. M. Andral observes, (Clinique Medicale, Ed. ii. tom. iii. p. 451, "il resulte de nos observations, aussi bien que de celles des autres auteurs, que c'est depuis l'âge de vingt ans jusqu'à celuï de trente, qu'elles (les fievres continues) sont les plus communes. Depuis trente-cinq ans jusqu'à la vieillesse elles deviennent de plus en plus rares: *puis* après l'âge de soixante-dix ans, on voit reparaître la fièvre adynamique."

But the most extraordinary and interesting fact is, that in this disease of old age, similar as it may be to the typhus of the young, there is the absence of the "dothinenterite." M. Andral observes "chez les *jeunes gens*, sur *dix* cas de fièvre dite adynamique, il y en a *au moins neuf* dans les quels la lésion principale qu'on trouve sur le cadavre est une dothinenterite chez les *vieillards*, au contraire la dothinenterite est rare." Who does not perceive that the typhoid affection of the old is frequently, in fact, a different disease from the typhus of the young?

There is another fact of the utmost importance in this discussion. It is, that *all* the diseases of old persons are particularly apt to assume a typhoid aspect. This is known to all. It has also the particular testimony of M. Andral in its favour. M. Andral having observed, "chez les vieillards la dothinenterite est rare," adds, "mais, en son absence, des symptomes exactement semblables à ceux qu'elle a produit dans la jeunesse, apparaissent avec la plus grande facilité à l'occasion de toute autre lèsion, soit d'une pneumonie, soit d'un simple érysipèle, soit d'un phlegmon leger, soit d'une maladie des voies urinaires, &c. Leur langue se sèche et noircit, &c. In fact, the mere sinking of old age frequently assumes the form of typhoid fever.

Now if the dothinenterite be *peculiar* to typhus, unseen in other diseases, it is plain that all the typhoid cases of old age must be excluded from the number of those in which that peculiar morbid affection is expected.

It is equally plain that merely typhoid cases, at any period of life, must be equally excluded from the number of the dothinenterite: such cases occur every day from harass, anxiety, poverty, &c. They are principally distinguished by being traced to their causes, and are very liable to be confounded with true typhus in hospitals. I have designated them as common fever.

2. But besides this view of the subject, there is another of nearly equal interest. What were the very cases adduced as examples of typhus without dothinenterite? I will briefly examine those given by MM. Bouillaud and Andral.

I shall confine myself to a critical notice of an article at p. 252, t. iii. ed. ii. of M. Andral's Clinique Medicale. It is entitled,

"Fièvres continues sans lésion appréciable du tube digestif."

The first case detailed (xlvi. observation) is headed thus: "Erysipèle phlegmoneux du bras. Symptomes de fièvre adynamique. Langue sèche, etc. Tube digestif sain. Rate volumineuse et molle."

This very heading proves that the case was *not* typhus fever. And, on perusing the detail of symptoms, it proves to be blenorrhagia, accompanied, after several attempts to sound the patient, with inflammation of the prepuce and glans. To these affections succeeded erysipelas of the right arm; then

* See the valuable Treatise of Dr. Tweedie; the works of Andral, Louis, &c.

followed typhoid symptoms. But who does not perceive at once that the case is not typhus?—that, therefore, no dothinenterite could be expected?

The next case is briefly headed thus: "Phlegmon terminé par gangrène autour de scarifications faites à un membre infiltré; fièvre adynamique; langue et dents fuligineuses; evacuations involontaires. Tube digestif sain; rate ordinaire. Affection organique du cœur."

The patient is 53 years of age. Is it not surprising that such a case should be adduced as an instance of typhus occurring without dothinenterite? It is any thing but typhus.

The next case is that of a patient aged 60. The symptoms are typhoid. But the morbid anatomy consists of suppuration in the infundibula of the right kidney. It is not typhus.

The next case is that of a patient aged 50, with an abscess in the prostate, and another in the muscles of the thorax.

The next case is one of a woman aged 81, with latent pneumonia. The next that of a woman of middle age, attacked with apoplexy. The next that of a man aged 81, in whom the brain was transformed into a gelatinous mass. It is sufficient to enumerate these cases to shew that they do not in the least bear upon the question, whether the dothinenterite be constantly present in true typhus: we know that it is absent in all other acute diseases.

The last case adduced in this place is of such intense interest, whilst it shews how little the whole really illustrates the question before us, that I purpose giving it, only greatly abridged. It is a case of *gangrene of the under lip, with the presence of pus in the veins, arising from the bite of a fly.*

A female aged 39, was brought into the Salle-Catherine on the 16th of August, with a gangrenous eschar on the under lip. This woman, who was in good health, and had nursed her infant during eighteen months, was bitten on the lip by a fly in the Champs Elysées, on the 11th. The lip swelled. On the 13th she was seen by M. Piedagnel, who prescribed fomentations and leeches, and who observed a triangular white mark on the lip, like that of a leech-bite. The swelling increased. M. Mirat saw the patient on the 14th, and prescribed the application of caustic potash. There was delirium during the night. On the 16th the swelling was augmented, the pulse rapid and feeble. The eschar was divided, and the potash re-applied.

On the 17th, the eschar, an inch broad, was raised by pus. There was considerable œdematous swelling of the cheeks, chin, neck, and even the upper part of the chest; there was a violet redness, which disappeared under pressure; there were weakness, anxiety, frequent pulse; more delirium; no cephalalgia; thirst; no heat of skin; the belly was soft; there was slight diarrhœa.

In the evening there was great anxiety and suffocation. The throat and mouth were filled with viscid mucus.

On the 18th the swelling had still further augmented towards the thorax; the tongue was dry and black at the back part of the mouth. This patient died at noon.

Several abscesses were found in the swollen parts. The facial and jugular veins, especially the right, were full of pus. Another abscess was found between the great and small pectoral muscles; the brain was a little injected; the left pleura was inflamed; the lung contained numerous abscesses; the spleen was softened.

Still this case is not typhus, nor even typhoid. It is gangrene, with pus circulating with the blood, and the wonted effect of abscesses in various parts. It is interesting to observe that the spleen was soft.

From the whole of this essay, which I have rendered as brief as possible, it appears—

1. That there is a form of common fever, or *synochus*, which arises from harass, anxiety, and other similar causes, which is entirely different from *true typhus*; and, consequently, free from disease of Peyer's glands.

2. That *typhoid* symptoms not only occur in this fever, but in many other diseases, as phlebitis, erysipelas, &c. especially in *old persons*.

3. That when these cases are abstracted, the remaining typhoid cases are principally, but still probably not entirely, *true typhus*, in which the dothinenterite may, however, occasionally be absent, as the rash is sometimes absent in scarlatina, and the pustules nearly so in small-pox.

4. That the symptoms of *true typhus*

even do not arise from the presence of the dothinenterite, but rather coincide with it, arising from one and the same cause; and very probably from the condition of the circulating fluid.

It now only remains for me to explain the object which I have had in laying before the readers of the Medical Gazette these essays. It was to present an *example* of the manner in which I purpose to treat the subject of diagnosis in my forthcoming treatise, a subject obviously far too extensive to admit of being discussed in any satisfactory manner in this place.

I may add another sketch or two of the same kind. But if I should not, the observations already made, and the specimen given, will communicate a sufficient idea of the plan and extent of my undertaking.

P.S.—Since the preceding sbservations were written, one of the cases noticed above has proved fatal. The outlines of the history, symptoms, and morbid anatomy, are the following:—

The patient, in the seventh month of pregnancy, suffered from great anxiety, fatigue, and watching, during the illness of two little children, for upwards of a week. She then became affected with a violent hysteric paroxysm, febrile symptoms, and delirium. These affections augmented daily; the tongue became dark-brown, and dry; the teeth covered with sordes; the pulse frequent, thus losing its force; the delirium more and more violent, and continuous. The powers of life at length failed. I had expressed my opinion that the case, however *typhoid*, was not real typhus. An examination was made, in the most accurate manner, by Mr. Hammond and Mr. Crowdy, of Brixton, Mr. Heming, of Kentish Town, and myself. The brain was free from effusion, or morbid appearance of any kind; so were the viscera of the thorax. The stomach and small intestines, and the colon, were free from any mark of disease; there was no affection of Peyer's glands whatever; the only morbid structure consisted in a very few instances of inflammation, with minute central ulcer, in Brunner's glands, occupying a small space at the uppermost part of the rectum. There were no petechiæ, nor was there the least tympanitic affection of the intestines.

It appears to me impossible that an individual case can establish more facts than this one. The cause, the course of the disease, were known; its typhoid character amply attested; the absence of dothinenterite predicted; and the conclusion that it was not typhus, however similar to typhus, fully established and confirmed.

OBSERVATIONS
ON
ASTRONOMICAL PHENOMENA,

As connected with the Periods of Gestation and Menstruation.

To the Editor of the London Medical Gazette.

SIR,

HAVING prepared the enclosed for the private perusal of some medical friends, I have been induced at their suggestion to enclose you a copy, which, if you think it may benefit the cause of science, I shall be glad to see published in the work of which you are Editor.

* * * * * *

Your obedient servant,
R. J. MORRISON.

Liverpool, Aug. 20, 1832.

The remarkable coincidence between the periods of gestation both in women and in the females of various quadrupeds, and the times of incubation in birds, and also of the period of menstruation in women, with the time taken by the sun and moon to form certain angles in the zodiac, appears to be worthy of more examination than has hitherto been given to the subject. If it shall appear, on inquiry, that there is a constant agreement in the motion of the moon, for instance, with the period which the human female carries her child in the womb, it will give rise to some curious inquiries; but, if an exactly similar agreement be discovered between the motion of that body and the times of gestation in all other animals, and also with the times of incubation in the feathered race, we may reasonably ask, whether these facts are not evidence of some connexion between the two things, however we may be at a loss to discover the medium of such a connexion?

If the fact of this connexion should be established, the certain knowledge it would lead to, on a subject which at present is enveloped in conjecture, would conduce greatly to the interests of physiological science. This consideration has been sufficient to induce me to offer the following observations:

M. Majendie, perhaps the first physiologist of the day, has declared that "the fluids in the human body are to the solids as *nine* to *one*." It may be asked, then, whether if the human body be so vastly full of fluids, we can be reasonably surprised that the moon, which is found to have such extensive power over the sea, should be endued with some kind of influence over them also? May the motions of that orb not have similar influence, though not so evident to our senses, over the air and other fluids, and, if so, why not over human fluids? The answer may be, that the moon's power over the tides is governed by the laws of attraction, but that the power I argue for is hidden and occult, and therefore remains to be proved. But, I ask, what *is* Attraction? and no answer can be given. Philosophy can calculate its laws, but cannot yet explain its inherent nature. In fact we are wholly ignorant of its *modus operandi*. It, therefore, is also a power which is equally occult and hidden, since the knowledge of its ratio is not that of its essence or first principle. And there is nothing to show that a *similar* power, if not the *same* power, may not act upon the fluids of animals, by drawing upwards, or by intermixing, increasing, diminishing, or in some way altering the minute particles of the blood, or other fluids. This becomes conceivable when we reflect that these particles are so minute, that 18 of them may be covered with the point of the finest pin! But the globules of the brain are said, by Majendie, to be *eight times less* than those of the blood! If this fact be connected with the observation that the moon's power is greatest on those unhappy creatures (hence called *lunatics*) when it is greatest on the tides, does my notion of a similar medium of action seem impossible? Is it so miraculous that those stupendous masses of matter which move the mountain wave, should have some power over the numerous fluids of the human body? I think, on the contrary, that, when considered *à priori*, there is good reason to suppose they have; and that even thus far only to look at the question, it affords small space for legitimate ridicule.

M. Majendie asks, "could it be believed, that authors of note have employed themselves in attempting to discover the immediate cause of the periodical illness of females? I submit that it is reasonable that philosophy should inquire into this as well as into other secrets of nature; and as this writer offers no reason, cites no law, raises no argument against the doing so, I presume, of course, that he was aware of none. He adds, "and could it be believed, that they have been attributed to the influence of the moon?" Here we have an instance of that "extreme repugnance to confess ignorance"—to utter, as Gallileo observed, the words "I do not know," which M. Majendie has himself reproved in another part of his work. He could not even allude to the subject without scorn, because he was aware of his own utter ignorance of this "immediate cause."

Another celebrated writer, (*Dewees*, on Midwifery,) after stating, that "the influence of the moon was very early assigned as the efficient cause of menstruation," and, that "this opinion is not entirely exploded at the present moment," declares that "to destroy this hypothesis, it is only necessary to state the fact, that there are women menstruating promiscuously every day of the year, and every hour of that day." Now this fact, on the contrary, seems to me to offer a powerful argument in favour of the hypothesis. The conclusion against it is false, because it is drawn from fallacious premises, which consist in supposing that the effect is to follow from the moon's age, or, in other words, the position she holds as regards the sun; whereas, according to my opinion, formed on observation, it depends altogether on the position she holds as regards that particular degree of the zodiac she was in when the woman was born. And as women are born "promiscuously every day of the year, and every hour of that day," it follows that they may menstruate independent of the moon's age, but yet altogether dependent on the moon's motion.

I shall, before entering farther on this point, examine the question of the duration of the period of gestation in the

human female, since it will lead to that of menstruation.

"The time of pregnancy," says M. Majendie, "is generally *nine* months, or two hundred and seventy days." But Dr. Dewees declares, that "it would seem, from the best calculations that can be made, that nine calendar months, or forty weeks, approach the truth so nearly, that we scarcely need desire more accuracy."

I have quoted these two writers, because I believe they come the nearest to the true period. But it is not a little curious to find them both falling into the same egregious arithmetical blunder, which is that of declaring nine months to be, according to one of them, 270, and according to the other, 280 days, which are 40 weeks. The terms are evidently put to mean the same thing. They do not say that there are two periods, according to the first 9 months and 270 days, and according to the latter 9 months and 40 weeks, but each writer appears ignorant of the actual number of days that there are in 9 months, and treats the two periods as convertible terms. I allude to this, to shew how little the physiology of the day has been able to determine the question.

I will now proceed to point out what is the *true period* of healthy and perfect gestation; *why* it has been generally considered to be 9 months, not by medical men, for the "doctors differ" hereon, but by females in all countries; and lastly, *how* this time depends, as do many other operations of nature, on the joint influence of the sun and moon.

The period of nine months, which it is well known is that on which females calculate, agrees exactly with 273 days during 7 calendar months out of 12; during 2 of the other 5 months, the like period gives 274 days; 2 others give 275 days; and one month only gives 276 days. Thus, there is an average of 273 days and three quarters, which is the real extent of time in 9 months, being the exact amount of three quarters of the year of 365 days: which, therefore, is not what M. Majendie makes it, namely, 270 days; and is still farther removed from Dr. Dewees' period of 280 days.

Thus we have one reason why women observed nine months to be their period, simply because it did actually consist in the majority of cases of *exactly* nine months. Another reason is, that there being 12 months in the year, consisting of 52 weeks, by which some women would count, and they finding their period to consist of exactly 39 of these weeks, or three quarters of the whole 52, (which are just 273 days) they very naturally concluded that *that* period was three quarters of the year; or, if you please, 9 months out of 12. I contend, then, that the general opinion is correct, and that the *true* and *natural* period, or in other words, the *perfect* period of gestation in the human species, is 9 months, which is 9-12ths of the sun's period through the zodiac—which is again 3-4ths of the year of 365 days. This period I establish, not as 270 days —not as 280 days—but as 273 days and some odd hours.

I now come to shew the connexion between this period and the motion of the sun and moon. First, as to the sun. It is obvious, that if the sun pass through 360 degrees of the zodiac in one entire year, he will pass through 270 degrees (which are 3-4ths of the same) in 9 months; he does in fact, then, arrive at 90 degrees, or a right angle, from the place he was in at the time of the conception, just at the period when the pregnancy is completed.

Although I believe that the sun, by forming a right angle to its place at the conception, has some influence in bringing on the birth, yet, as in raising the tides his influence is comparatively small, so is it in this case. The moon is the principal agent in producing both these phenomena. Where then will the moon be found at the period of parturition? This is an interesting question. The moon, at that time, will invariably be found *on the very same spot* she was on just 9 months before—the very same place in the zodiac that she occupied at the moment of conception—just 273 days and 5 hours before; that is to say, she returns to her own place in the zodiac in 273 days and 5 hours; at which time it has been shewn that the sun is at a right angle from his own place at the time of conception.

Let my assertion of the moon's motion be verified. The lunar period, or time of traversing the zodiac from any given point, say the 1st degree of Aries, to that same point again, *is* 27 *days* 7 *hours and* 43 *minutes*. Multiply this by 10, (for I say that the moon performs 10 revolutions during the preg-

nancy) and you have precisely 273 days 5 hours and 10 minutes. Here I may remark that Buffon has stated that the time of a natural delivery extends to "9 months and 4 hours;" and this he did without considering the agreement of this period with the motion of the moon.

I venture to submit to unprejudiced judgment, that this accurate coincidence of the situation of the two luminaries, at the time of parturition, with their relative places at conception, is something more than blind, unmeaning chance. In my humble opinion, it is by means of their joint influence that the fœtus is brought to light. That this influence acts through a medium at present unknown, is admitted; but I contend that this circumstance alone is no conclusive evidence against its existence. The same objection might have been raised anterior to the year 1807, to the now well known cause of combination between acids and alkalies. How many years did an unmeaning belief exist among chemists, that there was a violent *hostility* naturally and perpetually existing between these substances! This, too, was a loose unmeaning term to describe an unknown manner of acting, which became known, however, when Sir Humphry Davy, in that year, discovered that acids and alkalies combine, in consequence of being in opposite states of electricity. Very much of nature is still hidden from the eyes of science. We know not the original cause of attraction, or of vegetable or animal life, or of the needle pointing to the north pole, or of the variation of that needle from this direction. "We are not made to give a reason for every *why*"—this is the privilege of the Deity alone. But we may yet discover, for aught we can determine, the cause of this fact as to the sun and moon having a connexion with the cause of parturition.

That such is indeed the case, will become more evident after I have thrown farther light upon the subject, by other similar facts. Some children are brought into the world at *seven*, some again at *eight* months. Let us examine whether any thing of the same kind occurs at those periods. For the 7 months' child we have 7-12ths of the sun's period; this is 213 days. And I find that the moon, after performing 7 revolutions, comes again to the exact distance of 72 degrees from her own place at the conception, in just 213 days. "Yes," it may be replied, "but what is there in this circumstance—what virtue exists in an angle of 72 degrees, rather than in 77 or 80 degrees, or any other number?" The peculiarity is this, that each one of the particular angles I shall have occasion to remark, will be found to be the exact angle itself, or the supplement of the angle of a *regular polygon*. Thus, 72 degrees are the supplemental angle of a regular *pentagon*. And while the moon forms this angle, the sun also forms an angle of 150 degrees with his place at the conception, which distance is the angle of a *duodecagon*.

Now, as to the child born at 8 months. The sun travels 8-12ths of his course in 243 days and 12 hours, and he then forms an angle of 120 degrees with his place at the conception—that of a regular *hexagon*. The moon, after travelling 8 times round the zodiac, is so far advanced on her ninth revolution, as to be, just 3 hours afterwards, exactly 30 degrees from her original position, which, of course, is the supplemental angle of a *duodecagon*.

These, it will be acknowledged, are remarkable coincidences; but, lest I may not be allowed to consider them as any thing but the effect of mere accident, I must enlist others of a similar nature, to storm the strong towers of prejudice.

It is a striking fact—it is rather a *series* of remarkable facts, that the periods of gestation in other animals coincide exactly, also, with the position of the moon as regards her own place at the moment of conception. This is both strange and unacccountable, if we deny that the planet has any influence on the animal; but easy to understand, if we admit that the moon's position marks the moment, and in fact brings about, by some unknown action, the event of parturition. But to the facts:

The cow and some other animals carry their young nine months; the female rein and common deer do so during 8 months. These periods I have already remarked upon. The mare and she-ass go, according to Buffon, 12 months with young, or 365 days. The moon, in this time, traverses the zodiac 13 times, and returns to a distance of 120 degrees from her original place, forming an angle equal to that of a regular *hexagon*. The sun, of course, re-

turns to his own place. The wolf, fox, and some others, go 5 months, or 152 days, when both the luminaries will be found at the distance of 150 degrees from their places, forming the angle of a *duodecagon*.

The same facts obtain with the elephant, and other large quadrupeds; but with smaller animals, whose period is brief, the sun's motion cannot of course correspond; they seem to depend altogether upon the action of the moon. Thus, the bitch, the Isatis, and some others, carry their young 9 weeks, or 63 days; the moon goes twice round the circle, and returns to an angle of 120 degrees from her place (that of a regular *hexagon*) in 63 days and 18 hours. The rabbit brings forth on the 31st day, and the moon's motion corresponds; for, in 30 days 17½ hours, she forms an angle of 45 degrees from her position at the copulation, which is the supplemental angle of a regular *octagon*. I shall mention only one other quadruped, the common cat. This creature goes 6 weeks with young, or 42 days; at which time the moon arrives at a distance of 180 degrees from her place—exactly half the circle. If these instances be not an argument for a connexion between the planet's motion and the female's state during gestation, I am at a loss to know what they are to be considered.

I shall not here examine other cases, but it may be well to mention, what is not exactly foreign to the subject, that kittens and puppies are known to open their eyes on the ninth day after birth, when the moon will be found to form the angle of a regular *hexagon* as regards her place at the copulation of the bitch; and also that of an *equilateral triangle* to the same place as to the cat. Thus she appears to perfect the condition of the animal by a repetition of the same action, or influence, or whatever we may term it, which first brought it into life.

If this idea of the birth being brought on by lunar influence, when the moon arrives at certain angles with her place at the copulation, be correct, it may be expected that something similar should be observed in the feathered tribe. Nature generally exhibits a consistency of action; and so it is in this case. If we look to the canary, and many other birds, we shall find that they hatch their eggs on the 14th day; and the moon forms an angle of 180 degrees with her place at the commencement of incubation in 13 days and 16 hours. Most birds, however, bring forth their young from the eggs on the 21st day, as does the common hen. Again, we find that in 20 days and a half, the lunar orb arrives at a right angle of her place at the commencement of incubation, which is of course the angle of a *quadrangle*. Some, however, as the young duck and goose, break the egg on the 28th day, when the moon completes her revolution.

I have now gone near to show a most striking and extraordinary agreement between the period of animal gestation and incubation, and the motion of the moon in her heavenly path. I find no case where this planet is not in some position with regard to her original place at the commencement of pregnancy or incubation, which forms *the angle or supplemental angle of a regular polygon, which may be inscribed in a circle*. Nor is there any case wherein she forms the angle of a *septagon*, which may not be so inscribed. It will hardly be believed that this is, nevertheless, all accidental coincidence—having no cause, and generating no effect,—or, that all this is undeserving of a moment's consideration. Surely I may say that it requires great credulity to believe that there is nothing in all this chain of circumstances, than to suppose that the Almighty *may* make use of such an instrument to effect the objects of his will, even though the secret spring by which it operates be not yet discovered by that pigmy, mortal man.

To elucidate the apparently similar connexion between the moon's motion and the periods of menstruation, I shall quote a few observations of Buffon. "There is much uncertainty," he remarks, "on the causes which occasion delivery, and we do not perfectly know what obliges the infant to quit the womb. The flowing of the *menstrua* is periodical, and at determined intervals; and although conception suppresses its appearance, it does not destroy the cause; for notwithstanding the blood does not appear at the accustomed times, yet a kind of revolution takes place. * * * I imagine that when a woman has conceived, the periodical revolution is made as regularly as before; but, as the matrix is swelled, the canals cannot give issue to the blood, unless it arrive

in such quantities as to open a passage in spite of the resistance opposed to it. In this case blood will appear, and if it flow in great quantities, abortion will ensue. When no blood appears, even, as is generally the case, the first periodical revolution" (that is, the time at which the *menses* would have returned, but for the pregnancy,) "is *remarkable*, and felt by the *same pains* and symptoms."

This is a curious fact, and, like the true cause of this periodical illness in females, appears hitherto to have baffled the ingenuity of all medical writers. Dr. Dewees declares, that "hitherto, nothing satisfactory has been advanced upon this curious subject." Now, as all modern hypotheses have fallen to the ground, it may occur, that by reverting to the earliest of all theories, which was "lunar influence," (but which has been misunderstood to be connected with the age of the moon) we may at length fall into the track of truth and nature. It is a fact beyond dispute, that the moon passes the place she was in, in the zodiac, when the woman was herself conceived in her mother's womb, once a month—that is every 28th day; and I have shown, that with few exceptions, (where they have been carried more or less than 9 months) that is also the spot on which the moon was found when the woman herself was born. Now the moon returns to this place, it is ascertained, exactly *in* the same time, and I believe exactly *at* the same time, with the periodical return of the menses. "But," says some medical man, "the illness is sometimes varied; it is occasionally a week earlier or later, &c." Yes, and I have no doubt, that though the moon's return TO HER OWN PLACE is the *general* cause, as it is of the birth of children of nine months, yet the illness does at times supervene when the moon forms the angles of regular polygons with that place, as do deliveries at 7 or 8 months.

If my authority were admissible, I could relate facts I have observed with regard to several females which agree perfectly with this theory. But I shall refrain from mentioning any thing relying on my own evidence, and return to Buffon. "The fœtus," he observes, "generally comes into the world during the *tenth revolution*. At the *eighth revolution*, the fœtus begins to get strong, and may, therefore, come into the world at *seven* months. The fœtus which has acquired this same strength only a little later, will come into the world at the *ninth* revolution. And those which require *nine months* to obtain the same strength, will come at the TENTH REVOLUTION; which is the most common and general term."

It must be remembered, that Buffon did not mean by the term "*tenth revolution*" the moon's revolution in the zodiac, which he in all probability thought nothing about, but that he meant that revulsion of the blood in the female system, which he calls "a kind of revolution;" which evinces its existence by the flow of the menses, and which though not observed during pregnancy, still exists, and it may be supposed aids in nourishing the child, and which secretion, if it do not flow while the woman nurses, goes off in the form of milk. It must also be remembered that exactly at the completion of *ten* of these "revolutions," the moon also completes *ten* revolutions of the zodiac, and this is effected in 273 days, which are 9 months, the common period of pregnancy.

To prove that the cause of delivery is the flow of the *menstrua*, he says, "females of every animal which have no menses, bring forth at nearly the same time, and there is but a slight variation in the duration of their gestation. We may, therefore, suppose that this variation, which is so great in women, comes from the flow of the menstrual blood, which is constantly exerted at every *periodic* return."

Had this writer merely added the words "*of the moon*," he would have fully expressed the doctrine I contend for; which is, that *it is the periodic return of the moon to her own place, or to certain angular distances from her own place, at the woman's birth, which produces this effect—menstruation.*

I shall be prepared to say *why* this is so, or *how* there can be any connexion between the secretion of such a fluid in the human body and the motion of the earth's satellite, if the person who propounds the question will explain *why* the earth revolves on its axis from west to east, in preference of from east to west; or *why* it has one moon more than Mars, or is less in magnitude than Jupiter.

One objection, however, to this theory I will notice. *Why*, it may be inquired,

is not the effect invariable—why are not women always ill exactly on the same day, and exactly at the same hour? The answer may be, that although the moon's motion, as already spoken of, is her average monthly period, yet that period is not always precisely the same, but varies occasionally several hours; also, as it is found that the tides are more potently agitated when the moon is particularly situated,—greater when in perigee, or having north declination, and less when in apogee, or in south declination, so may it be in this case. Again, her position with regard to the sun at the time may be found to have some influence in accelerating or retarding the effect, as it does with the tides; and, these things considered, it will not appear inconsistent, that though the menstrual flux may occur generally when the moon is in one particular place, it may occasionally be observed when she is found three or four days earlier or later, at 45 degrees distance from that part of the zodiac.

P.S.—A curious case of affiliation was reported in the London papers in February, 1832, wherein it was proved, that the child was conceived exactly 202 days before its birth. It is remarkable that it will be found, by calculation, that in 202 days the moon traverses the zodiac *seven* times, and returns to an angle of 144 degrees from her original position, which form the angle of a regular *decagon*.

FURTHER REMARKS ON THE CIRCULATION.

By David Badham, M.D. Oxon.

The reader will recollect that the principal point in which I ventured to differ from Dr. Wilson Philip, related to the action attributed by that distinguished physiologist to the veins, and to the active share which those vessels are presumed by him to take in the circulation. Indeed Dr. Wilson Philip seems to attribute the reflux current of the blood almost entirely to their agency; while I would still refer it (stating my dissent to the conclusions at which he had arrived, I trust, with all possible respect) partly to a "vis a tergo" never entirely lost (however it may have diminished in proportion to the distance of the thing moved from the organ moving it), and partly also to the elasticity of the central organ giving effect to the pressure exerted on all bodies by the atmosphere.

Dr. Wilson Philip had stated, in his former communication, that the suction of the heart, as far as such a power could be supposed to exist, would rather produce a tendency to collapse in the veins than raise the blood through them. I endeavoured to shew that no "tendency to collapse" could obtain, because this system of vessels must be, of necessity, maintained in an equable state of distention by the indraught of as much blood into their distant or remote extremities as is required to fill the right sinus of the heart by immediate derivation from the venæ cavæ.

From this statement, it must be evident that I never could have meant to impugn the accuracy of Dr. Wilson Philip's argument against the elasticity of the heart, on the score of sufficiency in the vis a tergo to maintain the vessels in a state of distention. But when Dr. Wilson Philip adds, that I must be aware "that the power of suction in raising the blood can only operate in proportion to the atmospheric pressure on the external surface of the vessels, and in proportion to the degree in which they resist this pressure; and that, therefore, if the vessels (the veins) can offer no resistance to the pressure, the blood can in no degree be raised by suction," I must refuse my assent to that proposition; for, if I understand Dr. Wilson Philip rightly, he states the power of suction to vary directly in the ratio of the rigidity of the vessels, or (to use his own words) to vary with "the degree in which they resist the atmospheric pressure;" while I should rather presume that the efficacy of this agent would be found in direct correspondence with want of rigidity in the tube (the incompressible quality of the contained fluid being also taken into account): from the first property, the force of the external air will not be expended on a resisting vessel; and from the second, none of that force will be expended in condensing the fluid itself. The veins, indeed, appear to be beautifully constructed for the passive share that has been allotted to them in the circulation of the blood. By their unresisting structure, the blood is far more easily

moved by external pressure than could have taken place had they been differently formed (the disposition of valves where this pressure is applied securing the direction of the current), while the arterial structure, on the other hand, possesses an energy fully adequate (with the assistance of the heart) for the projection of their contents, without requiring any external agency whatever; which agency, indeed, if it took place, might, without valves, impede rather than promote it. But to return to the more immediate object.

Dr. Philip does not refuse the property of elasticity to the heart, but only affirms that this elasticity is "very inconsiderable." Now I really do not see much force in this objection; for if it be allowed that the heart has but just sufficient of this property to dilate its cavities, no more is required. The ready indraught of blood does not depend upon the actual amount of resiliency of the heart, but upon its possession of that quality in sufficient extent to give effect to that atmospherical pressure to which the external circulation is constantly exposed. Allow the heart a power just adequate to expand its cavities, and the tendency to the formation of a vacuum will be as complete, and the result the same, as if the expanding force had been equal to a hundred weight.

2. Dr. Wilson Philip also says, in his last paper, that he had not denied the action of a "vis a tergo" upon the contents of the veins, and even would attribute the engorgement which occurs on tying the vein behind the ligature, partly to this circumstance. I own that I cannot exactly possess myself of Dr. Wilson Philip's idea in this place. The only "vis a tergo," however, which I had in view, was the remaining momentum which I supposed the blood to retain after it had left the arteries and had come into the veins; and this, I think, must have been his notion of it too, in the passage to which I allude, since he says, "the vein behind the ligature was gorged with blood by the combined power of the 'vis a tergo' and that of the vein itself;" thus clearly separating this power from the action which he supposes to reside in the veins. But I cannot see how to reconcile this admission with the following extracts from a former communication from his pen, in which it is suggested, that "since the circulation in the capillaries is independent of the heart, it is evident that the influence of that organ cannot extend to the veins." And again, in summing up the evidence in favour of this view, he concludes, "in comparing the whole of the foregoing circumstances, is it not evident that the motion of the blood in the veins, like that in the capillaries, depends on the powers of the vessels themselves?" These passages seem to deny the existence of that very "vis a tergo" which the ingenious author, in his last paper, explicitly admits.

3. With regard to the notion of the blood assisting to move itself, which I hazarded as a mere query, I contemplated an integral and expansive motion, not a relative change of position in its constituent globules, as I am understood to mean by Dr. Wilson Philip.

It is difficult, by any abstraction, to present to one's own imagination such a picture of the circulation, in the act of its accomplishment by many concurrent forces or consentient mechanisms, as shall fully satisfy the mind; and that part of it transacted by the veins will always be the least prominent part of such an imaginary picture. For all peculiarity of structure there must be a final cause; and as the laborious part of the circulation, so to speak, is evidently thrown on the weaker vessels, (exactly the reverse of that adjustment which the limited views of a mortal Prometheus would have perhaps suggested,) some additional agents are necessarily required: we find them, or some of them, in the elasticity of the heart and the pressure of the atmosphere—that pressure of which the amount is so well known, and the effect so obvious in the emptying of vessels, under the cupping-glass. Had veins carried the blood from the heart, and arteries returned it, what mischief would not have ensued on both sides of the circulation? Yet, though we may here be able, perhaps, as elsewhere, to admire the adaptation of means to an end, it were vain to flatter ourselves that we are in possession of the entire subject, concerning which not a little remains to be investigated. Can we yet assign, for instance, the cause of that varied rate at which the circulation is performed during the first ten years of existence, and of which

we possess a pretty accurate scale, shewing a difference in velocity of at least one-third from the first year to five or six? The fact is constant and well known; and when we shall have arrived at the physical cause, as we suppose ourselves (perhaps very falsely) to have done at the final cause of that fact, we shall perhaps have made a further step towards viewing the important function of the circulation in a more satisfactory manner than, after all, we can presume that we are able to do at present.

I am not sure that it is particularly incumbent on me to notice the conclusion of Dr. Wilson Philip's paper, in which a very intelligent avarice of fame decides him to break no lance with undecorated assailants; yet it should seem no ill compliment to this distinguished writer to accept his experiments at once, and only differ as to the lesson they inculcate. It is clearly competent to every one to state objections derived from the exercise of his reason and reflection, and I trust that in the pursuit of truth, if, on the one hand I feel myself prohibited from paying more than just deference to authority, I shall never be found occupied in the endeavour to abate that reputation which all desire to accumulate, and which Dr. Wilson Philip has so fairly earned.

—— "Neque ego illi detrahere ausim
Hærentem capiti multâ cum laude coronam."

P.S. As the postscript is often alledged to contain the purport of a whole letter, can the phenomenon observed by Dr. Wilson Philip, of a portion of vein near the heart forwarding its blood into that organ when cut off from all forces acting from behind by a ligature, be explained without having recourse to his explanation of a vital action in the vein? I am of opinion that it can; that the vein is entirely passive; and that the elastic expansion of the heart would extract the blood, or other fluid it may contain from the vein, just as if it were an artificial tube placed under similar circumstances. If Dr. Wilson Philip were to make the same experiment (which might be done, I conceive,) on a dead vein, what would be the result?

ON THE EXTENT OF THE INFUENCE OF THE HEART IN THE CIRCULATION.

To the Editor of the London Medical Gazette.

Sir,

On looking over Dr. Marshall Hall's Treatise on the Circulation of the Blood, I find, when bringing proofs of the action of the heart extending to and being perceptible in the veins, that he has omitted to state that this may be distinctly seen in the lung of the frog and toad, when the action of the heart has become enfeebled. This I have repeatedly witnessed, and take the liberty of sending you an account of it, as an objection has been made, in the last number of the Medical Gazette, to the experiment of the ligature applied round the limb; the action of which is, nevertheless, only to take off the power of the heart and diminish the velocity of the blood, so as to render the phenomenon perceptible. In the lung, however, no ligature is necessary. As the power of the heart becomes enfeebled, it is distinctly seen that the motion of the blood first becomes pulsatory in the arteries, capillaries, and veins (the pulsations corresponding to the beats of the heart;) next that it ceases in the veins, then in the capillaries, and lastly in the arteries.

I remain, sir,
Your obedient servant,
Henry Smith.

67, Torrington-Square,
Sept. 10, 1832.

NOTE ON THE TREATMENT OF HERNIA.

To the Editor of the London Medical Gazette.

Sir,

A letter appeared in last week's Gazette, bearing the signature of "A Country Surgeon," who has made some remarks on my work, which was lately reviewed in your excellent journal; and as the writer seems not only to have mistaken my views, but has also given me credit for a whole passage which is no where to be found in my book, I

trust you will favour me with an insertion of this communication at your earliest convenience.

I find in this letter of "A Country Surgeon," after a few prefatory remarks, the following passage. "Mr. C. says, "after having tried the taxis, bleeding, and the warm bath, disregard all those proposals which have been made of *trying* what will be the effect of the tobacco-clyster; whether a bladder of pounded ice will do good, whether a weight left upon the tumor will reduce it, whether a purgative may possibly extricate the gut," &c.

"A Country Surgeon," in his eagerness to criticise, has attributed to me the words and sentiments of the *reviewer* of my book; for if he will take the trouble of referring to the article in question, he will discover that the passage above quoted is not given as an extract from my "Surgical Observations," but conveys only the opinion of the writer of the *review*, of the great danger necessarily attendant upon delay in operating.

With that opinion I fully concur; and although I cannot, like "A Country Surgeon," boast of thirty-five years practice, I have, in half that period of time, so frequently seen the fatal effects of indecision and procrastration, as regards the operation for hernia, as to make me a warm advocate for its early performance.

Respecting the efficacy of bleeding and the warm bath in strangulated hernia, I can state from my own experience that in many cases "the reduction of the gut *was decidedly* effected by these means; and I place greater confidence in their use than in the application of cold to the tumor, though the latter is a very powerful agent, and one which I have strongly recommended.

As this letter, sir, is intended to be merely explanatory, it would be foreign to the purpose to discuss at length the propriety of administering tobacco enemas; but I shall, at any other time, be most happy to enter into controversy with "A Country Surgeon" on this particular subject. For the present I rest content with repeating what has been stated in my book, that I *never* saw an operation for hernia succeed after the tobacco clyster had been given; and that in two instances I have known fatal effects "*decidedly*" produced by its administration, the patients having expired in the state of exhaustion produced by the remedy.

The object of "A Country Surgeon" in addressing you was laudable, if by so doing he thought to remove erroneous doctrines, or prevent their mischievous application; but I may take the liberty of remarking that his opinions would be entitled to greater consideration if freed from the cloak of an *anonymous* communication.

I have the honour to be, sir,
Your obedient servant,
Wm. James Clement.

Shrewsbury, Aug. 27th, 1332.

PELICANUS AQUILA, OR SEA HAWK.

The man of war birds, or sea-hawks, (Pelicanus Aquila) are seldom or never seen far distant from land; the male birds are black, and have a red pouch; the females have a white breast, and are destitute of the pouch. In procuring fish for their food, these birds prefer seizing it from the boobies and gannets instead of catching it themselves. To attain this object, the sea-hawk hovers above the gannet, (which is the bird most usually selected for attack) and darting rapidly down, strikes him on the back of the head, which causes him to disgorge his prey, which is seized by the hawk with an inconceivable rapidity before it reaches the water, and he afterwards soars aloft to look out for another object of attack. It is not an uncommon circumstance to observe a single gannet selected from a flock, and come out to be the subject of attack, as if he had been called by the hawk in preference to the others. The gannet, however, manœuvres to avoid the blow by darting about, lowering himself from his elevation in the air at every dart, and raising his beak in a perpendicular direction, eludes the blow of the hawk from behind, and frequently both fall into the water; the hawk only having the advantage over the gannet when hovering in the air, the latter escapes. At the island of Ascension, where these birds are common, I was informed by Lieut. M'Arthur (Marine Artillery) that the method practised by the hawks to oblige the gannets to disgorge their prey was tried by a gentleman who lately

visited the island; he had seen the attack of the hawk on the gannet, and the successful result. When he visited the part of the island named "the Fair," where these birds congregate in great numbers, he struck some of them with a cane on the back of the head, and the disgorgement of the fish they had swallowed immediately took place.

The use of the pouch in the man-of-war hawk will be an interesting subject for investigation; why it should be found in the male, and not in the female, is curious. One of the officers at the island of Ascension replied in answer to one of my inquiries, that the pouch was larger in size during the breeding season. The adjutant bird of India has also a pouch, which has been the subject of a communication from Dr. Adams, published in the Transactions of the Medical and Physical Society of Calcutta, but his hypotheses are very inconclusive. My friend, Mr. Rooke, mentioned to me at Oahu (Sandwich Islands) that he had seen these birds on the reefs, and on his approaching them, they were obliged to disgorge a quantity of half-digested fish before they could rise; they then inflated the pouch to a large size, and running along to windward, soared in the air. The inflation of the pouch is somewhat contradictory to the knowledge we have of the anatomy of the part: I merely mention it as it was related to me. The opinion I am inclined to adopt is, that the pouch may be used during the breeding season to secrete or prepare food for the young: for the present, however, we must consider its use as unknown.

George Bennett,
F.L.S. M.R.C.S. &c. &c.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

Reports of Medical Cases. By Dr. Bright. Vol. II. Part II. Price 9*l.* 9*s.*

[Concluded.]

Phenomena and Causes of Epilepsy.

Dr. Bright does not pretend to give a complete history of this disease, but confines himself to a general outline of "the most prominent varieties, or the chief points relating to the history of the disease." The more common periods for its invasion, according to the experience of our author, are about seven or eight years of age, or during the second dentition, and shortly before, and for a few years subsequent to, puberty; but he has also known several instances in which it first displayed itself between the ages of thirty and forty, and "not a few" in which the first fit has taken place in persons above sixty. That the circumstances which determine the period of the first invasion are, to a certain extent, accidental, is abundantly probable; but Dr. Bright, with apparent justice, alludes to the general history of the invasion above pointed out, as connected with the delicate and irritable state of the nervous system in infancy, the excitement of puberty, and the excesses and exposures which often follow it in manhood. Lastly, the failure in advancing life of the vigour of the frame, offering less resistance to the inroad of any disease to which predisposition may exist. The ordinary, and even the extraordinary phenomena exhibited by epilepsy, are too well known to warrant us in following Dr. Bright through all the parts of his sketch, which we shall content ourselves with remarking, bears evidence of being taken from nature.

With regard to the pathological condition on which the disease essentially depends, the following observations are made; and it will be perceived that they are at variance, as to the morbid anatomy, with the dissections of the Wenzels, who found the pituitary gland diseased in a very large proportion of cases. Dr. Bright, who gives details of numerous post-mortem examinations, only mentions one case in which this portion of the contents of the cranium was found in a morbid state, though his attention appears to have been directed to this point; indeed he remarks, in another case, that the pituitary gland was "rather small."

"Such being a short sketch of the varied history of epilepsy, we are naturally inclined to ask, on what does the disease depend? That it is the result of nervous irritation, the whole character and course of the symptoms,

and the mode in which they approach and subside, afford the most complete evidence; but it still remains to be discovered what is the essential nature of that irritation, and in what way it differs from the irritation which produces hysteria or chorea, tetanus or hydrophobia; and whether that difference depends upon the degree of irritation, the mode of irritation, or the portions of the brain or nervous system irritated. Unfortunately, upon these points, our investigation of the morbid appearances afford us but imperfect information. The tangible source of irritation is often within the brain itself, or its membranes, or its bony parietes: at other times we are unable to trace any other appearances than such as would mark a degree of congestion in that organ. Sometimes we infer from the symptoms, or deduce from appearances after death, that the attack has depended upon some distant source of irritation, as the uterus, or the intestines; and there is still so great a similarity between those attacks produced by distant irritation, which leave scarcely a trace upon the brain itself, and those which depend immediately on cerebral disease, that we must suppose that the state to which the organ is brought, in order to produce the attack, is nearly the same in both. It is probable that there is an original formation of the brain, which renders one individual more liable than another to the irritation producing epilepsy; that in such brains, comparatively slight irritations, and such as would produce little disturbance under ordinary circumstances and a more healthy original organization, give rise to the epileptic attack; but that there are sources of irritation so overwhelming that scarcely any brain can withstand them; such, for instance, as important changes in the skull or in the brain itself and its membranes; and when once that irritation has been excited by any cause whatever, the brain becomes more liable to its renewal; and I believe that almost always, during the epileptic paroxysm, either as a cause or an effect, sanguineous congestion takes place within the brain.

"As far as I have been able to infer from my own observation, I should say that the organic causes of epilepsy, connected immediately with the brain, are more frequently such as affect its surface than such as are deep seated in its substance. Thus we find that morbid growth, taking place in the skull, shewing itself by a thickened heavy state of the bone, or by a roughened surface either internally or externally, or a remarkable prominence in the natural projections at the base, is often associated with epilepsy. Slow changes, producing a thickened condition of the membranes, will not unfrequently be found attendant upon epileptic attacks. Tumors pressing on the surface, or amalgamated with the cineritious substance, will also be found in cases of epilepsy: and these observations connect themselves in some way with the hints thrown out at page 46 and 331, respecting the apparent dependence of spasmodic action, in many cases, upon injury done to the cineritious substance. It is an idea entertained by Dr. Foville, that the cineritious is the more active part of the brain generally, with regard to all its functions; and that the medullary part is more particularly employed in the conveyance of the motions and sensations, or whatever else may be acted upon or produced, in the cineritious part. And supposing for a moment this to be the case, we might expect that lesion of the cineritious substance would produce disordered action in that part; and that such action might be transferred to the distant parts of the body, producing disordered and involuntary motions: whereas, if the great injury were done in the substance of the brain, the means of communication with the active part being cut off, paralysis might result, more or less mingled with convulsion, in proportion as the cineritious substance is more or less involved."

In the treatment of epilepsy, Dr. Bright urges the necessity of ascertaining, as far as possible, whether it depend upon functional or organic mischief; a point which, it will be remembered, is dwelt upon in like manner by Dr. Pritchard, and which, though of very obvious importance, is practically too much lost sight of. Where organic changes are supposed to be going on in the brain, every effort must be made to promote an equal and regular distribution of blood—especially avoiding whatever is calculated to decrease the circulation of the suffering part: open bowels, local depletion, and counter irritation, are the chief means to be employed. If organic disease do not appear to be

present, more is to be hoped for from improving the state of the nervous system by mineral tonics—as zinc, iron, arsenic, and silver, combined or alternated with the diffusible stimula, and employed conjointly with the shower-bath.

The *convulsions of children*, in the opinion of our author, can scarcely be looked upon in any other light than as epileptic paroxysms, but at the same time of a nature which, as they are often quite independent of any organic disease, frequently pass away without having any tendency to their recurrence in after life; the general irritability of the infant being such as to admit of convulsive paroxysms, from causes which the vigour which is afterwards acquired in adult age is able to resist.

"The exciting causes of infantile convulsions are very numerous. I have known an instance in which the effusion of nearly two ounces of blood on the surface of the brain during birth, gave rise to incessant convulsions, which destroyed life in about twenty-four hours. Tumors in the brain, and effusion into its cavities, will also produce convulsions; but much more frequently vascular congestion is the exciting cause: thus in pneumonia, in the bronchial affections of infants, and in hooping-cough, the congestion within the head seldom fails to produce convulsions. The excitement of febrile diseases,—as in the coming on of small-pox, or in the course of scarlatina,—is the very frequent cause of convulsion. The irritation of teething and of worms, or even of burns and external injuries, will induce convulsions, as will also sudden alarm. Another cause of convulsion has been sometimes traced in the deficient supply of blood to the head, as in cases of inanition and exhaustion, resembling the epilepsy induced occasionally in adults by bleeding.

"These exciting causes of the convulsions of children will be perceived to resemble very much the usual exciting causes of epilepsy,—allowance being made for the irritability of the infant's frame; nor are there any circumstances in the progress of the attack which enable us to draw a distinct line between the two conditions, except the very frequent occurrence of convulsions in children, without leaving either a tendency to return in after life, or any trace of the attack when it has passed away. I have seen children for six or eight hours convulsed and senseless from abdominal irritation recover completely in a few hours, and grow up to manhood without a symptom of disease. I have known a child, from the time it first began to cut its teeth, suffering unceasing convulsions, despaired of from day to day, and from week to week; yet, after the lapse of several months, recover completely on the appearance of its molar teeth. It does, however, on the other hand, occasionally happen that repeated convulsions impair the faculties, or are followed by a tendency to epileptic seizures during the remainder of life, or leave traces of paralytic infirmity of a more or less durable character. In such cases it is probable that slight lesions have been produced in the brain during the excessive congestion of the convulsive paroxysm, or that the vessels have been so distended as to be unable to recover their healthy tone and condition."

Tetanus and hydrophobia are the diseases which follow next in order; and though numerous well detailed cases are given, yet as the great secret of the method by which the symptoms are to be removed remains still undiscovered, we do not think it necessary to enter upon the subject minutely. Dr. Bright expresses his belief, that as there are functional diseases, and not necessarily attended with organic change, they are, therefore, not "beyond the sphere of rational hope." The general indications are afforded by the consideration that the increased irritability depends upon diminished tone, and is apt to be accompanied by congestion. Depletions, employed with caution, on the one hand; and mineral tonics, (as iron, silver, &c.) with vegetable narcotics, or stimulants, on the other, are the most rational means by which such indications may be accomplished, or at least their accomplishment attempted.

A considerable number of "additional cases" are introduced after the chapter on hydrophobia, and are intended to illustrate the various subjects treated of in the preceding parts of these volumes. In another edition, they will, we presume, be introduced in their proper places: meantime, we extract two which bear upon an important practical point:—

Paralysis connected with irregular Menstruation.

"Sarah Clarke, aged twenty-four, a woman of spare habit of body, was admitted under my care early in February 1831, affected with imperfect paralysis of her extremities. She had been married eight years, but had never borne a child. The catamenia had commenced at the age of sixteen, but had never been regular, the periods being marked generally by great pain in the loins and thighs for a couple of days, and a slight trace only of sanguinolent secretion, and she was always subject to a leucorrhœal discharge. Three years ago, the right foot had been affected with numbness and decrease of sensibility, and of the power of motion, for which no cause could be ascribed, unless it depended on a slight injury she thought she had sustained in the back three years before. The numbness extended as far up as the knee, and continued without change for a year and a half, in spite of blisters being applied to her loins, and other remedies being tried. At this time the numbness suddenly passed to the left foot, leaving the right quite well, and gradually crept up the leg and thigh reaching the loins, where pain was occasionally experienced. For the last fortnight before her admission, the right foot was slightly, and the hands also occasionally, affected with numbness; and when she came into the hospital, both the left and right inferior extremities were numb as far up as the knees, with dulness of sensation, and imperfection in the power of voluntary motion, so that she was unable to walk on an uneven surface. She complained of some pain when pressure was made, or when a hot sponge was applied about the fourth and fifth lumbar vertebræ; there was some tenderness of the right side. Pulse 72, full and soft; tongue white and moist; bowels irregular; pupil large. She had slight palpitation of the heart, was flatulent, and apt to fall into fits of weeping; but she had no globus, nor any decided fits of hysteria.

Applicetur Emplastrum Cantharidis inter scapulas. Habeat Pil. Aloes cum Myrrh. gr. xv. omni nocte. Sumat Infus. Cascarillæ cum Ammoniæ Subcarbonatis gr. v. ter die.

The same treatment, with repetition of the blister, was continued.

March 31st.—The strength of her legs was so greatly increased, that she was able to walk about as if in perfect health: the pain in her loins was gone, and nothing but a leucorrhœal discharge remained; she therefore left the hospital qnite convalescent.

Paraplegia connected with suppressed Catamenia.

Eliza Collins, aged nineteen, was admitted under my care into Guy's Hospital, March 9th, 1831. We learnt that the catamenia had appeared once when she was twelve years of age, but never since that time. Her appearance was robust and rather full; but she stated that she had not enjoyed good health, and for six months had suffered pain in the head, and vertigo, with occasional sickness. About three weeks before, she began to feel great pain in the lower extremities, and they became paralytic, so that she was quite unable to support herself, although she could move her legs as she lay in bed; their power of sensation was also greatly diminished.

Habeat Pilul. Aloes cum Myrrh. gr. xv. omni nocte.

11th.—Habeat Misturæ Ferri comp. ℥ss. ter die. Repetantur Pilulæ.

15th.—Applicetur Emplastrum Cantharidis lumbis.

18th.—The blister has discharged very freely, and the bowels act three times daily. All pain in the legs is gone, but there is no return of power.

22d.—Repetatur Emplastrum Cantharidis lumbis. Repetantur Mistura et Pilulæ.

25th.—Strangury from the blister.

Habeat Infusum Lini pro potu. Repetantur Medicamenta.

April 4th.—Applicetur Emplastrum Cantharidis lumbis; et repetantur Medicamenta.

8th.—She has complained of pain in the head, with decided hysteric symptoms; and has had severe cramp in the right leg.

Applicetur Emplastrum Cantharidis nuchæ; et sumat Spirit. Ætheris Sulphurici ♏xx. ex Mistur. Camphoræ pro re nata. Repetantur Medicamenta.

13th.—She is able to stand, and make a slight progressive motion.

May 5th.—Her progress has been constant and decided; still, however, she cannot walk without some support.

31st.—Left the hospital able to walk without support, and decidedly convalescent.

The work concludes with "a concise statement of the diseased appearances of the brain and its membranes, which consists of an elaborate and able digest of the morbid anatomy of those parts, with particular reference to the cases, and plates in illustration, which constitute so essential a part of these volumes. The essay, however, is of a nature to defy analysis, and is greatly too long for extraction: we must, therefore, content ourselves with having referred to it.

We thus bring to a conclusion our

account of this work, which cannot fail to constitute a standard authority on the subjects to which it relates. It has been our object, in the succession of articles which we have given, to lay before our readers the most interesting portions of the work which admitted of being copied into our pages, or of being given with sufficient perspicuity in the form of digest. The expense of the volumes will necessarily prevent them from being found in the studies of so many as their intrinsic value would render desirable; but we would advise that all medical schools, and reading clubs for professional men, should be provided with them, so that they be rendered generally accessible.

In this way, any one who takes the pains to go over the subjects successively, as we have given them in this journal, and who then embraces the first opportunity of examining and studying the plates in the work, will derive as much information regarding them as he can do without actually possessing the volumes themselves.

EXHIBITION OF COLD WATER IN CHOLERA.

To the Editor of the London Medical Gazette.

Abbey-Square, Chester,
Sept. 6, 1832.

Sir,

In the Gazette of the 1st instant appeared some highly interesting observations, by Dr. Hardwicke Shute, on the good effects of a *free and unrestrained* exhibition of cold water in "cholera." The ingenious simplicity of the doctor's speculations interested me exceedingly, and, perhaps, more than they otherwise would from the circumstance of having myself witnessed a case in which extraordinary quantities of cold water were taken, *nolens volens*, by the patient, with, I now verily believe, decided benefit.

The case occurred, towards the latter end of June, 1832, in the person of a man, aged about 50 years, and of very intemperate habits: the disease was promptly attacked with the most active and energetic remedies, but, notwithstanding, the patient continued to grow worse till 4 P.M. (twelve hours after his seizure,) when little or no hopes were entertained of his recovery. At this time he could not be persuaded to persist in the medicines prescribed for him, but was constantly calling for tumblers of cold water, which he kept drinking and rejecting with astonishing rapidity. I was forcibly struck with the eagerness, indeed I may say the relish, with which these copious potations were swallowed; and, under the impression that it mattered little what was administered, as his hours were apparently numbered, he was suffered to repeat them as often as he chose, which he continued doing at short intervals till early the following morning, when, to our great surprise, a manifest amendment had taken place, and he slowly recovered. Severe consecutive fever ensued, attributable, I have no doubt, to the previous unsparing administration of stimulants, both internally and externally.

Since the occurrence of this case, I have never hesitated to allow the cholera patients that have fallen under my care to quench the excessive thirst that generally attends the complaint with plentiful draughts of toast and water, but of course never thought of employing it as an exclusive curative agent. The merit of this suggestion rests entirely with Dr. Shute; and should you consider the preceding facts in any degree illustrative of the efficacy of the cold water practise, their insertion in your impartial and scientific hebdomadary, will much oblige,

Your constant reader,
R. Roberts.
Surgeon to the Lying-in Charity, Chester, &c.

P.S.—Might not additional benefit be obtained by employing at the same time injections of cold water per anum?
R. R.

COLD WATER TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

At your suggestion I have reduced the cases occurring at the St. James's Cholera Hospital to an abbreviated tabular form, which I send you, should

you deem it worthy of insertion in your journal. You are doubtless aware of the prejudices existing against Cholera Hospitals in the public mind, and that therefore few patients are brought into them, unless their cases be of a very severe nature: indeed, they are sometimes received in a moribund condition, and hence with but little hope of benefit from treatment of any kind. Permit me now to offer a few remarks explanatory of my views regarding the treatment of cholera.

It appears to me to have been generally assumed, that it is absolutely necessary to control what I have been led to think is uncontrollable. I believe also that reaction is *best* established by those means which nature points out for its accomplishment. Now the value of cold water, in my opinion, extends no further than in assisting reaction, as suggested in the several propositions contained in the letter which I addressed to the Central Board of Health: it must, too, I think, lessen the dangers of subsequent congestions and inflammations. For example, if, speaking of inflammatory fever, I propose,—that the patient being oppressed with thirst, and the blood being altered in its properties, so that when received from a vein into a bason it possesses the power of coagulating more slowly and firmly than in its natural state, cold water largely received into the system as a diluent, has a direct tendency to reduce the blood to its normal condition: while I act on this proposition, I do not neglect the use of other remedies, which the nature of the symptoms may seem to require. It is dangerous and mischievous to allow prejudice to warp the mind in favour of any exclusive remedy.

The cases which follow I have arranged according to the ages of the patients, and it may be remarked, they have all occurred since the 8th of July.

RECOVERED.

	Age.		Age.
Geo. Hardwick	6	*W. Chalkwright	30
*Sarah Harrison	7	*Chas. Standring	30
Marg. Lidgate	9	Sarah Thomson	35
Eliza Coy	10	*Mary Harrison	36
Henry M'Carthy	17	Louis Jacomo	39
Ann Coffee	21	*Ann Pollock	40
Dan. Caulwell	22	*Ellen Ellis	50
*Isaac Shed	24	*John Poole	50
John Chandler	25	*Mary Drew	66
Eliza Thomson	26		

DIED.

	Age.
William Dixon	77
Mary Priest	76, died on the 8th day.
Eliza Sherridan	75, died 8th day.
Mary Mills	72
William Price	72
Mary Evans	70
Mary Drake	68
William Ainsley	67
M. Stubbleday	59
Mary Jenkins	56, death in 6 hours.
Rachel Wood	55, death 8th day.
*John Pickering	55, death 4th day.
Edw. Barrow	52
Ellen Ross	41, death in 6 hours.
*Eliza Bell	33, death 7th day.
*Marg. Gardener	29, death 8th day.

The age and consequent infirmities of the eight first precluded all rational expectations of recovery from so severe a disease. Jenkins and Ross were moribund when admitted. Pickering died of irritative fever consequent on diseased prostate and a very narrow stricture, reaction having commenced very favourably. Gardener was the victim of circumstances combined with the disease: during the commencement of reaction, she was brought from a neighbouring parish in a common sitting chair: on the following day she miscarried, and gradually sank.

Now if all the circumstances of these cases be taken together, and if it be considered that reaction was the particular point to which I wished to direct attention, the success of the treatment may appear to sanction the opinions I have ventured to advance. In all the cases, water was taken *ad libitum*: those marked with an * took nothing but water till reaction was established: the other cases were treated with calomel, for they fell under my care before I had reason to doubt the value of that medicine.

I have the honour to be, sir,
Your very obedient servant,
JOHN GEO. FRENCH.

St. James's Infirmary,
Sept. 12th.

PURGATIVE MEDICINES IN CHOLERA*.

To the Secretary of the Central Board of Health.

SIR,

HAVING attended, in conjunction with

* Communicated to the Central Board of Health.

Mr. Day, the cases of cholera which have lately occurred at Barton, in the county of Nottingham, I beg leave to offer, for the consideration of the Central Board of Health, one or two improvements in the treatment of the malady, to which we have been led by an almost incessant attendance on every severe case, and by diligently observing the effects of the different remedial measures employed.

The subject to which I would first solicit the attention of the Board, as being one of vital importance, is the propriety of using in the early stage of the disease, and even after collapse has commenced, the most powerful purgatives, instead of attempting to restrain the action of the bowels by opiate and other astringent medicines. In every one of our fatal cases, we had been able both to check completely the purging and vomiting, and to raise the body to its natural temperature; but no sooner had we effected these objects, than the respiration invariably became laborious and impeded, and death in a very short time ensued. Reflecting on these facts, we were led to consider whether, by the exhibition of large doses of purgative medicines, our practice might not be rendered more successful. To the adoption of such a plan of treatment we were besides encouraged, by certain appearances discovered on inspecting the body of a patient who died of the disease on Saturday the 11th ult. The stomach and bowels were found filled, almost to distention, with the peculiar serous fluid of which both the vomitings and the stools during life had consisted; and the mucous membrane of the intestinal tube, throughout its whole extent, was covered with a thick coating of muco-albuminous effusion. From these facts, taken in conjunction with the ones already mentioned, it appeared evident that it was a fatally erroneous idea to suppose that the danger of the disease was diminished, when the purging and vomiting were restrained. The true indication of cure, on the contrary, seemed to be, to assist the natural efforts of the system to expel the accumulated contents of the stomach and bowels. The thick coating with which the mucous membrane was covered, shewed that, before it could be properly acted on, a large dose of some strong purgative would require to be administered.

Impressed with these truths, we resolved, whenever another case should occur, to give the purgative plan of treatment a full and a fair trial. An opportunity soon offered itself. On that very night we were called to a child, aged seven years, whose father, mother, and sister had all, within the space of four days, died of the malady. She had been indisposed during the whole of the day, but had refused to acknowledge her illness; and she was then rapidly sinking into the stage of collapse. An emetic was immediately given, and as soon after its operation as the stomach was quieted—which object was speedily attained by placing the patient's feet in hot water, and by administering a few drops of the black drop in peppermint water—a large dose of calomel was exhibited. This brought away during the night several copious rice-water stools, after each of which our little patient continued for a considerable time greatly relieved from the indescribable feelings of distress which are usually attendant upon this awful disease. About seven o'clock on the following morning, the stomach, which had remained easy during the night, became again affected with nausea, on account of which symptom half a grain of opium, combined with two grains of calomel, was given with the desired effect. Two hours afterwards, respiration becoming very laborious, and symptoms of collapse again making their appearance, five grains of calomel were administered. This was followed by a plentiful dejection, in which it was remarked that there was contained a very large proportion of the flocculent matter, an appearance which experience had taught us to regard as highly favourable in its import.

Between 10 and 11 o'clock, the patient was seen by Dr. Blake, of Nottingham, the author of the valuable essay on Delirium Tremens, who concurred in the propriety of giving another dose of calomel, together with a drop of the croton oil; and who prescribed, in addition, a drachm of tartarized soda, to be taken in an effervescing draught every half hour, till the bowels should be once more thoroughly acted on. In the second evacuation which was passed after these medicines were taken, a slight tinge of bile could be discerned; and in the succeeding one there were seen floating on the surface, several little round

lumps of mucus, the shaggy surface of which was covered with bile. A striking change was now visible in the patient's countenance. The lips had become quite red; the cheeks and forehead had lost much of their dingy, leaden appearance; and the eyes did not seem so deeply sunk as they had previously done. The voice, too, was more natural; the pulse was not merely perceptible, but somewhat full and regular; and respiration was performed with comparative ease. Thus she continued until near 6 o'clock in the afternoon; about which time she suddenly began to sink, and very soon she appeared to be moribund. From this state, however, we succeeded in raising her, merely by giving, every now and then, when she was capable of swallowing it, a teaspoonful of arrow-root with brandy. Subsequently, she fell into typhus fever, under which she still labours.

I have entered pretty fully into the detail of the above case, because I feel thoroughly convinced, that, had it not been for the system of active purgation which was followed in it, the little patient never could have got through the stage of collapse into which, when we were first called to her, she had already begun to enter.

Another case has since occurred in which the purgative plan of treatment, owing to its having been instituted at an early stage of the disease, appeared even to greater advantage.

The next point to which I would advert is the immense importance of upholding the patient's strength, which cannot but be greatly exhausted by the excessive secretion into the alimentary canal. This was well exemplified in the case just related, in which death must undoubtedly have very soon ensued, had we not, by cautiously and perseveringly supplying the organs of nutrition with materials to work upon, tempted them to resume the exercise of their functions. Indeed, I have no doubt that numerous lives have been lost merely from this one essential particular not having been duly attended to. I would, therefore, strongly recommend that in every case the patient should be allowed, from the very commencement of the disease, as much chicken-broth as he pleases. By this means the operation of the purgative medicines would be encouraged; the intense thirst, which is one of the most distressing symptoms of the malady, would be relieved; the bowels would be soothed, and induced to renew their office of absorption; and thus the system would be enabled to bear up against the violence of the disease. This measure has in its favour the recommendation of Sydenham,—an authority of no small weight in any thing relating to practice.

While we continued to look upon the purging as a very formidable symptom, we were in the habit of employing a mixture which Mr. Day had long found singularly efficacious in the common cholera. It is composed of one drachm of nitrous acid, from 40 to 60 drops of laudanum, and half a pint of camphor mixture. The dose is two or three table spoonfuls every second or third hour. This medicine we still continue to use, not because it has a tendency to check the purging, but because it seems to exert a very beneficial influence on the lining membrane of the intestines, probably by its action on the thick muco-albuminous coat with which it is covered. We have thought too, that, in conjunction with the nitro-muriatic acid bath, it has had considerable effect in re-establishing the biliary secretion.

When I speak of the above measures as having proved eminently advantageous, it is not, of course, my intention to represent all others as either useless or injurious. There are several, on the contrary, which may, under certain circumstances, be not merely admissible, but absolutely necessary. Bloodletting is one of these; but of its effects, when employed in union with the purgative plan of treatment, I cannot speak from experience. The propriety of giving stimulants too, even while abstracting blood and administering purgative medicines, is unquestionable.

In venturing to lay before the Board the above observations, I have been actuated solely by one motive—the wish to procure a sufficiently extensive trial for measures which, both from reasoning and from experience, I conscientiously believe to be extremely beneficial. Some of these measures may not be altogether new, but that is a matter about which I do not concern myself. I see in every newspaper, in every medical periodical which I take up, methods of cure the most absurd, or the most pernicious, confidently recommended to the public. I consider it, therefore, a sacred duty which I owe to humanity, to come forward and make

known a plan of treatment from which we have experienced the most excellent effects. In doing this I have mis-stated nothing—I have kept back no important fact—I have explained the grounds upon which our practical conclusions have been founded; and, should further experience not confirm the truth of the principles laid down by me, I shall not be backward either in rejecting them myself, or in persuading others to do so.—I am, sir,

Your most obedient servant,

JOHN M'DIVITT.

Kegworth, Leicestershire,
18th August, 1832.

Council Office, August 20, 1832.

SIR,

I am desired to thank you for your communication on the use of purgative medicines in cholera, and to request that you will be kind enough to state, at your earliest convenience, the particulars of your second case, with the termination of your first case; as also, whether you have witnessed the effects of the drastic plan in any other than the two cases alluded to.

You will please to address all future communications to this Board, as directed on the cover.

D. B.

To John M'Divitt, Esq.
Kegworth, Leicestershire.

To the Secretary of the Central Board of Health.

SIR,

In reply to your letter of the 20th ult. which you did me the honour to address to me, I regret to have to state that both the cases of cholera in which I represented the purgative plan of treatment to have proved so beneficial, have since terminated fatally. The child (Chamberlin) whose case I detailed in my former communication, and who, I said, was still labouring under typhus fever, consequent upon the malady, died at seven o'clock on Sunday morning last. For three or four days previously, there had been symptoms of cerebral congestion, for which a blister had been twice applied to the nape of the neck, and other suitable remedies used. On Saturday morning she began to reject by vomiting whatever was given her, whether food or medicine; and on the evening of that day the woman in attendance upon her remarked that when drink was offered to her, her hand wandered widely from the cup which she attempted to grasp. Early on the following morning she was seized with a severe rigor, which she did not long survive.

The second case was that of Thomas Glover, aged two years and eight months. This child was attacked on Monday, the 13th ult. about eleven o'clock, A.M. and was seen by both Mr. Day and me a few minutes afterwards. The purging and vomiting were violent; and, short as had been the interval from the moment when the little patient was first discovered to be ill, marks of deep-felt distress were evident in its countenance. A *hippo* emetic was administered on the spot, and, when its operation had ceased, the child was placed in a warm-bath. This measure having failed completely to tranquillize the stomach, three drops of the black drop were given in a tea-spoonful of peppermint-water. This was followed, in the course of fifteen or twenty minutes, by an eight-grain dose of calomel, which was repeated an hour afterwards. At first the dejections consequent upon the exhibition of this medicine, consisted merely of the rice-water fluid so often mentioned as characteristic of the malady. Towards night, however, they began to alter; some traces both of bile and of fæces making their appearance; and on the following morning they were of a bluish slate colour. The condition of the little patient was now wonderfully improved. It was apparently quite free from pain; and except that the forehead had not yet quite lost its dingy leaden hue, and that the eyes, heavy and sunken, and having the under half of the conjunctiva completely covered with a net-work of injected blood-vessels, an appearance first pointed out to us by Dr. Blake, and which we have never yet found wanting in any genuine case of cholera, still retained a livid circle around them. There were no symptoms present by which even an experienced person could satisfy himself that the child had been, and indeed still was, labouring under cholera. On the afternoon of this day, the nitro-muriatic acid bath was used, with considerable benefit; and, towards night, the evacuations from the bowels having ceased, while the abdomen felt full, and

the respiration was laborious, another dose of calomel was given. Several copious stools, containing an abundance of bile, were passed during the night; and on the following morning we found our little patient in a state which might have well justified us in entertaining the most confident hopes of his recovery. From this period until Sunday last, (the day before that on which he died) there was not a single symptom which could lead us to anticipate an unfavourable result. All that we did, therefore, was to endeavour to keep up a healthy condition of the hepatic and intestinal secretions by a small dose of the hydr. c. creta, given every night and morning; and by a little saline aperient medicine, administered twice or three times a-day. On Sunday morning the little patient appeared drowsy, and on being roused, was exceedingly peevish and irritable. Towards evening coma came on, and the eyes were turned up, leaving nothing more than the under half of the conjunctiva visible. During the night two or three convulsive fits occurred, and on the following morning the child expired.

It will readily, I think, be admitted that the fatal termination of these two cases cannot, in the slightest degree, detract from the merits of the purgative plan of treatment. In neither case did death take place before the eighth day, long before which time the original malady might very fairly have been said to have entirely ceased. The true cause of death, in both patients, was cerebral congestion, an affection which, in such young subjects, is very likely to supervene upon any violent disease whatever. But any comments on the facts of the cases are, I believe, unnecessary.

I shall now briefly detail a case which was under treatment at the date of my last communication, and to which, as the event was doubtful, I did not then allude.

Mary North, aged 78 years, grandmother to the last child, had been in attendance upon it from Monday, the day on which it was attacked, until Wednesday night. She then went to her own house, where she remained during the next day much indisposed, and affected with diarrhœa. On Friday morning the symptoms of her illness becoming alarming, we were called in. We found her vomiting, and suffering from cramp about the heart and stomach. The pulse was exceedingly feeble, and her skin was partially covered with a cold clammy perspiration. A little brandy and hot water were given, and very soon afterwards a scruple dose of calomel. When dread was no longer entertained of the calomel being rejected by the stomach, a little more brandy and hot water were allowed, and the patient was permitted to drink freely of any warm, bland, and nourishing liquid. In about three-quarters of an hour, she passed a very large watery evacuation, which was followed by others at different intervals during the whole of the day. The patient was not a little surprised to find that, instead of being rendered weaker by these evacuations, she felt as if she gathered strength from each. She passed a pretty favourable night, and on the following morning took, with great pleasure, another dose of the calomel, which consisted, however, only of ten grains. On Sunday morning she felt quite relieved; her stools had become of a natural colour, and she complained merely of debility. For this, gentle tonic medicines were prescribed, and she is now, I am happy to say, convalescent.

It will be remarked, that in this case, as well as in that of the grandchild Glover, calomel was the only purgative employed. Our reason for this was, that both patients were exceedingly feeble, and we had an opinion that calomel, while it would prove sufficiently active in its operation, would be considerably less irritating than any other energetic purgative. Under other circumstances we would have joined, as an auxilliary to the calomel, the croton oil.

Besides the cases above related, we had, within the last few days, two others. Of these, one was very mild, and the patient, without owing much to medicine, is already convalescent. In the other, which was an exceedingly severe case, we are at this moment making a further trial of the plan of treatment by purgatives. Should the Board not think me tiresome, I shall probably trouble you with the result of this case.

I have the honour to be, sir,
Your most obedient servant,
JOHN M'DIVITT.

Kegworth, Leicestershire,
August 22, 1832.

[By a subsequent communication,

dated August 31, but for which we cannot at present make room, we learn that John Glover, and another patient similarly treated, recovered.—Ed. Gaz.]

MEDICAL GAZETTE.

Saturday, September 15, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—Cicero.

MEDICAL REFORM—SOCIETY OF APOTHECARIES.

The order of our course carries us to the gentlemen of Blackfriars. We have taken a rapid but a fair view of what we should deem the discommendable qualities—the sins, indeed, of omission, rather than of commission, which belong to the two older medical corporations—and we have stated freely and candidly the points in which reformation in them ought to be begun. We have shewn that the deficiencies of those two bodies were owing, perhaps, not so much to their want of power as their want of energy in wielding it. Their wheels would seem to us to have become clogged from pure neglect, until the machinery cannot be put in proper working order without the application of some very skilful repairs. That the engines are fully adequate to the work they have to execute, we never entertained a doubt, and he who would suggest the taking them to pieces in order to set up some showy whirligig of his own in their stead, we should be strongly inclined to call by some other name than that of a mere projector.

And this brings us to the rival establishment—for a rival, and a very formidable one, too, we hold it to be—to which their own supineness has given birth. The Society of Apothecaries, with the enterprise and zeal usually attendant on new beginners, has already effected so much as to throw its elder competitors a little into the shade; but this ought to act as a stimulus, and cause them to rouse from their long repose. This is surely not a time for plodding on in the dull cold track of former days: the original impulse must be propagated, or the rate of going will be soon retarded. Something of this sort we feel when we contemplate the proceedings of the medical corporations: two of them, like heavy men at arms, efficient enough in their particular quarter—but who could be made far more generally useful by doffing some of their honourable but needless accoutrements—while the third is skirmishing through the field with all the activity of a light and unencumbered combatant. But — a truce to metaphor—whence comes this rivalship all of a sudden? How is it that the ancient establishments have got a junior competitor entering the field, and at least dividing with them the favour of the public? We must go back a little, and trace the causes of the circumstance.

In all the works of our ancient founders, whether of constitutions or of colleges, however complete in other respects their foundations may have been, one great and besetting fault is visible—that proper allowances were never made for the changes which "time and the hour" would inevitably beget in them. So has it been with the establishments in question: their founders left little or no scope for possibly needful alterations; in consequence of which, and perhaps but too much owing to the sluggishness induced by long continuance in the same inveterate habits, the wants of the occasion were lost sight of, and all the world saw with displeasure, nay felt, the inconveniences which such a system brought with it.

The circumstances of the country had become greatly altered from what they were in the olden days of bluff King Harry and good Queen Bess: the population, with all its physical wants and

infirmities, had increased enormously, without any thing like a proportionate supply of medical aid: the *officinæ medicorum* could not answer the demand —or rather, they would not, being perhaps too dignified to diffuse themselves through the mass of the people: and what might naturally be expected was the result. The poorer classes were without any proper medical attendance—they were left in a state of nature—to get well as they got sick, unless they could afford to add to their misfortune by paying some village curemonger for his or her dangerous assistance. At the best, they were fortunate if they could have the aid of the parish functionary, who had them farmed *at sixpence a head,* and to whom they should have to travel, for miles perhaps, before they could have an audience of his doctorship. The state of the lower orders throughout the country, a few years ago—and, if we mistake not, that state is still far from being thoroughly amended—was such as scarcely to be believed. "It may be as well," says the Country Clergyman (Mr. Hornby, we believe, the rector of Winwick) in his excellent letter to the Bishop of London, some eight or ten years since, "it may be as well if we look to the situation of a poor labouring or manufacturing man during the sickness of himself or his family according to the present state of things. He is taken ill at his labour with the symptoms of incipient fever; his nerveless limbs refuse the excitement to work, which, nevertheless, he continues for several days. Overpowered at length, he applies to the overseer, who gives him a note to the parish doctor; this he takes to the doctor's residence at the next town, five, six, seven, or possibly eight miles off. Here, if he is fortunate enough to meet with the doctor himself, he has some medicines given him, and is told to go home and go to bed, and come back the next day. By the time the man arrives at his cottage, however, he is in no condition to obey the latter order, if it should have been given, but instinctively complies with the former, whether given or not. He lies in bed day after day, till the doctor's assistant calls. The result of the visit is, that the patient is desired to send to the doctor's for more medicine: for this his wife must leave her husband's bedside, if she has no child old enough to go for her, or can get no neighbour to undertake the journey. If the man's constitution be strong enough to carry him through his illness, nature does her work, and in spite of every thing, he recovers; but if his constitution be unable to struggle with the disease, he grows worse. Somebody tells the clergyman, who finds the poor man in danger, and speaks to the doctor or the overseer, and then more attention is paid. The doctor comes himself, but too late; and the man dies, or at best is brought with difficulty, through a long course of debility, to a tardy and imperfect recovery." Such a state of things as this could not fail to attract the attention of the public—and especially of the more benevolent members of the community—and even of those in high places. But it failed to arouse the effective sympathies of those in the proper quarter. Neither of the great medical corporations bestirred themselves in the least to remedy the want we speak of. A class of practitioners was required to administer to the shamefully-neglected mass of the population, whose infirmities from over-work and diminished comforts, were increasing visibly to an alarming extent. Such a class, we maintain, could well have been supplied by the existing corporations, if they would but have condescended to forego a little of their dignity, and have thrown open their halls for the licensing of an order of practitioners such as the necessity of the occasion demanded. Such things, we know, have been done in war-time;

the strictness of the stated qualifications has relaxed under the pressure of palpable calamity; but an evil which was only spreading gradually, though with equal severity, and though perfectly appalling to those who turned to it their best attention, could not excite any adequate degree of compassionate regard in those who had the power to remove it. It was no time for ceremony—the want was generally perceived; how to relieve it was the question. In this conjuncture the Society of Apothecaries stepped forward, and offered their services. The moment was favourable—the minds of the influential members of the community were prepared to adopt any feasible measure for supplying so needful a requisite, and the act now in force was the result.

The year 1815, we fancy, will be a memorable year for more things than the defeat of the French. In the annals of the medical profession it will mark the origin of an order of practitioners in England, suited in a great degree to the wants of the rapidly extending population, and who, by the popular form of their admission to the privileges of their order, are destined to have a boundless influence among the more numerous members of society. The elder corporations have, in short, in their perfect nonchalance, permitted a most formidable rival to spring up, and they must now, however unwillingly, look to the consequences. They see before them a class whose proper business it was, to compound and dispense *their* prescriptions, but who can now themselves prescribe with equal right. They see an establishment suddenly reared, and taking from them their most valuable—though most neglected—prerogative, of providing for the education of a fitting race of practitioners. They find themselves thus stript of the best feather in their bonnet, thrown back upon their dignities, honours, immunities, and antiquities, to "breakfast" on them "with what appetite they may. But come we to the Society. We have noticed the date and circumstances of their origin: let us see in what manner and with what success they have acquitted themselves.

There were general practitioners, as all the world knows, before 1815: but who that dispassionately considers the race gone by, and compares them with their successors in point of numbers, attainments, respectability, and rank, will not confess that things are now completely altered? To the Society of Apothecaries, then, be given the merit of having effected this most desirable change. It may be argued, perhaps, that the improvements in this branch of the profession have been owing to the individuals themselves,—to their increased zeal and their better education. But whence this better education and this increased and increasing zeal? Whence but from the higher standard of education that has been adopted. Men of inferior acquirements were no longer eligible; and those who intended their sons for this vocation were made to see, that, to be admitted at all, they must have opportunities of acquiring a very extensive knowledge of the art they were to practise, and of the collateral branches of learning. In the acquisition of knowledge, the first steps are generally attended with labour, but no sooner are the difficulties overcome, than the pleasures begin to be tasted, and it then begins to be cultivated for its own sake. Thus the opening having once been made by the improved education, which has been, as it were, forced upon them, the stream of knowledge has flowed in upon the minds of the new generation of general practitioners, and swept from our view—we trust for ever—the many mortifying instances of ignorance which placed a bar to the respectability of their predecessors, and cast a blot on the reputation of the whole profession. Of the benefits that have hence

accrued to the public we have already spoken; but we may add, that the advantage to the present race of practitioners has been incalculable, and we repeat, that they owe it mainly to the Regulations of the Apothecaries, by which the pupil has been *compelled* to do what of himself he might never have done—toil for a distant reward.

We have gone somewhat further than we intended to do in alluding to the *education* appointed by the Society of Apothecaries: in pursuance of our plan laid down some time since, of taking a general view of the legitimate objects of Medical Reform, we stated, as a special item, the educational system patronized by the several existing medical establishments in this country. To that topic we shall presently come, when, of course, we will not overlook the extensive Regulations adopted by the Society's Court of Examiners. But it was almost impossible to allude to the proceedings, or, indeed, to the very existence of the Society, without some notice of their educational arrangements—the feature by which they are most favourably known to the public at the present day. With reference to the question of reform, we can principally have to do with them only as a licensing body, regulating the education of those whom they think proper to admit,—but there are many who cannot forget that they are a *trading company* at the same time. On the first head, after having expressed so much satisfaction generally for what they have accomplished, we have only to suggest the principle of *moderation* to them, by which we trust their zealous labours will always be governed. *Ne quid nimis* was not the least wise maxim that came down to us from former days. They will do well to meditate upon it, as no doubt they do, when their zeal prompts them to adopt new regulations for the supposed improvement of the system of education. There is a certain limit—but we shall not dogmatise on what their prudence, judging from what they have done, cannot fail to dictate to them: we have not, indeed, much apprehension of their suffering themselves to be carried away, like the burgomaster with the cork leg, by the machinery which they have themselves constructed.

But with regard to the second peculiarity in the condition of the society—their being a trading company—privileged by act of parliament to deal in drugs as well as in diplomas—we wish it were not left in our power to speak to that point: it is, however, a peculiarity, the discussion of the propriety of which belongs perhaps rather to the general question of reform, than to that subject considered in a medico-political point of view. This, with much regret we say it, must ever be looked upon as a depreciatory characteristic of the general practitioner's education: be the standard of his attainments raised to the highest pitch commensurate with the powers of the human mind, still the court which decides upon his sufficiency is composed of the members of a corporation fundamentally chartered for the sale of medicines. No doubt the said members may be as competent individually, and as upright as any individuals can be desired to be; but with this we have nothing to do; we merely allude to the well-known circumstances under which the Court of Examiners is established. Nor do we deem it necessary to detain the reader with an allusion to another distinctive character in their educational system—their licentiates being obliged to have served *apprenticeships* to persons in the trade. Our sentiments with regard to these degrading bond-servitudes have often been expressed, and are not likely speedily to be altered. We recollect, however, that the Society is not to blame for the perpetuation of these: apprenticeships were omitted in the construction of the ori-

ginal Bill; and when, in its passage through the Lords, they were introduced, the Society strongly remonstrated against such introduction, shewing, in fact, that they felt exactly as we do with regard to this needless species of degradation.

CHOLERA IN LONDON.

During the last eight days the cases of cholera have been steadily, and even rapidly, on the decrease. At the present date (September 14) the number of deaths each day averages about one-fourth of what it did when the disease was at its height. We may state, now the evil has so far subsided, that, about a month ago, a hundred *per diem* would not have been an exaggerated calculation of the deaths from cholera in London.

PROFESSIONAL APPOINTMENTS.

The King has been pleased to appoint Mr. Keate "Surgeon to his Majesty's person." We hear there is some hitch with regard to the office of Surgeon to Chelsea Hospital — some arrangement with a view to economy, we believe, is contemplated by Lord John Russell, as Paymaster General.

APPARITION IN THE FRENCH ACADEMY.

The following incident, described by one of our French contemporaries in terms amusingly grandiloquent, has lately disturbed the philosophic researches of the learned Members of the Academy of Medicine, in Paris. Before the revolution, the bust of Louis XVIII. decorated the apartment, and the grave and immoveable countenance of the monarch seemed to act as a check on those around, when either the love of science or the *odium medicum* gave rise to any unbecoming vivacity. When the Academicians reassembled after the momentous "days of July," behold! the bust was gone, having vanished in the tempest, as Romulus did of old. No remark was made, however, and they soon learned to look upon the empty pedestal with indifference; at all events, the times being ticklish, they adopted the advice of the maxim, "dans le doute, abstien-toi." What, however, was their astonishment a few days ago, to find themselves once more face to face with the departed Bourbon! Had he risen and stood among them *in propria persona*, the sensation could not have been greater. No translation can do justice to the terms in which the event is described: it was "*inouï — imprevu — incalculable!*"

No one could or would explain how the bust had come back; but it was resolved by a great majority, that, being there, there it should remain.

REGULATIONS OF THE APOTHECARIES.

The Court of Examiners have published a new edition of their Regulations (dated August, 1832): but we are not aware of its containing any thing essentially different from the last, except that it requires the course of Botany, which is to consist of not less than thirty lectures, to be attended in the summer season (between the 1st of April and the 31st of October).

ANATOMICAL LICENSES.

We were wrong in supposing that pupils attending anatomical schools would be required to take out licenses; and we therefore hasten to correct the error, which we were led into by some ambiguity in the wording of the act. The license taken out by the teacher extends to his pupils, so long as they dissect within the premises specified. What farther elucidation the subject requires, it will receive from the subjoined correspondence.

To the Editor of the London Medical Gazette.

London, September 8, 1832.

Sir,

According to my promise, I beg to inform you that the "office-fee of 2*l.* 2*s.* 6*d.* is still demanded by the chief clerk of the Home Department from all applicants for the anatomical license under the new act. I have twice ap-

plied in person at Whitehall for my license, which, I had been officially informed, was ready for delivery, and have, on both occasions, been informed by the gentleman acting as deputy for the chief clerk, that the fee in question could not be dispensed with. I have, for the present, declined receiving the license on such conditions, and I now inclose for your use a letter addressed to Lord Melbourne on this subject, in which I have stated my reasons for objecting to the claim advanced by his official agents.

I beg of you, sir, and of other members of the profession, to observe, that in our communications with government on this subject, we should not allow ourselves to be considered as persons suing for a license to exercise a profitable trade, or exclusive privilege, but as men claiming a legal protection for studies which they follow in the general interests of humanity. The very principle of the fee is, in my opinion, degrading to the profession. I may, perhaps, be allowed to take this opportunity of correcting an error into which, as it seems to me, you have fallen with respect to the obligations imposed upon "students of anatomy" under the new act. The license, I think, you will find, on a careful examination of the act, and especially of clause 12, is required rather for the *place* in which dissection is carried on than for the person there dissecting, so that one license held by the teacher will protect all students engaged in anatomy on his premises.

A separate license is not necessary for every student dissecting in a school of anatomy; but it is required, under heavy penalties of fine and imprisonment, from every physician, surgeon, or student of anatomy, who may wish to improve himself by private dissections at his own place of residence. By such persons, if they did not elude the act altogether by omitting to apply for their license, the fee now demanded at the Home Office would be considered a serious grievance.—I remain, sir,

Your obedient servant,

A Physician.

Letter to Lord Melbourne.

September 8, 1832.

My Lord, — As a physician and teacher of anatomy, I venture respectfully to address a few observations to your Lordship respecting certain fees demanded by the chief clerk of the Home Office, from those whose claims to practise anatomy under the regulations of the new statute have been investigated and approved by your Lordship.

Having declined payment of these fees, in the belief that they are not in any way authorized by the statute, under plea of which they are required; and that the exaction of them from all applicants would be extremely prejudicial to the interests of anatomical science, as consulted by the legislature, I am, for the present, deprived of the license to practise anatomy, of which your Lordship considered me in other respects deserving. I am therefore induced, in the hope that my license may no longer be withheld, to submit, with all possible deference, to your Lordship's consideration, that the "act for regulating the schools of anatomy," recently passed into a law, was framed for the advancement of useful knowledge, and in the general interests of society, as influenced by the study of anatomy, that these objects are set forth in its preamble, and that it does not contemplate the private loss or gain of any individual or parties whatever. I beg of your Lordship to observe, that in complying with its several regulations, great trouble is imposed on the applicants for a license to carry on anatomy in any given place, to which, as necessary for the protection of society, and for the prevention of scandal to anatomy, all such applicants will, no doubt, cheerfully submit; but I must beg leave to express to your Lordship my belief that the "office-fee," now farther required from these parties, will be generally considered as a partial and oppressive fine on the medical profession, and that it can very ill be spared by many who would otherwise be anxious to prosecute their studies under the protection of the "act." Your Lordship will be pleased to remark, that by clause 12 of the new act, a separate license is required from the anatomist for every place in which his dissections may be carried on, and that such license must necessarily be renewed by the same individual on every change of residence. Thus, to the student of anatomy whose residence is for the most part unsettled, and who, in his private

studies, is subjected by the law to the same restrictions and penalties with his teacher, the "office-fee" of 2*l.* 2*s.* 6*d.* if exacted on every license, would become exceedingly oppressive. I further venture to submit to your Lordship that no analogy can be assumed to exist between the protection extended to science by the license to practise anatomy, and the licenses granted to individuals for other purposes by government; and that, as the statute is now first coming into operation, there can be no precedent for the fee in question. Should it be required as a compensation for trouble incurred in preparing the license, I most respectfully suggest to your Lordship that such expense, as it is incurred in the public interest, should be defrayed at the public charge, and that it should not be allowed to devolve exclusively on those engaged in the study of medicine and surgery. I may further be allowed to remark, that the sum required is excessive in reference either to the trouble incurred or to the ordinary means of the anatomical student. With many apologies for having trespassed so long on your Lordship's time, I venture again to express a confident hope that your Lordship will not consent to make the anatomical license conditional on the payment of a sum of money which, though demanded as a "fee of office," must in its effects be considered as a tax upon useful knowledge—a tax of which the principle would be directly opposed to the whole tenor of the act, and altogether inconsistent with the liberal spirit of the government under whose protection the safe and honourable cultivation of anatomy has been at length secured.

I have the honour to remain,
My Lord,
Your Lordship's faithful and
obedient servant,
* * * *

Answer to the above.

Whitehall, Sept. 11, 1832.

Sir,—I am directed by Lord Melbourne to acknowledge the receipt of your letter of the 8th instant, and to acquaint you in reply, that the fee demanded on each license to practise anatomy, is in accordance with the invariable usage of this office; and the amount is so trifling, that his Lordship does not see any sufficient grounds for dispensing with it.

I am at the same time to acquaint you, that he has given directions (with a view of reducing the expense to the parties as far as the official regulations will admit of), that any number of persons requiring a license to practise anatomy at the same place, shall be included in the same license; and that, in those cases where it may be necessary to renew any license on account of a change of residence, no fee whatever will be charged for such renewed license.—I am, sir,

Your most obedient humble servant,
G. Lamb.

CIRCULARS, REQUESTING INFORMATION REGARDING CHOLERA.

We beg to direct the attention of our readers, in those places especially where cholera has prevailed, to the subjoined circulars recently issued by the Central Board of Health. If the requests which they contain be faithfully complied with, much valuable information must be elicited. This method of ascertaining the remedies which have proved, or may prove, of most avail, is infinitely preferable to the idea thrown out by a contemporary, of a batch of travelling doctors to experiment upon patients labouring under cholera. Even as it is, there is odium enough excited towards our profession in the minds of the ignorant, and of a surety we envy not the members of this proposed itinerant board; they would be torn in pieces by the mob in the first place they went to.

No. 1.

Council Office, Whitehall,
September 3, 1832.

Sir,

The Central Board of Health being anxious to obtain, from authentic practical sources, short outlines of the different plans of treatment in cholera which may have been considered most successful, I am directed to request that you will have the kindness to submit the enclosed (No. 2.) to the medical members of your Board, and to any other medical gentlemen in the neighbourhood who may have had extensive practice in the disease.

You will also request the medical gentlemen in charge of Cholera Hospitals to fill up, and forward through you, a Return of the following Form, for each of these establish-

ments within the district under the superintendence of your Board.—I am, sir,

Your most obedient servant,

———

To the Secretary of the Board of Health of ———.

No. 2.

Council Office, Sept. 3, 1832.

Medical gentlemen who have had experience in the treatment of cholera, and are of opinion that they have been successful in their practice, are requested to forward to the "Secretary of the Central Board of Health," under cover to "The Clerk of the Council in waiting, Whitehall," a short account of their respective methods of treatment of the epidemic:—

1st. When in the form of bilious diarrhœa.
2d. In that of rice-water evacuations.
3d. In the stage of collapse.

Return of Patients admitted into the Cholera Hospital at ———, *from* ———, *to* ——— 1832.

TOTAL NUMBERS.			
Admitted.	Died.	Recovered.	Remarks.

(Signed)

CHOLERA *NOT* AT ETON.

To the Editor of the London Medical Gazette.

Eton, September 10, 1832.

Mr. Editor,

A letter from Mr. Moss, dated Eton, August 28, 1832, on the subject of saline treatment of cholera, which appeared in your Gazette of September 1st, has created much surprise in the minds of many of the inhabitants of this town; it having been generally believed, upon the faith of respectable practitioners in Eton and Windsor, that the disease had not yet actually visited Eton. The Parochial Committee, acting under the sanction of the Board of Health established for the Hundred of Stoke, by the recommendation of the Lord Lieutenant of this county, have thought it their duty to institute an inquiry into the facts stated by Mr. Moss, and they have ascertained from the Board of Health that no report has been received by the members thereof, of the cases mentioned by Mr. Moss; and have been informed by Mr. Moss himself, that no other medical gentleman saw any of the cases enumerated by him in his letter, with one exception—namely, the case which is stated by Mr. Moss to have been the first which occurred to him, in Eton Poor-house. Upon that case, the opinions of Mr. Moss and the other medical gentlemen who saw it were at variance. Mr. Moss, however, as the Committee are most credibly informed, appeared ultimately to acquiesce in the opinion expressed by the other medical gentlemen, and the belief that cholera had reached the town was at that period completely dismissed from the minds of the inhabitants. Of the remaining twenty-eight cases mentioned by Mr. Moss, no other medical gentleman has had any opportunity of judging.—I have the honour to be,

Mr. Editor,

Your humble servant,

Richard Okes,

Acting Chairman of the Eton Parochial Committee.

FINAL REPLY TO DR. PHILIP; WITH A RECAPITULATION, &c.

By Marshall Hall, M.D. F.R.S. &c.

Dr. Philip's note consists, to use a military phrase, only of two or three "diversions" from the real points before us. But although I have certainly been much *amused* by them, I have not, nor do I think the most inexperienced of your readers has, been *diverted* by them.

In fact, it consists of mere subterfuges. Shall I enumerate them? 1. I mention the experiment of *removing* the brain and spinal marrow, without arresting the action of the heart, as an experiment which Dr. Philip quotes as old, in one publication, and represents as new in another. What is Dr. Philip's reply, or *diversion*, here? "That the *removal* of the brain and spinal marrow is an experiment of a totally different nature from the *destruction* of these organs!" Does Dr. Philip really think your readers so weak as to be so easily turned from the subject immediately before them? 2. I quote two passages from two several works of Dr. Philip, which contradict each other diametrically, on the subject of the effect of *mechanical irritation* of the brain on the action of the heart and voluntary muscles. What ingenious subterfuge do we find here? Again the thread-bare, worn-out difference between "the *removal* and *destruction* of the brain and spinal marrow!" It is ineffably droll to see how a reference to this profound experiment, which Dr. Philip thinks neither I nor any one can possibly understand, is a reply to every thing. 3. Lastly, what reply does Dr. Philip make to Dr. Thomson and Dr. Hastings?—to the witnesses of my own experiment? None! Is it then honestly confessed that Dr. Philip, and not I, "stands alone?" No!—These things I have stated

as briefly as I could—not in "anger," as Dr. Philip insinuates, but in *pity*.

The cause of my anger, if I *was* angry, is removed. But if Dr. Philip really thinks that I am angry, and if he really thinks I do not understand the difference between "removing" and "crushing," let him wait and read a little work which I am preparing for the press, to be entitled, "An Analysis and Comparison of the Works of Legallois, Philip, Bell, Flourens, Serres, &c. on the Nervous System." He will there see that I can do him strict justice where he is right, as I now do where he is wrong. This is not the usual proceeding of an angry man. I think too, that Dr. Philip's reputation will, after all, suffer much less in my hands than in his own; besides the difference between a sacrifice made at the shrine of truth, and in mere shuffling.

In fact, my notice and reply have received no answer. I have stated, and I repeat, that there are six out of eight propositions, in Dr. Philip's paper recently published in the Philosophical Transactions, which are erroneous. I have offered to prove my assertion, by an appeal to experiment before competent witnesses, the only way of determining such questions. Does Dr. Philip accept my offer? If not, I do not purpose to occupy your pages further by discussions, which, however interesting and important the subject, must be fruitless at last.

In parting, perhaps I might beg as a favour, that Dr. Philip would give me the names of one or two of the twenty Fellows of the Royal Society, who are *still* of his opinion.

And now, Mr. Editor, being no shuffler, I beg distinctly to recapitulate the points in discussion between Dr. Philip and myself, so that if any one wishes to see the argument, he may. I contend—

1. That Dr. Philip's experiment, opposed to Sir David Barry's views, is totally insufficient.

2. That Dr. Philip's statement, that the impulse of the heart is not seen in the capillaries and in the veins, is erroneous.

3. That Dr. Philip's statement, that the velocity of the blood in the different capillaries of a part is different, is erroneous.

4. That Dr. Philip's statement, that the circulation continues in the capillaries long after the influence of the heart is withdrawn, is erroneous.

5. That Dr. Philip's experiment of applying stimuli to the capillary vessels is totally a mistake, since he could only apply them to membranes inclosing them.

6. That Dr. Philip's experiment of a ligature applied to the vein in the neck, is equally entirely a mistake.

7. That my "grave charge" against Dr. Philip, that he describes the experiment of *removing* the brain and spinal marrow, without arresting the beat of the heart, as old in an old work, and as new in a new one, is not "unfounded;" nor is it removed by Dr. Philip's *diverting* reply, that I do not distinguish—do not understand the distinction—between "*removing*" and "*crushing*."

8. That the extraordinary contradiction which I have pointed out in the statements made in the same old work, and the same new work (as quoted literally in my last), is not explained by the same sort of subterfuge.

9. That my offer to repeat my experiments, is a little more to the purpose than evasion; and

10, and lastly, That my allegation that Dr. Philip's last paper is more replete with error than any I have seen in the records of any society, remains in its full force.

14, Manchester Square,
Sept. 11, 1832.

WEEKLY ACCOUNT OF BURIALS,

From the BILLS OF MORTALITY, *Sept.* 11, 1832.

Abscess	6	Hernia	1
Age and Debility	88	Hooping-Cough	10
Apoplexy	16	Inflammation	89
Asthma	20	Inflammation of the Bowels & Stomach	29
Cancer	1	Brain	5
Childbirth	7	Lungs and Pleura	2
Cholera	257	Insanity	6
Consumption	116	Jaundice	4
Constipation of the Bowels	1	Liver, Diseases of the	8
Convulsions	61	Locked Jaw	1
Croup	2	Measles	19
Dentition or Teething	11	Miscarriage	1
Diarrhœa	1	Mortification	4
Dropsy	25	Paralysis	5
Dropsy on the Brain	25	Scrofula	1
Dysentery	1	Small-Pox	15
Epilepsy	1	Sore Throat and Quinsey	2
Erysipelas	8	Spasms	4
Fever	22	Stone and Gravel	1
Fever, Scarlet	25	Thrush	2
Fever, Typhus	13	Unknown causes	2
Gout	1		
Hæmorrhage	2		
Heart, Diseases of	5	Stillborn	13

Increase of Burials, as compared with the preceding Week . . . 430

METEOROLOGICAL JOURNAL.

September 1832.	THERMOMETER.		BAROMETER.	
Thursday . 6	from 41	to 65	29·94	to 29·80
Friday. . . 7	40	61	29·76	29·69
Saturday . 8	41	60	29·60	29·69
Sunday . . 9	48	65	29·84	29·91
Monday . . 10	50	62	29·75	29·84
Tuesday . 11	40	64	30·00	30·11
Wednesday 12	37	63	30·13	30·17

Wind variable, S.W. prevailing.

The 7th, 9th, 10th, and 12th, cloudy; rain at times on the 9th and 10th, and in the evenings of the 6th and 7th; in the evening of the 6th, a few peals of thunder, accompanied by vivid lightning.

Rain fallen, ·225 of an inch.

CHARLES HENRY ADAMS.

ERRATUM.

As the *Literary Gazette* has noticed an erratum in our little paragraph (p. 680) about the illness of Scarpa, it may be right to mention that "recovered" was printed by mistake for *suffered*: the context might have shown this.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 22, 1832.

ABSTRACT

OF

A SECOND LECTURE ON INFLAMMATION;

Delivered during the Course of the Institutions of Medicine in Glasgow College, 1831.

By David Badham, M.B. Oxon.
And now Radcliffe travelling Fellow from that University.

On the Theory of Gangrene.

To attach some definite idea to the term inflammation, and, by separating it from affections with which it is often confounded, to assign to it a place, as well as a name, in our systems of nosology, is a task of which the difficulty has been constantly felt by the best writers on medicine. Inflammation is at present loosely understood to designate an "increased and altered mode of action in the extreme branches of blood-vessels," (Lawrence); but to wave at present any discussion respecting the truth of the first of these propositions—concerning which various pathologists have maintained different and even opposite opinions—it is impossible not to see that the diversified products of inflammation (as they are reputed to be) could never be brought about by any one altered mode of action—such action requiring to be as various and dissimilar as the assigned products themselves. These formularies, therefore, supply no definition of inflammation. But further, any disease may be attended with a changed action of blood-vessels; indeed almost every disease implies some abnormal vascular action in the seat of such disease, and hence the adoption of a definition applicable to almost all diseases cannot be said to fulfil the purpose it proposes. The very admission into medical language of such qualifying terms as suppurative inflammation, gangrenous inflammation, or ulcerative, may further shew the confusion of our ideas; they are evidently unphilosophical, and exhibit a want of simplicity and truth in our conceptions. To state that inflammation in one instance ends in gangrene, because the "travail inflammatoire" is carried to such a height that the part is incapable of sustaining it (Lawrence), or to conceive that, in another, suppuration results from a less degree of activity in the same agent, is to make, in the first case, an assertion contradicted by experience, and, in the second, to affirm that which is repugnant to reason; since it represents the only difference between suppuration and gangrene as being not of kind, but of degree;—whereas, if there was the least ground for such supposition, we might, by artificially exciting and controlling inflammation within the requisite degree, secure the manifestation of either of these states. Now this we know it is not in our power to do; nor does it depend on us to divert that state of parts which nature is preparing, to produce the one into the substitution of the other. But, moreover, inflammation (says Mr. Lawrence) by no means concludes when suppuration, ulceration, or gangrene, have taken place; for which reason he very justly objects to consider these as "terminations" of inflammation, while, to me, it seems equally objectionable to call them, with the distinguished author, "parts of inflammation." The frequent concurrence of inflammatory action with any stage of progress towards such terminations, ought not surely to conduct us to so considerable an error in language when we sit down to inquire into their pathology.

It was the object of the lecture which I had last the honour of delivering in this place, to inquire whether suppuration be, as commonly stated, a pure consequence or effect of inflammation, and not rather an action *sui generis* to which inflammation is not indispensible. The suppurating process is a secreting process; and therefore it may be further urged not inflammatory in its ori-

gin. We know nothing, it is true, of the actions which produce any secretion; we cannot even presume whether they are the same in the instance of each secreted product, nor, at present, can we even hope to arrive at the laws of that vital chemistry which must lend its aid to the mechanism of structure in giving origin to all new products. "We do not know how it is brought about (to use the words of a celebrated writer) that the capillary vessels of one part deposit the substance of muscle; those of another the substance of bone, and so on;" but though the present state of our knowledge afford no positive conclusions on these heads, yet we are agreed in never supposing an action of the nature of inflammation in any such case; nay, we see inflammation commence by destroying the secerning action—by arresting the further secretion of its fluid in a gland or membrane thus affected—and therefore derive the strong argument from analogy, that suppuration is not an inflammatory process, nor pus an inflammatory product.

We now proceed to the consideration of gangrene; and here, dividing what I have to say into two heads, under the first I must object to consider gangrene as a product of inflammation, and in the second I shall venture to offer my notion of its pathology. 1. Gangrene I hold not to be inflammatory in its origin, and though the onus of proving that it is rests with those who say so, I shall endeavour to meet the commonplace arguments in favour of what I think a false doctrine—leading, as most false doctrines do, to corresponding mistakes in practice.

From the loose way in which many authors discourse on gangrene, it would appear to be merely the death of a part, or (to use the language of a French writer) "extinction of the life of a part, with re-action of the vis conservatrix of the surrounding ones," (Hebréard). Mr. Lawrence, with his usual clearness and perspicuity, speaks of gangrene as consisting in the death of a part, together with a peculiar change of structure in it—the result of a previous and peculiar vital action; "and so different from that death of a part which is involved in the death of the whole system, that some kinds of mortification are to be considered as complete preservatives against putrefaction:" hence also inferring the inutility of antiseptics in the treatment of gangrene, because the change which occurs in mortification is "not to be considered as identified with putrefaction." After reading these sentences, by the way, I cannot but wonder that the same eminent authority should elsewhere relapse into the loose notions of less informed and less accurate observers, and be found to say, that "when the cessation of vitality has taken place, the part then undergoes spontaneous or chemical change;" as if this change had not been stated by himself, and truly stated, to be of a very different nature.

Gangrene, unlike suppuration, is reputed to have various efficients—such as intense cold; the infliction of any serious local injury; the direct interruption of a supply of blood to a part by pressure or ligature; diseases of the heart; and, lastly, inflammation. Marjolin has assigned no fewer than ten distinct causes of gangrene, most of which may, however, be considered as referable to one of the above heads; and yet, correctly speaking, I conceive that no one of them is the direct efficient of gangrene, which, in my opinion, is to be sought in some common condition of all parts previous to their assuming that action which terminates in mortification. That inflammation, for example, is not an efficient, but only a remote cause of gangrene, I think quite clear; though the opposite opinion be the orthodox one. In fact, if it should, on a superficial view of the subject, appear that "in violent inflammation the disorder in the circulation is carried sometimes to a pitch which the part is incapable of sustaining, so that the blood stagnates and the part perishes," (Lawrence,) at other times the inflammation preceding gangrene is, we know, so very inconsiderable (as, for example, where it succeeds to the application of a blister, or of scarifications made to let out the fluid of anasarca), that nothing would be less philosophical than to attribute gangrene, in this case, to the "travail inflammatoire," to which it bears no proportion; while it seems equally repugnant to experience to explain it, with Mr. Lawrence, by "a comparison of the action with the degree of power in the part." This statement, indeed, which involves the notion that gangrene consists essentially in "excess of action over power," would preclude the necessity of "a peculiar vital action;" which the able writer whom I have so frequently alluded to had maintained in his very definition of the process.

In the next place, mere debility can never produce gangrene, whether such debility be determined to consist in the excess of action over power, or in an equal diminution of both. Consider, for instance, the condition of hybernating animals, and the examples afforded by parts of the body congealed by excessive cold. Here we have instances of a great depression in the energies of vital and organic life; yet, in the first example, gangrene never occurs at all, and, in the second, only when the parts are imprudently resuscitated;—neither does gangrene necessarily occur in constitutions weakened by scurvy, by valvular disease of the heart, or, in short, by any asthenic ailments; nor, lastly, does it always, or even frequently, occur in those cases of intense

inflammation where the balance between power and action is wholly destroyed; thus it seems a fair inference that parts do not sphacelate in consequence of either excess or deficiency of their natural action, or by a destruction of the equilibrium which in health is maintained between action and power. They sphacelate in consequence of a new and morbid action being called into play, for which pathology has at present no name, but which might be called mortifactive, since mortification implies a passive state of parts, and the dead part must have been killed. In short, previous to "the death of a part, and re-action of the 'vis conservatrix' in the surrounding ones," some positive action of a destructive tendency must, but an inflammatory process need not, be supposed. Take in, too, the consideration that parts largely endowed with the nisus formations, or inherent power of repair, should have a corresponding fitness for inflammatory action, and yet that the muscular solid is little disposed to gangrene; while ligament, tendon, and bone, parts by no means equally capable of reproduction, or liable to inflammation, fall readily enough into gangrene; and reflect that it is in the extremities, where the action of parts, instead of being exalted, as in inflammation, is more frequently depressed, that gangrene most frequently supervenes, and I think we can hardly maintain this supposed relation of cause and consequence between the two processes of inflammation and gangrene. If, then, neither inflammation nor (by a process of reasoning nearly similar) those other conditions of body which precede gangrene, are in fact the agents which produce it, it remains for us to inquire, secondly, what that efficient may be. In order, if possible, to ascertain this, let us consider what is the state, and what are the appearances, presented by the part just previous to its mortifying. Now, as inflammation often comes before gangrene, we may first give a moment's consideration to the state of inflamed parts. In local inflammations, we are told that "an increased quantity of blood is sent to the part;" as, indeed, the redness and the general congestion which we find throughout its tissues abundantly attest, besides which, Hunter proved the fact by actual experiment. But we are also taught by Mr. Lawrence, that "more blood actually circulates through the part than in a state of health;" which is, I think, not so satisfactorily to be made out. In proof of this fact, Mr. Lawrence calls our attention, first, to the state of fulness in the vessels. "If there were merely a larger quantity sent to the part, and it remained stagnant, you would not have the vessels distended, as you will find takes place in inflammation of the hand and forearm." Now it is quite certain that distention is not caused by motion, but by the quantity of material moved, so that the turgescence of blood-vessels is not the consequence of an accelerated, any more than it is of a retarded, circulation; it can have no other cause than a redundancy of blood, which would produce the phenomenon of distention equally in motion or at rest. But, secondly, Mr. Lawrence would, it appears, infer a greater circulation of blood through an inflamed part, from the circumstance of more blood flowing out and from a larger number of vessels yielding it, during the performance of a surgical operation on an inflamed than on an uninflamed portion of the body. "In a case where the hand and forearm were inflamed, I had an opening made (says this gentleman) in each arm; and I found that, within the same space of time, dividing the veins at the same instant, about three times more blood flowed from the vessels of the inflamed limb than from those of the sound side." But might not the more copious blooding which followed the division of the vessels of the inflamed arm be owing to the increased development of the size of some vessels and of the formation of new ones? When there is an "appel des fluides," the part must necessarily become more vascular, in order to accommodate the increased supply of blood which it receives. More blood would also, I conceive, continue to flow from the veins of an inflamed than of an uninflamed part, because large vessels, and these increased in number, would withdraw more blood to fill their canals out of the circulating mass, as a series of pipes will derive from the main trunk which fills them, more or less, in proportion to their number and to the sum of their respective capacities. I do, however, believe that more blood is actually circulated through an inflamed part than in the same part in health, and would cite in proof of this opinion, 1st, the florid red colour of the skin; 2dly, its increased heat; and, 3dly, augmented sensibility;—one and all of these being consequences of the larger presence of arterial, not of venous blood. But arterial blood becomes venalized by confinement; as may be proved by compressing the finger tightly—when the part beyond the ligature is seen to become blue, to lose its sensibility and temperature, and to afford dark blood on being cut. We may, therefore, safely conclude, that in inflammation which is characterized by opposite phenona, that the arterial blood so copiously sent into the part affected is returned with equal readiness, and to the same amount, by the veins; in other words, that the circulation of the part is augmented, and this augmentation would seem, within certain limitations, to be in proportion to the intensity of the inflammation. But if "inflammation be carried beyond a certain point," the part may then come to mortify; because its cir-

culation being now prevented, the vitality which is dependent on that circulation must also cease, and it perishes in consequence." Now two questions seem to occur here, which have not received that attention from pathologists which they deserve: in the first place it may be asked, How does intense inflammation lead to the suspension of the circulation?—and in the second, by what "immediate circumstance is the act of mortification called into existence?" In reply to the first, it may be sufficient to observe for the present, that although inflammation seems to be a stimulus to the minuter vessels, whereby they more readily forward their contents, yet, if the exciting cause be violent, and the "appel des fluides" so great as to overcharge and distend rather than to stimulate, then the coats of the vessels, being overstrained, will come either not to act at all, or to act so feebly as not sensibly to promote the flow of blood through them: this fluid, therefore, already accumulated, must shortly stagnate in a part so circumstanced; discolouration, loss of sensibility, and decrease of temperature, accordingly follow, and the tendency towards mortification has commenced. Thus we see how an organ under inflammation (that is, an organ of which the action is primarily exalted) may come to have no action at all, and how the congestion which occurs previously to gangrene must of necessity take place—for such congestion, or stagnation of the blood, I hold to be as indispensible to the production of all gangrene, whatever be the remote cause, as it is the obvious condition of a part tightly compressed—in which case the blood may go to the part, but cannot be returned from it. Gangrene may also occur from chemical causes; as from the blood being impoverished, or deteriorated, so as to act no longer as a stimulus to the blood-vessels. Ossification of some of the arteries is a mechanical cause of gangrene on the same principle. Here one of the circulating forces is destroyed, so that the blood can no longer be propelled with the requisite momentum through these tubes into the capillaries, as it does when the arterial structure is unimpaired;—in short, whatever has a tendency to produce stagnation of the blood, has a tendency to produce gangrene.

Let us now inquire what may probably be the immediate cause of that action which terminates in gangrene? A portion of the blood sent from the common source of animal temperature and of life, that is, from the lungs destined to carry vitality and heat over the body—is, under some of the foregoing circumstances, arrested in its return; it speedily imparts and parts with the vivifying principle to the texture in which it has become, as it were, included: it soon ceases to be arterial, and becomes wholly unfit for the support of life; by degrees the animal temperature, which is the measure of vitality, is found greatly to have abated; fresh arterial blood is kept from coming to the resuscitation of the part by the conditions alluded to, while the noxious influence of venous blood operates upon it after the manner of an applied poison. The vehicle of life has become that of destruction, and it is probably owing to the directly poisonous influence of *effete*, rather than to the mere want of fresh supplies of healthy blood, that mortification ensues. The part, then, does not die; it is killed. The process of mortification is a "morbus verenatus" set up in the organ by the noxious properties of undecarbonized blood; and by this principally, if not entirely, the parts are stimulated to the last action of which their expiring energies are capable.

Let me observe, in conclusion, that the circumstance of the circulation being nearly suspended in hybernating animals, and quite suspended in parts that have been frost-bitten (in the first of which cases mortification never happens, and in the second not of necessity), does not, in reality, make against my views concerning this deleterious influence of venous blood on parts where it is lodged, and its agency in the production of gangrene. In the first case (hybernation), the stagnation is never complete; the retardation of the circulation is uniform over the body, so that no part can be said to be congested; and, finally, the diminution of the actions of vital and organic life are also in strict relation with the diminished powers of the bodily organs. Hybernation is not disease. The second case (that of frost-bitten parts), where congestion of vessels may be supposed, these parts do not mortify, perhaps, exactly because they are frozen: their congealing is probably a provision against their sloughing—which it may prevent by rendering them incapable of action; and the nature of their tendency to mortification is well illustrated in the facility with which they are observed to fall into this state on being thawed.

PHENOMENA OF DOUBLE VISION.

To the Editor of the London Medical Gazette.

Hackney, August 28, 1832.

Sir,

I take leave to offer a few remarks on a debateable point which has been set aside in pursuit of another object in your discussions with Dr. Graves; the Doctor having merely denied its probability, and yourself resolving to main-

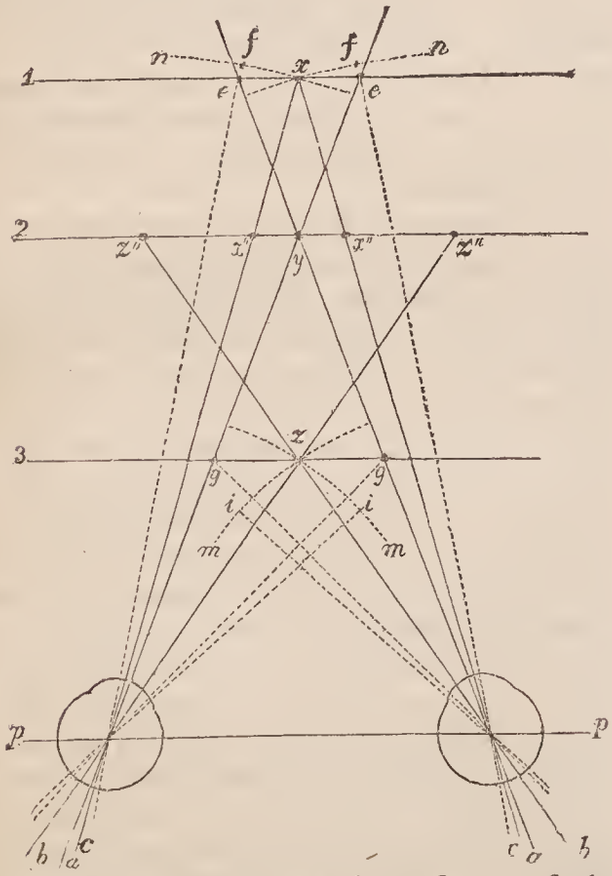
tain it till a better hypothesis be substituted. I allude to a theory which you advanced at first, and which I have, in the following paper, endeavoured to disprove by reducing it to test, and demonstrating some difficulties connected with it; and have offered another theory grounded partly on my former letter, which, I think, will appear more plausible. I beg the favour of your examination and publication of both, when your pages may not be better engaged, and remain, sir,

Your very obedient servant,

GROVE BERRY.

The hypothesis advanced is in the first column of p. 551, beginning with the words, "the eye, as it appears to us, in viewing any number of objects at once, *projects* their images into one and the same plane, or sees them as if the pictures of those objects were represented to it in a determinate surface," &c. &c.; and is repeated in a condensed form at p. 615, and there advocated as the result of "further reflection." To enable one fully to appreciate the theory advanced, I have endeavoured to represent it in the following diagram, which, I think, gives a correct view of it, and then endeavoured to examine the effects of such a position of things.

The *entire* lines are all that are now referred to; the interrupted or *dotted* lines will be used afterwards.



Let $p\ p$ represent the plane of the eyes drawn through their centres; $x\ y\ z$ the objects at different distances from that plane; $1\ x$, $2\ y$, $3\ z$, the planes of ditto, parallel to $p\ p$.

$a\ a$ the lines of the axes of vision, meeting on the middle object y, and forming the angle $a\ y\ a$.

$b\ b$ the lines of the incident ray, or those by which the image of the near object, z, travels to the eye, and which, if the object be seen in the direction of these lines, must intersect with the object; and which, therefore, since light flows in right lines, must be that line by which z attains its place at $z''\ z''$, the plane of the primary object y, on which the axes of vision meet, having attained these places "by projection."

$c\ c$, the lines of the incident rays, or those by which the images of the more distant object, x, reach the eye; but which images it is unnecessary that the eye should "project," as in their passage they have to cross the plane of y, the primary object; they are projected by the object itself, and the eyes are supposed to arrest or "intercept" them at the points $x''\ x''$.

Now, first, if the images of z be seen by the eyes at $z''\ z''$, they must be seen *through* the object itself; and, consequently, if the object be opaque, it cannot be seen at all: or in other words, the *image* must be "projected" *through* the object.

But, secondly, allowing that they were seen at those points, they are carried so far from the eye to attain such a position, that the object they represent should appear, although in the same plane, to be at a still greater distance from the eye which receives the impress than the object y itself appears to be, and which we know to be further off, and does not fail to appear to be so; and this is the consequence of the greater declination of the line $b\ z\ z''$, which makes it longer than the line $a\ y$, this latter being nearer to the perpendicular of the planes. But especially if the point of decussation of the lines of visible direction be still anterior to the object, while its images yet attain the primary plane, their declination from the perpendicular must increase, and the apparent distance of the object must also increase in like manner.

Thirdly, by actual experiment, if that described at p. 635 be repeated, the image of z will never appear to be so far removed from the line of axis of the

eye as z'', which this diagram would represent it to be.

Again: by parity of reasoning, x, the most distant object of all the three, (allowing that the eye *can* arrest or "intercept" its image, certainly the only way in which it can attain that plane,) x should actually appear to be the nearest; because x'' x'' are its images, supposed to be brought to the plane of y; and the line $c\ x''$ from the centre of either eye, is shorter than $b\ z\ z''$, or even $a\ y$, measured from the same point, because it approaches most nearly to the perpendicular of the planes, or its declination is less.

It is, therefore, an inseparable consequence, from this theory, that the relative distances of objects viewed under the given circumstances, should be reversed, the nearest appearing farther off, and *vice versa*; and yet this is the only way in which the images of all objects, whatever their distances be, *can* attain one and the same plane—that of the "primary" object; because it is allowed that the images of the near object do cross, and it has been shewn that the images of the more distant object do not and cannot cross.

There may be other reasons that I have not hit upon, but these appear to me sufficient to invalidate the theory.

The following is that which I offer as a substitute; and the dotted lines of this diagram are those I use for explanation. I have purposely used straight lines, wishing to give an entire diagram, and avoid the confusion which must have arisen from the use of refracted lines in so small a compass.

I imagine each image to be in the plane of the object which it represents; and this I humbly conceive more rational than a removal from that plane would be—at least to the extent required, as I have shewn, by the former theory. The old diagram, at p. 636, will shew in what way I conceive this *may* occur, and I shall aim to shew *why* it may do so. But that diagram also shews that the image is not at precisely the same distance from the eye as is the object: this is a necessary consequence of the different degrees of declination of the line of visible direction and the line of the incident ray. The difference, however, as I shall aim also to shew, can be but barely appreciable at even a few feet distance, and not at all so at one hundred yards.

First, with regard to the nearer of two objects, as z, the further object, y, being the "primary" one.

It is shewn in the diagram at p. 636, as well as in this, by the dotted lines from g to the focus, or rather central point, in what way and where the images cross so as still to be in the plane of the object; the point of their decussation being, *not in the* object, as the above hypothesis requires, and as Dr. Graves's diagram serves unquestionably to shew, though his arguments disprove it; but this point would be considerably *anterior* to the object and nearer to the eyes; which must necessarily be the case when the images, while yet in the plane of the object, are carried away from it along that plane.

I conceive the image of the object z to be seen at g, which is situated at the point of intersection of the line of visible direction with the line of the plane, and also (as it happens, and will always happen when the object is equidistant from either eye) also with the line of axis of the other eye; and I think this to be the point because it will always measure (according to a rule which I mentioned in my former letter, at p. 636) the same distance from the object as the object itself does from the axis of the eye which apprehends the image. There is one other reason I shall give when I have mentioned an experiment on which it is grounded. Now, if this *be* the place of the image, it manifestly represents z to be farther off than it really is; and I think the following experiment will shew such a representation to be correct in point of *fact*, though it is difficult to say whether it is so in point of *degree*.

If a person hold a book, for instance, in one hand at his ordinary reading distance, fix his eyes on a particular spot on the page; now hold a finger of the other hand within an inch, or half an inch of his nose, on a level with the eyes, and equidistant from either; if he now shut one eye, keeping the other fixed on the book, the thumb will actually appear to move away to a much greater distance than we know it really is; so far will it move, that it will seem to be opposite to the other eye, or on the line of its axis of vision. But while the thumb moves laterally along its plane, it does not appear to leave its plane, and come nearer to the other eye in point of anterior distance, as may be proved by adjusting one of the straight printed lines of the book to its movement; but

yet which it ought to do if its *true* distance were represented, and which I will now endeavour to show.

If the image which we receive of *z*, give the eye a correct idea of the distance of *z*, the image should be seen, not opposite to the other eye, but somewhere in the arc *m*, *i*, *z*; and as the image appears (as shewn in the former letter) to be just as much removed from the object as the object itself is actually distant from the line of axis, the point *i* should be the spot where the eye sees *z*, and the lines of visible direction would thus decussate still nearer to the plane of the eyes than in the former case. But I think the above-mentioned experiment will prove that the image of this near object, at any rate, does not give an accurate idea of the distance of the object. On the other hand, I think it will also shew that the image is seen, as nearly as we can judge of it, at the point *g*. Whether the explanation I have advanced for its being at that spot be correct and satisfactory, I leave others to judge. (If it should be said here, on the part of the former theory, that the line by which an image is projected into the primary plane, be either of these dotted lines—see *b*, *b*, in the explanation of the diagram—then the idea of the object's distance given by an image in such a direction, must be still more inaccurate, because the declination of either of these lines is still greater than that of *b z z*.")

And now with regard to the more distant object, *x*, the "primary" object being *y*, a nearer one.

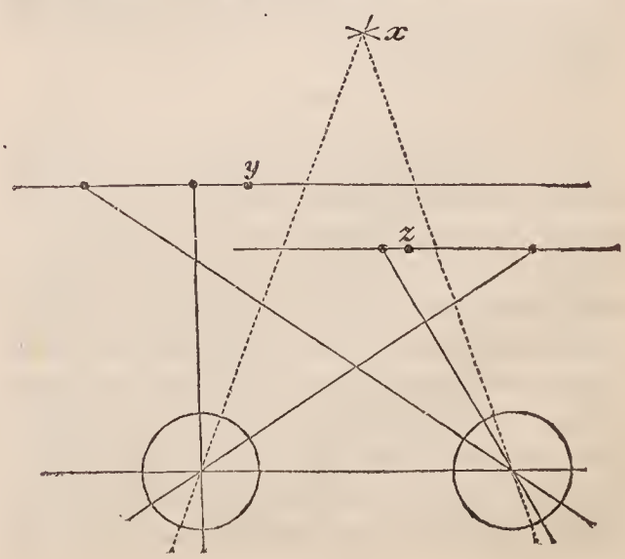
For the same reasons that I have considered *g* to be the place of *z's* image, do I consider *e* to be the place of *x's* image; for though the axes of vision are now differently disposed, because the object is not within the angle which the two axes form, yet the measurements will prove to be the same, because the object is equidistant from either eye. The point *g*, however, represents *z* further off than it really is; but *e* represents *x* to be nearer than it actually is: both are the consequences of the difference in degree of declination from a perpendicular to the planes, that each line assumes.

It has been, I think, proved that we do not gather a correct idea of distance in reference to the *nearer* object; but it is difficult to prove such to be the case in reference to a *distant* object, because the degree of declination of the line of its image must be so extremely small. In the diagram, however, where the distance of *x* from the eyes is but small, the degree of inaccuracy of distance may be shewn.

If in this case, also, we do gather a correct idea of the distance of an object, its image must be situated somewhere in the arc *u x*; and the law, applied before to the other case, now applied to this, shews that point to be *f*. But even at the distance of these few inches the deviation is but trifling, and how much more must it be diminished when the distance is increased! To explain what I mean: suppose the centres of the eyes to be two inches apart, the lines of their axis meeting on a point at six inches perpendicular distance from the middle of that base line, the angle they form would measure about 18° 30′; if the point be carried to nine inches, the angle would measure only about 12° 30′; if to twelve inches, the angle would now diminish to about 9° 30′: how few, then, must be the minutes, or even seconds, to say nothing of degrees, which would be required to measure the angle if the point were at 100 yards distance!

The deviation then, if it *do* occur, must in reference to distant objects be quite inappreciable; and thus, though our idea of the distance of such an object seen under these circumstances be not mathematically correct, yet it would be impossible for the unassisted eye to detect the error, and much more so the amount of that error.

The same rules, with certain modifications, will obtain in reference to objects that are not equidistant from both eyes; and if the experiment be tried, matters will be found to assume such conditions as I have represented below.



x represents the object on which the axes meet.

z, an object at a shorter distance, and within the angle; its images are represented on the plane of the object, as seen by each eye.

y, an object out of the angle, but still nearer the plane of the eyes than x; its images are represented both on its plane.

Though this may not be the philosophical mode of explaining the relations of images to their objects, which ought to be in accordance with various optical laws I have not noticed; yet if this method, or examination, be correct, (and the line of direction always appears perpendicular to the point of the circle or concave retina from which it proceeds) it may be adopted as an exceedingly ready and easy one—the relations of the images, *on* the plane of the objects, to the fixed lines of the axis of vision.

I have chosen to anticipate the objection to the foregoing theory as to distance, because it could not fail to be detected in examining the whole; and I have entered into the subject to shew *how much* it would interfere with its application, though it does not immediately belong to it. A comparison of the two theories will shew how much *less* it interferes with that I have advanced, than it must do with that I have combatted.

Lest I should be thought to have been unnecessarily precise in my explanations, and thus chargeable with prolixity, I would urge that I conceive our utmost precision in the use of the most apt language to convey an idea, is nothing in comparison of the extreme precision with which we adjust our two eyes to an object, in order to acquire distinct and correct vision; the former being a matter of *reason*, the latter of *instinct*.

[It is remarkable that Dr. Graves's theory, Mr. Berry's, and our own, give three several places for the images of objects seen double. This is differing not merely in matter of opinion, but in matter of fact. The readiest and simplest—and perhaps most convincing—way, would therefore seem to be, to try the question by actual admeasurement. So far as we were able to satisfy ourselves on the point, when our attention was directed to it a short time ago, the result favoured, and indeed founded, the theory which we ventured to propose. But we will not be positive: when we can command a little more leisure than we have at present, we shall certainly return to the subject, and bestow our best consideration on the views of both our opponents.—Ed. Gaz.]

BOTANY OF NEW ZEALAND;

BEING A

Description of Trees, Plants, &c. indigenous to that country.

By G. Bennett, Esq. F.L.S. M.R.C.S. &c.

[Concluded.]

Coprosma fetidissima, Forster.
Karamu, or *Patété*, of the natives of New Zealand.

Natural Family, *Rubiaceæ*.

Class, *Pentandria*. Order, *Dygynia*.

The Coprosma fetidissima is a shrub indigenous to New Zealand, and is named Karamu, or Patété, by the natives. I have usually found it growing on the banks of rivers, or in the vicinity of the sea coast; its leaves have a disagreeable smell when rubbed on the hand, which has occasioned its specific name; the flowers are solitary, and of a white colour; the berries are of a bright red colour, and two-seeded. I collected specimens in flower on the banks of the River Wykéré, Bay of Islands, on the 8th of July, 1829. The leaves of this tree are used in one of the ceremonies among the New Zealanders, in which the Rakau Karakia, or praying sticks, are employed, (Rakau signifying a stick, or piece of wood, and Karakia praying). They are used by the chiefs of New Zealand, through the Tohunga, or priest, to discover the will of the gods, or spirits, respecting war, &c. The ceremony is as follows:—

A stick, or piece of wood, is procured for every separate party, and a leaf of the Karamu is tied on each with the Vivi (Scirpus Sp.), or with flax. The Karamu leaf is on one side of the stick, a knot of the Vivi, or the flax which ties on the leaf, on the other; it is considered immaterial which is at first placed uppermost.

The sticks, or pieces of wood, are

then laid in order on the ground, after which the chiefs and people retire to some distance, and the Tohunga, or priest, places himself a short distance from the sticks, and prays for a short time, after which the chiefs are desired to approach. The sticks, on being examined, are found all moved from their original places; some will *disappear*, which is considered a certain sign that the party they had intended to represent will be destroyed. Others will be found turned over; if the knot should be found turned down, the sign is considered bad, because the Karamu leaf, which is supposed to represent the *spirit*, will be uppermost. On the contrary, if the knot should be uppermost, and the Karamu leaf underneath, the spirit is considered defeated; and it is therefore looked upon as a sign that the party represented by those sticks will be prosperous in their undertakings.

The Miro Tree, of the natives of New Zealand.

This is a tree of the natural family Coniferæ, but the specimens collected were neither in flower or fructification. It is named Miro by the natives of New Zealand, and attains the height of from thirty to forty feet, and from six to eight feet in circumference. The wood is hard, and of a reddish colour. It is not used by the natives for any particular purpose. A gum-resin exudes from this tree, which is of a dark-reddish colour, but is not used by the natives as a masticatory, like the other gum-resins, on account of its having a bitter taste. The tree is abundant on elevated land, and grows in good soil.

Cryptogamia.

Cyathea dealbata.
Ponga of the natives of New Zealand.

This beautiful arborescent fern grows abundantly at New Zealand, on the declivities of hills, and is usually found enjoying the shade afforded in the forests by the towering and wide-spreading branches of the more elevated trees; it attains the height of fourteen or sixteen feet, and the length of the fronds are from three to five feet. Above, the fronds are of a dark-green colour; underneath, of a beautiful silvery white, which gives to it when waving to the passing breeze a rich and beautiful appearance. The trunk in circumference is seldom more than a foot to a foot and a half, and is, as well as at the attachment of the fronds, covered with a profusion of chaffy scales.

Externally the trunk of this tree is composed of a black substance, as hard as ebony, which is also continued into the interior, intersecting the white pith-like substance of which the greatest portion of the interior is composed, and which is usually eaten by the natives. When the tree is cut down, an adhesive juice exudes in some quantity.

This fern is the Cyathea dealbata of botanists, deriving its specific name from the elegant white appearance of the under surface of the leaf; and it is named Ponga by the natives, who use the trunks as posts in the erection of their houses; and it is found very durable. The internal part, which is principally the meduliary substance just mentioned, soon decays, but the external will remain for a great length of time undecayed.

I observed at New Zealand two other species of arborescent ferns, growing in marshy situations, but am unable to decide on their generic character, from their being destitute of fructification at the period of my visit (May 1829). These, in beauty of appearance, and magnificence of growth, surpassed even the species just described.

I had heard the natives speak of two other species, when they observed me admire and collect specimens of the C. dealbata; but, in answer to my inquiries, they stated that those were only found in marshy situations, but they were not at that time aware that any grew in the vicinity of our present anchorage (Wyshaki Cove, River Thames, New Zealand). A native, one of my *botanizing friends*, who usually accompanied me in my herborisations (and appeared to take some interest in my pursuits, either from a *natural love of science*, or *of the tobacco* with which he was usually rewarded after our daily excursions), directed me to a place, on the 23d of May, 1829, where I could observe them growing. On descending from a woody hill, by a rivulet encompassed by dense vegetation, I observed these tree ferns. The largest was about twenty feet in height, and about two feet in circumference. The principal

object of attraction was the large size of the spiral stipes, and the enormous extent of the fronds; the trunk, stipes, and central stalks of the fronds, were of a beautiful shining black colour, the length of the fronds being from sixteen to eighteen feet, and the leaflets from two to three feet. This splendid fern was named Korau by the natives. Not far distant from it grew the other species, named Feki by the natives; it attains the size of the Ponga both in the trunk and extent of fronds, but the leaflets are smaller. The stalk and under part of the fronds are distinguished by being of a yellow colour. The two last species grow in marshy and shady situations.

Melicytus ramiflorus, Forster.
Myhoé of the Natives of New Zealand.

Natural Family, *Terebinthaceæ*.

Class, *Pentandria*. Order, *Monogynia*.

This tree is the Melicytus ramiflorus of Forster, the Myhoé of the natives of New Zealand; it grows abundantly in, and is indigenous to, New Zealand. It grows to the elevation of fifteen or twenty feet, of small circumference and branchy; the fruit (the only state in which I have seen the tree) is very small, growing singly from the branches; is a berry, of a purplish colour, and containing several small seeds. This tree is figured in Parkinson's drawings, made during Cook's first voyage, now deposited in the British museum. The leaves are of a light green colour, ovate, and in a slight degree serrated at their edges.

From the wood of this tree, when in a dried state, the natives readily procure fire by friction.

Aralia polygama, Banks and Solander's MSS.
Paté of the natives of New Zealand.

This is an unpublished species of Aralia, the A. polygama of Banks and Solander's MSS. deposited in the British museum, and is accurately figured in flower in Parkinson's drawings, made during Cook's first voyage, deposited in the same museum. It is a tree of slender growth, attaining the elevation of about twelve feet, with a trunk small in circumference, and containing much pith. The leaves are in colour of a dark-green, waved and cpinated at the edges, digitated, but vary in the number of digitations, some having seven, others nine. This tree is named Paté by the natives of New Zealand, and the wood is used by them for the purpose of procuring fire by friction. It is found usually growing on elevated land.

Kaiko-mako tree of the Natives of New Zealand.

From the specimens collected of this tree, being neither flower nor fruit, its generic character cannot be determined. It attains the elevation of twenty-five to thirty feet, and a circumference of two or three feet. The wood, when in a dried state, is used by the natives for the purpose of procuring fire by friction, but is not for any other purpose.

The wood of the trees just described, the Myhoé, Paté, and Kaiko-mako, is only used by the natives for procuring fire. When I asked them why no other kind of dry wood would be equally serviceable, the reply was, "that there was more trouble in procuring fire from the dried wood of other trees than from the three just mentioned."

Elæocarpus dicera, Forster.
Inau of the natives of New Zealand.

Class, *Polyandria*. Order, *Monogynia*.

This tree, the Elæocarpus dicera of Forster, the Inau of the natives, is found abundant on the hills of New Zealand; the leaves are ovate and serrated; the fruit a drupe, small, ovate, and of a dark-brown colour, when mature. This tree attains the elevation of thirty feet, and eight or nine feet in circumference; the timber is heavy and hard, but not very durable.

The bark of this tree is used by the natives for dying a red (and afterwards a black colour, by the aid of mud), similar to the species of Phyllocladus, or Tanakaá, before mentioned. The mode of procuring a black colour by soaking an article previously died in mud, is not peculiar to New Zealand. At the Sandwich Islands the edges of the roofs of some of the principal houses are formed from fern leaves, which are dyed of a

black colour by being steeped for some length of time in the Taro mud—that is, the mud in which the plant named Arum esculentum, or Taro of the natives, grows *. The natives, however, first dye the article red, with the bark of the Tui-tui, or Candle nut-tree (Aleurites triloba), previous to steeping it in the Taro mud. At Tongatabu, a similar process of dying red and black is adopted as at the Sandwich Islands.

Among a numerous collection of plants I made at New Zealand, were also the

Senecio tabidus (Banks and Solander, natural order, *Compositæ*), and found growing abundantly on the beach.

Veronica salicifolia. *Gorokiu* of the natives; growing abundantly, more particularly about the village of Kororadeka, Bay of Islands.

Corchorus sloanoides (Banks and Solander's MSS.); *Iwau* of the natives.

Cineraria dealbata (Banks and Solander's MSS.); *Boka-boka* of the natives. This species of Cineraria is a shrub of slender growth, of the natural order *Compositæ*. The leaves are oblong, of a light green above, and underneath white and tomentose. It is figured in flower in Parkinson's drawings, made during Cook's First Voyage, now deposited in the British Museum.

As the New Zealanders named our books and white paper Boka-boka, I at first thought that the white appearance of this leaf at its under surface might have caused them to call it after our white paper; but I found my conclusion was incorrect, for, on the contrary, they named our white paper Boka-boka from its resembling the under surface of the leaf of this shrub.

Avicennia resinifera. *Máá-noa* of the natives.—This species of Avicennia is a small shrub found growing in marshes to which the salt water has access; the leaves are thick, ovate-lanceolate, downy underneath, opposite, entire, acute, and of a dark-green colour above; whitish underneath. The peduncles are terminal, and loaded with a head of flowers.

Solanum lanciniatum, cut-leaved nightshade. *Koihohio* of the natives.—This species of Solanum is shrubby, attaining the elevation of six or seven feet, and bearing a small fruit of a red colour when mature, which is eaten by the natives. The taste of it, however, is insipid.

A species of the Palm tribe, (probably Sœforthia,) named *Enikau* by the natives.—I never saw this species growing to any elevation; it was observed in the woods, on elevated land. The natives use the leaves for thatching their huts.

Gualtheria antipoda. *Máá-kukáá* of the natives.—A small shrubby plant, found very abundant. "Stem shrubby, diffuse; leaves scattered, roundish, serrate-toothed." —*Forster's Prodromus*.

Passiflora tetrandra (Banks and Solander). *Po-hué-hué* of the natives.—This species of Passiflora is found in the New Zealand woods, and produces small orange-coloured fruit about May, containing numerous seeds of a beautiful crimson colour.

Two species of the Piper genus are found in the woods, a shrub, the *P. excelsum*, and a few small species, the *P. insipidum*.

Dracæna indivisa. *Ti* of the natives. This species of Dracæna differs from that found among the Polynesian Islands; it attains an elevation of ten or twelve feet, the summit terminating in a tuft of broad ensiform leaves, having no petioles, but are terminal, and half-clasping. The leaves form an excellent food as sea stock for cattle, &c. There is another species at New Zealand, the *D. Australis*.

Metrosideros florida. *Kahi-kahika* of the natives.—This species of Metrosideros I have found in the woods of New Zealand bushy, with opposite, ovate-oblong leaves, veined and glabrous, and attaching itself to other trees by offset roots, forming a bush around the tree.

Ceanothus Species. *Kumarahou* of the natives.—I collected numerous specimens of this plant in flower at Paihia, Bay of Islands, in July 1829.

Astelia Sp. *Kaha-kaha* of the natives.

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* If by accident an European, riding between the Taro patches, falls into one by his horse slipping, the mud stains the skin of a dark colour, and it is difficult to wash out. An analysis of the chemical properties of this mud would be interesting.

NOTE ON THE EXTERNAL USE OF THE COD-LIVER OIL, IN THE IMPETIGO SCABIDA, &c.

By Marshall Hall, M.D. F.R.S. &c.

To the Editor of the London Medical Gazette.

Sir,

If you think the following short account of a remedy for some very troublesome cutaneous affections worthy of a niche in the Gazette, it is much at your service.

The cod-liver oil is readily prepared at this very season: it is only necessary to place slices of the organ in a glass funnel; the oil flows, and may be received in a phial. The difference of specific gravity enables the preparer to separate the oil itself from foreign matters.

It is in some troublesome affections of the skin, especially of the hands, conjoining the characters of impetigo, with erysipelatous redness and swelling, and inducing the most severe suffering, that I have chiefly tried this remedy. After all others had been tried fruitlessly, it has speedily succeeded in restoring the cutaneous textures to a healthy condition.

For rhagades, or chaps, it is also a preventive and speedy cure.

I have also reason to know, that, in cases of eczema, and other diseases inducing excoriation and fissures of the skin, it is of great benefit.

My principal object in sending this paragraph to you, is to call the attention of your readers to a remedy hitherto little if at all known. The case which has brought the subject to my mind at this moment is that of a lady, whose hands had been literally crippled for several years, from painful, pustular, fissured, encrusted swelling of the fingers, palms, and back part (impetigo scabida). The case may be conceived from the mere fact of her having applied upwards of two hundred leeches. She experienced great advantage from the first application of the cod-liver oil, and she was speedily cured.

14, Manchester-Square,
Sept. 5, 1832.

P.S —I have requested Mr. Howlett, of 4, Thager-Street, to prepare some of this oil.

OBSERVATIONS on ANIMAL HEAT.

To the Editor of the London Medical Gazette.

Sir,

If you think the following of any worth, the author would be glad to see it inserted in your Gazette.

Your obedient servant,
A Student.

August 30th.

It is not a little remarkable that the important subject of animal heat should have received so little comparative attention, more particularly so, being a subject of personal interest, when considered in relation to that dreadful disease now traversing the empire, as well as an index of disease generally.

The striking analogy between respiration and combustion, in both of which oxygen gas is consumed, heat generated, and the substance under action materially changed in quality, first led the sagacious Dr. Black (to whom chemistry is so deeply indebted) to infer, that the production of warmth in the animal system depends solely upon chemical changes constantly going forward in the lungs, which changes, and the results as before observed, analogy favours as true.

The beautiful and ingenious theory of Crawford is built upon the same assumption, but enters more into detail; its consistency has likewise been admitted. He stated that animal heat primarily depends upon the condensation which takes place in the formation of carbonic acid gas and watery vapour during respiration; but since it is said, the lungs do not sensibly possess a higher temperature than other parts of the body, and we might conclude they would do, being in the immediate sphere of action, Crawford, in order to remedy this apparent defect in his theory, examined the capacities of venous and arterial blood for containing latent heat, and found the capacity of the latter to exceed that of the former in a ratio of 1030 to 892: this point ascertained, his inferences were, that the moment venous blood became arterialized, its capacity for specific caloric was increased; and that the heat rendered sensible by respiration, immediately became latent within it, reassuming its sensibility in

the course of its circulation through the body, in proportion as this property became lessened.

This is the Crawfordian theory, based upon philosophical principles, and strongly aided by collateral evidence, which gained, and to a great extent still possesses, such notoriety and confidence, its extreme simplicity and apparent reasonableness preventing its validity being questioned by abstract reasonings; and it was at last only by a denial of one of its most important facts that an inquiry was awakened. Dr. Davy maintained, upon the authority of his own experiments, that the capacity for latent heat in venous and arterial blood differed so little, if at all, as to give no support to Crawford. If this assertion be true, the hypothesis is necessarily crushed; but the experiment against it, is one of the greatest nicety, extremely liable, in this case, to error: moreover, the supporters of Crawford say, does not the blood possess vitality? and who can say, no.

It is well known that venous blood, when brought into contact with oxygen gas, rapidly changes colour, and carbonic acid, heat, and watery vapour, are given off: moreover, these changes do not occur, in ordinary circumstances, when oxygen is excluded. We may, indeed, safely infer, that this conversion of colour and quality solely depends upon the presence of oxygen, which probably acts either by effecting some chemical change in the saline ingredients, or by relieving free carbon; but the latter supposition is generally admitted.

According to the estimation of Allen and Pepys, during the medium respiration of a healthy man, 26·6 cubic inches of carbonic acid are thrown off in a minute: this volume, when calculated upon, gives nearly 12 ounces of carbon, as the product emitted during 24 hours; a quantity seemingly at variance with our nature; but be it observed, unless a uniform supply of carbonic acid gas is made at every inspiration, there would be a lack of animal heat;—a portion of blood would circulate without being duly charged with latent caloric, according to Crawford's system. It is a leading feature of his theory, that heat rendered *latent* in arterial blood, becomes *gradually* sensible as it takes the venous form in the course of circulation: in agreement with this statement, its change in quality ought to agree in a ratio with the distance from the source of arterialization. Since, however, arterial blood taken from extreme vessels claims an identity with that just emerged from the lungs, we might reasonably infer that it would contain the same specific heat: moreover, with certain morbid signs, this theory will not bear a relative comparision. In inflammation or other affections of the lungs, where, from an increased vascularity of the membrane forming the air cells, respiration is carried on but imperfectly, and a quantity of venous blood is again thrown into the circulation, we might expect that the temperature of the body would sink to a degree directly as the quantity of venous blood thus morbidly circulated; that this, however, is not the case, is well known, but fever heat is the result, as in other inflammations.

These, and many other relative points which could be adduced as unfavourable to the theory, seem in a great degree to prove it erroneous, and at variance with the received doctrines of the profession, but they do not indisputably do so, still leaving a large field for argumental inquiry.

It is this circumstance which brings me to the decisive fact,—a fact, the simpleness of which when once observed has been the means of awakening an inquiry, and pointing out the inconsistency of Crawford's hypothesis of animal heat, when examined in conjunction with the principles of animal nature. That there is a great elevation of temperature caused by the condensation which takes place during the formation of carbonic acid gas, in ordinary cases, is proved by the unerring testimony of our senses; and that the same chemical action operates in the lungs during respiration, appears equally well founded. The practicability of heat, thus rendered sensible, becoming latent in arterial blood, has already been questioned.

The following observations, *if correct*, will place further debate on this point in the shade of uselessness:—I find by a set of simple experiments that the gaseous mass we throw off at each expiration, possesses a higher temperature than the body. From whence this accumulation of heat? Undoubtedly from the chemical changes constantly in operation in the lungs. It is familiar that

air swiftly expelled from our lungs, or blown through small apertures, is colder than the surface of our bodies: this is occasioned by the compression which it is put under by the muscles of the mouth, and the rapid impulse by which its particles fall upon the surface; for it is equally familiar that, when expired without compression, an increase of heat is presently perceived. Bodies having a redundancy of heat, radiate with a rapidity proportioned to the temperature of other bodies by which they are surrounded. When we breathe upon the surface of our body it becomes warmer; this increase of warmth is not totally occasioned by the temperature of the air breathed upon it, but partially from an accumulation of its own heat, which it otherwise would have radiated, had it not been in contact with a body of a higher temperature than itself: hence the sensible warmth in this case is not to be attributed to one cause.

It appears doubtless, however, that the temperature of a part is materially influenced by the quantity of blood it contains, as we almost invariably observe in cases of great determination of blood to the head, or other part, that the heat is excessive there, while the lower extremities are cold; but if in these cases we consider a latent weakness to have existed previously in the part affected, we may refer the cause of this morbid accumulation to nervous reaction: hence the distention, and consequent excessive generation, of heat, are both sequents of one cause, viz. nervous irritation.

It is nutrition in its utmost comprehension, and of course entirely influenced by the nervous power, which supplies the heat of the animal body: the assimilation of matter constantly going forward in all parts, is attended with an increase of temperature, (as I believe has been before observed) and when we consider the large proportion of fluid in animals, and, consequently, the extensive field for chemical changes, there does not lack a source in the nutritive phenomena from which the warmth of the system may be kept up.

If the function of respiration is thus to be divested of one of its most important attributes, to what purpose must we refer its momentous action? It seems, from the intimate consent which exists between the action of the heart and breathing, that some very important change is to be effected in the blood before it circulates the body. Surely more than the mere evolution of carbonic acid, or change of colour, for probably the colouring matter is passive to animal life.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Dublin Journal of Medical and Chemical Science. No. IV.

THIS journal continues to be conducted in a very spirited style. The present number contains a variety of interesting papers, and among them we are glad to observe one by MR. CRAMPTON, *On the Treatment of Injuries of the Head.* We shall just barely notice this paper, and give an extract from it—all we have room to do this week. The author sets out with some excellent remarks on the utility of reporting cases in medical and surgical practice: this he calls the "autobiography of the healing art;" and adds, that the result of the publishing system carried on with this view in London and Paris, must be allowed to have diffused "not only a professional, but almost a personal acquaintance with the distinguished men whose modes of practice, and even whose modes of thinking and of speaking, they so graphically describe." He then alludes to the remarkable differences in surgical practice pursued in various places; and we must give what he says. With regard to the London and Dublin schools, "The chief difference," he observes, "appears to consist in this, that in fracture of the skull with depression of the bone, the trephine is less frequently employed in Dublin than in London. In Dublin we conform in general with the rule of practice as originally laid down by Mr. Dease (who preceded Desault by many years), namely, 'in fractures of the skull with depressed bone, *whether complicated with wound of the scalp or otherwise,* no attempt should be made to raise the depressed bone, *unless very decided symptoms be present of compressed or irritated brain.*' In London, on the contrary, the rule of practice seems to be, 'that if the depressed bone be exposed in consequence

of a wound of the scalp, the trephine is to be applied to elevate the depressed bone, *whether symptoms of injury of the brain exist or not;*' either practice may be supported by the most specious arguments, and each has the sanction of the highest authority. It is plain, however, that in this case the question cannot be decided either by argument or authority, but solely by comparative experiments and observations conducted on a very extensive scale. I doubt, however, whether any individual, no matter how extensive his experience may have been, has instituted a sufficient number of experiments, or observed a sufficient number of facts, to establish definitively either of the opposite doctrines. In surgery, as in every thing else, extreme opinions, or opinions which are not qualified by exceptions and limitations, are seldom safe. I own, therefore, that the opinion of Mr. Dease, qualified as it is by the exception in respect to those cases in which there are symptoms of compressed or irritated brain, has ever exercised a strong influence over my mind, and I may add, over the minds of all those who, like myself, received their earliest surgical impressions from the lessons and practice of Mr. Dease. I am quite aware that early impressions in favour of any particular mode of practice, are extremely apt to influence our opinions in after life, and that '*to seek the testimonies of truth,*' as Cicero beautifully expresses it, '*ex animis consuetudine imbutis,*' is neither philosophical or safe;—but, on the other hand, those who received their first impressions from the lessons and practice of Mr. Pott, are liable to a similar imputation of undue influence, operating in an opposite direction. We can only, therefore, approach to a solution of the problem, by placing in juxta-position the *practice* of those who in different countries cultivate the same field of inquiry on a great scale, although by different means."

Mr. Crampton's paper is not finished in the present number; but it breaks off with a case for which we must endeavour to make room.

Remarkable case of Gun-shot Wound in the Head.

"In the month of September, 1818, Mr. Henry Brougham (nephew of Lord Brougham), a fine, intelligent, and active youth, about eighteen years of age, received the contents of a fowling-piece, loaded with snipe-shot, in the centre of his forehead, by the following extraordinary accident. Holding his gun by the upper part of the barrel, he struck the stock obliquely downwards against the bar of a gate, which he wished to push open. The hammer was resting on the copper cap, which it compressed with the whole force of the spring; this pressure, aided by the vibration communicated by the stroke of the stock against the gate, was sufficient to make the detonating powder explode. From the oblique direction of the barrel upwards, the shot did not strike point blank against the forehead, but somewhat obliquely; it was, however, sufficiently direct to carry away an oblong piece of the integument of the forehead, nearly three inches in diameter—to fracture the os-frontis into small pieces—and to drive the greater number of them deep into the substance of the brain. Mr. B. was carried, in a state of insensibility, by some country people, to a cottage, who took some blood from his arm, but did not meddle with the wound. I saw Mr. B. on the following day; he was perfectly sensible; his pulse about 80, small and regular. * * * The head and face presented a frightful spectacle. The eyes were closed by the great swelling of the eyelids and cheeks. The skin of the face, which was perforated in many places by the shot, was covered with coagulated blood, rendered blacker by being mixed with gunpowder. The principal wound occupied the centre of the forehead, exactly where phrenologists place the organs of eventuality and comparison; here there was a wound of a circular form, above two inches in diameter, and nearly an inch in depth. The bottom of the wound, which rose and fell with every pulsation of the heart, was covered with a mixture of coagulated blood, disorganized brain, fragments of bone, and some of the wadding of the gun. Having cleared the face and head of the coagulated blood, in which they were in a manner incased, I proceeded to clear the wound of the extraneous matters which were lodged in it; but scarcely had I touched with the forceps a large fragment of bone which was buried in the brain, when the whole body was shaken by a convulsive movement, and he moaned deeply. Of course I desisted from all further attempts to extract the splinters, and determined merely to cleanse the wound with tepid water, of the wadding and gunpowder which lay on its surface; but scarcely had three drops of water fallen on the part, when he cried out in a voice more expressive of terror than of pain—"Oh, don't! Oh, what's that?" I asked him if I had hurt him. He said, "I do not know,

but the sensation is dreadful." I now gave up all further attempts at even washing the wound, and merely covered it with a piece of soft lint. The rest of the treatment may be described in a few words. Leeches were applied in large numbers daily to the forehead and temples; iced water to the head, generally; attention was paid to the bowels; iced water and iced lemonade was the only sustenance which was allowed for six days; and at the expiration of this time he was conveyed (eight or nine miles) on a litter, from the miserable hovel in which he lay, to his mother's house in Sandymount. The local treatment was equally simple. The discharge from the wound was gently wiped away from the face as often as was necessary. This discharge contained for several days a large proportion of softened brain, mixed with blood; but the wound itself was not so much as washed for twenty-two days, and even then, nothing more was done than to lift off the small fragments of bone, as they became detached by the process of nature. This great wound was completely healed in two months, although small spiculæ of bone were discharged from time to time, for more than a year afterwards. The cicatrix when healed was deeply depressed, and pulsated like the fontanelle in the head of an infant. Mr. Brougham resumed his studies in Trinity College, and graduated at the end of the year. I met him by accident six or eight years afterwards, and recognized in my old patient one of the most amiable and valuable clergymen in Ireland."

A Lecture delivered over the Remains of Jeremy Bentham, Esq. in the Webb-Street School of Anatomy. By Southwood Smith, M.D. &c.

The present production sets Dr. Smith before the public almost wholly in a new light—as an energetic expounder of the Benthamite or "felicitarian" philosophy. It would be rather beside our sphere, however, to analyse the curious moral and political doctrines with which about two-thirds of the lecture before us is filled; nor, indeed, would there be much novelty in setting forth the learned panegyrist's views of anatomy, with which the greater portion of the remainder is occupied; but we shall just extract one passage, in which Dr. Smith gives the reasons which induced the eminent deceased to bequeath his body for dissection. It ought to be known that the will in which Bentham first formally made this bequest, is dated so far back as 1769, when he left his body to his friend, Dr. Fordyce: only two months before his death, he revised the document, and solemnly confirmed that part of it which contained the act in question. "That the disposal of his body, which has brought it to this place, and which has called us together to express our admiration of the act and our gratitude for it, would give pain to some for whom he entertained a sincere affection, he knew; but he also knew that the amount of pain thus produced would be overbalanced by the good likely to result from such an example. He had a great regard for the science of medicine. How could it be otherwise with one whose thoughts were so constantly employed on the promotion of human happiness and the mitigation of human suffering? He knew that the basis of medicine is anatomy, and that the only means of acquiring a knowledge of anatomy is through dissection. He had an utter contempt of the prejudices which withhold the means of pursuing dissection. He was satisfied that there is but one way of putting those prejudices down—and that is, that those who are above them should prove it by giving their own bodies for dissection. He therefore determined to set the example. He was aware of the difficulties that might obstruct his purpose—he provided against them. He chose three friends to whom he was tenderly attached, and on whose firmness he thought he might rely. He prepared them for opposition and even for obloquy. He asked them whether their affection for him would enable them to brave whatever portion of either, or of both, might fall to their share in carrying his wish into effect? They assured him that neither opposition nor obloquy should deter them from performing what he required to the letter. 'Then,' said he, 'I charge you, by your affection for me, to be faithful to this pledge.' They have been faithful to it."

The personal traits of Bentham's character which this pamphlet contains are vividly drawn; and the popular exposition which it gives of the utilitarian principles, cannot but render it highly acceptable to the public.

TREATMENT OF CHOLERA.

SULPHATE OF COPPER IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

BELIEVING that the following facts may lead to a more successful mode of treating the prevailing epidemic, I request that you will allow them to be made known to the public through the medium of your very useful publication.

On the morning of the 17th August, I visited a young woman, a dress-maker, 20 years of age. She had been under medical treatment all night for a severe attack of cholera. All the symptoms were present, and strongly marked. I had contemplated trying the sulphate of copper, in a few doses, in the first case I met with, having seen all the modes of treatment recommended fail. I ordered a scruple in an ounce of water to be given. The gentleman who was in attendance reported to me that it remained in the stomach a quarter of an hour, a strong proof of the diminished excitability of that organ. When the vomiting ceased, (which was occasioned by the sulphate) the skin became warmer, the pulse more distinct, and the severity of the spasms very much mitigated. The stomach was now quiet. Two grains of calomel, and one tablespoonful of beef-tea, were given every half hour; at night three drops of croton oil; soon after which the bowels began to act, exhibiting some improvement in the secretions. This patient very slowly recovered, but the urgency of the symptoms soon subsided after the vomiting produced by the sulphate of copper.

A. Cook, 30 years of age. Aug. 19.—In this case, likewise, all the symptoms were present, and very severe. The same treatment was pursued as with the former patient, except that she had neither sedative nor stimulant, both of which had been administered to the former in the commencement of the treatment. This patient was able to come down stairs in a week.

Alderidge, 68 years of age, a cooper, Aug. 26.—In consequence of a complete misrepresentation of his case, was ordered calomel, eight grains, with one and a half grains of opium. In an hour and a half after the spasms became so severe that I was sent for. I found it a case of well-marked cholera, *without vomiting*. As his stomach was quiet, I ordered a purgative draught, and desired they would wait the result. I saw him four hours after: the spasms very troublesome, rice-coloured stools, livid hands and face, &c. I gave him the sulphate, which vomited him in two minutes; soon after which the spasms ceased; indeed, he complained but once after. The calomel and croton oil were given as before, and he appeared to be going on well when I saw him last at 10 P.M. On the 27th he got out of bed at 3 o'clock, fancying that he wanted to make water. The attendant with difficulty got him into bed again. He died at half-past 5.

On examination of the body, the lungs were found to have suffered extensively from frequent attacks of inflammation. The abdominal viscera presented the appearances usual in cholera.

King, about 25 years of age, Sept. 1st: was found in a state of complete collapse by my assistant, Mr. Green, a very active and zealous young man. He administered to him the sulphate, which produced an effect like magic: this was followed by calomel and croton oil. At 7 o'clock, the family where he lodged thought him so well that they might get rid of him; they therefore sent him to the infirmary in Mount-Street, Grosvenor-Square. Mr. Green called there to report what he had done. There he was put under their own system, which, after a little time, was given up for one which succeeded in restoring him. This, I understand, was small doses of sulphate of copper and hydrocyanic acid.

It may be asked why I adopted the above treatment. My answer is, I saw that no reliance was to be placed on any plan of treatment hitherto adopted. I had often witnessed the prompt advantages gained by a brisk emetic in severe attacks of remittent fever, where the stomach had been so irritable as to reject every thing. I determined to try the effect of an emetic in this devastating disease. Past experience pointed out to me the sulphate of copper as a powerful emetic, and one, from its astringency, likely to restrain, in a great degree, the effusion, for secretion it cannot be called, into the stomach of the peculiar fluid which forms one of

the characteristics of the disease. There are other advantages, which it is not necessary to enter upon. I hope I have said enough to induce others to try my mode of treating this enemy to the human race. I understand they are using the sulphate of copper now in the Mount Street Infirmary, but not in the same way.—I am, sir, yours,

JAMES MORRAH, M.R.C.S.

Sloane-Street, 10th Sept. 1832.

[The papers which follow are transmitted by the Central Board of Health.]

Large doses of opium before collapse—opinion of various remedies after collapse has come on —fickleness in the effects of remedies.

THE Central Board of Health have circulated a paper (No. 2,) in which they request those medical gentlemen who have had experience in the treatment of cholera, and are of opinion that they have been successful in their practice, to forward to the Secretary of their Board a short account of their respective methods of treatment of the epidemic.

"1st, When in the form of bilious diarrhœa.
"2nd, In that of rice-water evacuations.
"3d, In the stage of collapse."

I am not of the number of those who "are of opinion that they have been successful in their practice;" nevertheless, having been otherwise called upon, I proceed to present a very brief statement in reply.

In the hospital wherein I reside as medical officer, there has hardly been an opportunity of treating the disease in the two first forms, viz. in bilious diarrhœa, and in rice-water evacuations; but from a pretty ample observation of those states out of doors, as well as in my own treatment, as in that of competent medical friends, I have arrived at an opinion, which is somewhat at variance with that of many most able practitioners. This opinion is, that, at any reasonable period before the stage of collapse — even though there be rice-water purging — even though there be some vomiting—and although cramps may have come on—the whole disease may be stopped quickly, *safely*, and, with human certainty, by full and energetic doses of opium,—doses proportioned to the age, idiosyncrasy, and condition of the individual; but having for their object the principle of stopping all the discharges at once. Small doses do not avail; for, it appears to me, that some secretions are more easily stopped than others; that the hepatic, for example, is arrested by a smaller opiate than that from the intestines; consequently, if we give small opiates, the bile is arrested, whilst the intestinal excretion goes on, and the patient thus hastens on to collapse; but if a sufficient dose be given to stop every secretion, there has not, so far as I have witnessed, been any but the happiest result from it.

There is no advantage (the secretions being thus arrested) in being too hasty to act on the bowels by purgatives; they generally act spontaneously in a day or two, and the whole of the secretions are restored, without any active interference on the part of medicine.

If, however, they do not act, and circumstances seem to require it, some mild, warm laxative may be given,—an undue operation from which is to be guarded against by a small opiate.

This treatment does not apply to some other premonitory symptoms that more rarely arise; nor to those cases which, without previous diarrhœa, go rapidly into collapse. Plethoric and robust individuals will not bear so much opium as thin persons.

In the stage of *confirmed* collapse, I have seen no treatment so efficacious as to deserve especial praise; but some remedies have permitted patients to die more rapidly than others.

1st, The "Saline treatment" has increased involuntary purging, and done harm; or the powders have been immediately vomited, and have done no good.

2d, Excessive opiates have quieted the sufferings, but have appeared to hasten the deaths of those subjected to them.

3d, Spirituous stimulants, *in excess*, have oppressed whatever power remained, and have been of no avail: in moderation, they are not injurious.

4th, Those who have recovered from collapse—a few—have done so under frequent doses of calomel, with or without *minute* doses of opium; according to the degree of purging existing, frequent doses of ammonia, sinapisms, and a moderate portion of brandy and water, gin, or portwine, and the like.

This treatment, it will be perceived, is founded on the broad principle, that, in the cure of the malady, nature is the principal, and the physician her auxiliary; and is directly contraposed to that which seems to assume, that nature is the auxiliary, and that a very unimportant one to medicine.

A too meddlesome practice has been always fatal.

5th, Cold water, toast water, barley water, balm tea, are freely allowed to those who desire to drink: its rejection being no objection.

The consecutive fever bears no relation to the quantity of stimulants or opiates administered, some of the most severe cases of this fever having occurred in persons who had not taken a particle of either brandy or opium.

It appears to me that the spasmodic cholera assumes different types in different localities and at different periods: thus, the same remedy that is useful in St. Giles's shall fail in Whitechapel, and that which may have been beneficial in Clerkenwell shall prove injurious in Newgate; nay, the very remedy that was useful in a place six months ago, shall be useless in the same spot to day. In this, I think the analogy of puerperal fever may with benefit be consulted.

I am acquainted with an extensive parish, within the Bills of Mortality, whose surgeon has adopted one, little varied plan of treatment in cholera, from the beginning of this epidemic: for a few weeks nearly all his cases recover; and then, again, without any alteration of system, he finds that nearly all his cases for the next few weeks die: this has occurred more than once, without his being able to refer it to any tangible cause.

Not having the opportunity of communicating any important or useful fact to the Board, I have abstained from occupying their time at greater length, and have accordingly endeavoured to make my remarks as concise as possible.

ALEX. TWEEDIE.

September 12.

EFFICACY OF TARTAR EMETIC, &c.

To the Secretary of the Central Board of Health.

Droitwich Asylum, Sept. 14, 1832.

SIR,

ONLY two have escaped, and these I hope are impregnable, as they have been in the midst of infection hitherto without suffering. During the last five days (viz. on the 9th) we have had only one new case, and that, though a bad one, I am happy to say is convalescent. Those persons in constant attendance have all been attacked; those only occasionally in attendance, at the commencement of the disease, have escaped.

The progress and consequences of the disease, from its commencement (the 22d of August last), are as follow:

	Cases.	Deaths.
Between the 22d and 25th August	6	6
On the 26th (day of my arrival)	2	2
From 27th (3 A.M.) to the 13th September instant	45	13
	53	21

The treatment from 22d to 25th August inclusive, cal. and op. and creta mixture. The two cases on the 26th, being each between 60 and 70, incurable lunatics, were treated with opium, brandy, and tinct. capsici; and by this, one attacked the next day, æt. 70, was saved, and is still well. In many cases it was impossible to give solid medicine, and in three every thing offered was refused. From the 27th August to the 30th inclusive, the treatment was the same, viz. a large dose of calomel, with gr. j. opii, followed by small doses, with capsicum and camphor at short intervals, using, of course, the hot air bath, enemas, turpentine fomentations, sinapisms, &c. when necessary. Dissatisfied with the result of this treatment, particularly where vomiting was severe, I was anxious to attempt some plan which might promise more favourable results. I had frequently observed in the bad cases the utter impossibility of checking the sickness by any remedy usually employed. I had tried over and over again, and as often failed in administering it, the mustard emetic. I determined, therefore, no longer to attempt to check the sickness by the usual means. I accordingly in all cases, from the 31st of August inclusive, in which vomiting was extreme, gave ant. tart. gr. iij., and after the stomach became, as was the case, rather more quiet, I gave every hour a pill containing ant. tart. gr. iij. op. gr. j. In no cases thus treated, from the commencement, did the sickness last beyond four

doses. Very often both vomiting and purging ceased; but where the last continued, I gave rhubarb and magnes. c. tinct. op. ʒss. followed by cal. and capsicum, c. op. ¼ gr. ʒtiis horis, with a mixture of catechu, opium, and carbonate of soda. In one case, which happened on the 9th (the last reported), finding the purging and vomiting did not so readily yield, I gave at one dose ant. tart. gr. vj., opii gr. ij. This person was a woman of the name of Ball, a violent lunatic; it was therefore necessary to give it in coffee. After this the vomiting ceased, but the purging (the true rice-water dejection) continued, the pulse being still imperceptible, or at all events so feeble as to induce a doubt of its presence. I now gave ʒj. of tinct. of op. also in coffee, and repeated ʒss. in an hour afterwards. All the bad symptoms left, and she is now convalescent without other treatment.

The relative effects of the different modes of treatment to which I have alluded, will be seen by the following statement of cases and results:—

	Cases.	Deaths.
From the 22d to the 30th August, inclusive	26	17
From the 31st August to 9th September	21	4

Of these, two were violent lunatics, obstinately refusing medicine.

If you think the above worth publicity, and that it really possesses novelty, perhaps you will mention it to the Board; if, on the contrary, you judge it differently, silence upon the subject will oblige, sir,

Your obedient servant,

J. B. Steward.

UNION OF SALINES, CALOMEL, AND COLD WATER.

The treatment which has been found most successful in the Lancashire Lunatic Asylum, unites the advantages of the saline method of Dr. Stevens, the cold water system of Dr. Shute, and the calomel treatment of our Indian practitioners.

When the patient is first seized, two table spoonsful of common salt are immediately administered, in about a tumbler of warm water. After the operation of the salt, two-grain calomel pills, with the saline powders of Stevens, are given every hour, or every half hour in very bad cases. An enema of sixty drops of tinctura opii, in a pint of gruel or warm water, is immediately thrown into the rectum; cold water is drank *ad libitum;* camphorated mercurial frictions are applied to the regions of the liver and stomach; and warm frictions are used assidnously to the extremities.

Brandy, opium by the mouth, and the mustard emetics, have been found to be productive of much mischief.

When the epigastrium is very tender, we apply leeches, with evident relief; but not otherwise.

Bleeding has been had recourse to, when the pulse could bear it, and when congestion of the brain was present, with the most marked benefit.

Of the cold water practice of Shute we cannot speak too favourably—the patients deriving the most comfortable sensations from its use, and frequently calling for water when they expressed the utmost aversion to all other fluids. In no case have we observed the slightest bad effects to ensue from its exhibition.

We have tried the croton oil practice, as recommended by Dr. Teggart, but hitherto we have not had sufficient experience of its effects, to enable us to speak in any decisive terms of its efficiency.

To the convalescents we administer small and repeated doses of castor oil, and allow mild, digestive, farinaceous diet, with marked advantage.

Almost every species of fumigation, that has promised any advantage, has been had recouse to; but nothing appears to have produced such efficient and rapid effects, as the fumes arising from the explosion of trains of the coarsest gunpowder.

It is worthy of observation, that the cholera, from its commencement on the 5th of September, up to the present period (the 17th), has been confined exclusively to the women, who amounted on the first irruption of the disease to 188, of whom 72 have been seized, without any distinction of age, bodily strength, or infirmities; the epileptic, fatuous, and furious maniac, not being more exposed to its ravages than the healthy, tractable, and cleanly monomaniac. The disease has pervaded every part of the female division of the hospital.

We have every reason to believe that the cholera originated in a female patient, brought from the Manchester

Workhouse, who was attacked on the night of her admission with diarrhœa and partial collapse, had fully developed symptoms of cholera on the third day, and fell a victim to the disease on the 11th day after her admission.

LAWSON WHALLEY, M.D.
Physician to the Asylum.
EDWD. D. DE VITRE, M.D.
WM. DAVIDSON, M.R.C.S.E.D.
Surgeon to the Asylum.

Lancaster County Lunatic Asylum,
Sept. 17, 1832.

P.S.—It is requested that the above communication may be forwarded to the Medical Gazette for insertion.

TARTAR EMETIC IN CHOLERA.

MR. LANGFORD, resident medical officer at one of the Cholera Hospitals in Manchester, thus writes:—

"I am aware the practice, as applied to cholera, is quite novel in this country, but its success has induced me not to delay, for a longer experience, this communication.

"The following is the mode of application:—Dissolve 10 grains of emetic tartar in 7½ ounces of water, with half an ounce of rectified spirits of wine; of which give half an ounce every two hours, until the biliary and urinary secretions are properly restored, and then gradually diminish its frequency. Allow toast and water, or whey, *ad libitum;* prohibit all heat and frictions, and give no other remedy. A *tolerance* of the medicines is in a short time established." And again:

"I continue to entertain the same favourable opinion of the successful administration of emetic tartar for the removal of spasmodic cholera. In the advanced stage of the disease, it will be found necessary in the first instance to increase the dose, to produce the action of vomiting, as well as in younger patients, to diminish it; and if this remedy is not successful, in the most malignant and fatal stage of spasmodic cholera, when the nervous energy is subdued, and the powers of life laid prostrate, it is not wonderful.

"Sept. 5, 1832."

DR. SHUTE ON COLD WATER IN CHOLERA.

DR. HARDWICKE SHUTE has transmitted to the Board of Health a third paper on the above subject, and has politely directed that it should be forwarded to us. It appears that the Doctor, owing to the favourable state of the public health in Gloucester, has had no additional experience on the subject; but, as the paper is farther explanatory of his views, we shall insert it next week.

DR. PIDDUCK ON SALT AS AN EMETIC.

THIS communication, addressed to the Central Board, contains statements and opinions so similar to those contained in the paper lately published by us, that we deem it unnecessary to do more than refer to the one already before our readers.

DR. WARD ON CROTON OIL, &c.

A PAPER also lies before us sent by our respected correspondent Dr. Ward, of Wolverhampton; which, however, we received too late to insert this week. The writer speaks very favourably of Croton oil, especially in young subjects, "of 15 cases of children," says the Doctor, "attacked with cholera in every stage, from the age of 5 months to 12 years, whom I have treated, with the oil and with that alone, I have lost but one, a boy of eight died comatose of secondary fever." The Doctor also uses the salines of Dr. Stevens to the exclusion, if possible, of any other kind of drink.

MR. BUSOINES ON CHOLERA.

THIS gentleman states that he has had fourteen cases in which there were rice-water evacuations and collapse: the first six, being treated with calomel, opium, and stimulants, all died. The other eight had calomel (quantity not mentioned) every hour, a wine-glassful of a saline mixture every half hour, and "as much cold water as they could possibly drink—in some instances a gallon every two hours." Six recovered, and two died, under this plan of treatment. Want of space, however, prevents us from giving minuter details.

MEDICAL GAZETTE.

Saturday, September 22, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

SELF-SUPPORTING DISPENSARIES.

OUR remarks of last week on the condition of the poorer classes in various parts of the country with regard to medical assistance, were scarcely penned, when some papers reached us from Southam, in Warwickshire: they proved to be highly interesting. A society, it appears, has been formed in that town, with the provident design of alleviating the distresses which may possibly attend a visitation of Cholera in the neighbourhood, and this is to be effected by a fund collected for a three-fold purpose, — maintenance in sickness, funeral expenses, and a gratuity to be allowed to bereaved survivors. Their little regulations are so purely benevolent, and so sensibly devised, that it may gratify many of our readers to have an abstract of them:—

1. Each head of a family subscribing any sum from three pence to five shillings weekly, according to his ability, shall be allowed twelve times the amount of such subscription daily, from the time any of his family shall be attacked by Asiatic cholera, up to the time of their convalescence or death, and shall have the benefit of a medical attendant.

2. Of each parent who shall have subscribed any sum from three pence to one shilling weekly, the sum of three pounds shall be allowed to the family, in case of the death of either parent; and for each parent who shall have subscribed any sum above one shilling weekly, the sum of five pounds shall be allowed to the family in case of the death of either parent.

3. For each servant or child of a subscriber's family who shall die of cholera, the sum of two pounds shall in all cases be allowed, without any reference whatever to the amount of subscription.

4. The members shall agree to continue their subscription to the fund till the treasurer be reimbursed the payments he may be called upon to make beyond the sum he has in hand, and till such time as the cholera shall cease for twenty miles round Southam: on which happy occasion, or on any day of thanksgiving to Almighty God as shall be appointed by his Majesty in Council, it is proposed to divide the fund between the widows and children in such manner as a general meeting of the members, called for that especial purpose, shall direct: the widows to have their dividend at the time, and the children to have theirs deposited in the Savings-Bank, and allowed to accumulate till they shall respectively attain the age of fourteen years.

It is understood by every member signing these resolutions, that he is bound in honour not to retire from the Association until the same be dissolved.

These resolutions were unanimously agreed to at a large meeting of the inhabitants held in the course of the last month.

That such an admirable plan has been proposed and adopted at Southam, no person who is acquainted with the intelligence and moral worth of the people of that town will hear with surprise. It was in the same place, as most of our readers may recollect, that some ten years ago the first self-supporting Dispensary that this country could boast of was established; and since then, we find that the example has been followed with the happiest results, in no less than sixteen different quarters, — in Coventry, Birmingham, Walsall, Burton on Trent, Rugby, Derby, Lymington, Willesbourne, &c. The principle upon which these establishments are founded cannot be too universally made known, nor can it, perhaps, be better understood than by repeating the words of the late Dr. Gooch, originally published in this journal, and subsequently copied by Mr. Southey into the first volume of his Colloquies:—"In the year 1823, some opulent and benevolent persons held a meeting at Southam, a small

county town of Warwickshire, near Stratford upon Avon, when Mr. Smith, a surgeon of the town, proposed the establishment of a Dispensary for the sick poor of that neighbourhood. The funds for the support of this Dispensary were to be drawn from three sources,—parochial contributions, the subscriptions of opulent persons, and, lastly, voluntary subscriptions from the poor themselves. There would be two classes of patients; those who would have a claim on the dispensary from the subscriptions of their parish, and those whose claim would depend on their own subscriptions. The latter were to have the encouraging appellation of the 'Independent poor': their annual subscription was to be 3s. 6d. for an adult, and 2s. for a child; and this would give them a right to medical attendance whenever they required it. The medical attendants on this Dispensary were to be all the respectable surgeons of the neighbourhood, who were to take care of the sick poor of Southam and the surrounding county within six miles; and for this, the income of the Dispensary, after paying its expenses, was to be divided among the surgeons, according to the number of miles travelled and visits made. Attendance was to be given at the Dispensary one hour every day (excepting Sunday), when those patients who were able to go received advice; those who were too ill to go out, were to be visited at their own houses. As they were no longer dependent on one surgeon, they were not likely to be neglected, for it was not probable that all would be busy, or out of the way, at the same time. The Southam Dispensary has now (1827) been in operation four years, and the result appears to be highly satisfactory to the members. It has been wholly maintained by parish subscriptions for the pauper patients, and by the voluntary subscriptions of the independent poor, *without any assistance hitherto from opulent persons**. At a vestry meeting at Southam, in March of this year, it was resolved that of two hundred persons among the poor subscribers to the Southam Dispensary, *one half would have been on the parish if it had not been for this institution;* and they strongly recommend the formation of similar dispensaries all over the kingdom."

And so most heartily do we. It is in this, we take it, that the great benefit of such establishments consists—that, while they provide for the physical necessities of the poor subscribers, they provide also for their moral wants—by imparting to them a prospective feeling, and rendering them *self-provident* beings. How powerfully the existence of such a feeling in his poor patients—"hope springing eternal" in their breasts—conduces to the success of medical agency, every practitioner can testify—how it facilitates recovery, or soothes the sufferings of the bed of sickness. And which of our politicians is ignorant that the condition of the people can never be securely provided for, without due attention being paid both to their physical and moral necessities?—that the health of the body politic depends mainly on the body natural? We could run on with a series of suggestions on this topic, were it our cue to meddle with the councils of the politicians; but we shall only venture to allude to one more advantage derivable from the self-supporting dispensary system—and that because it concerns both government and people, and tends to remove a weighty responsibility from the one party, and a heavy burthen from the other;—we mean the economical improvement most certainly to be expected with regard to the poor-rates, from encouraging these provident little institutions. Our poor-laws—at once the glory and the blot of our political system—have, it is to be feared, in but

* We trust that this can no longer be said: we are almost sure it cannot.

too many instances, tended to denationalize our artizans and labourers, who are often not ashamed to assert that they have found in beggary "a better trade than the spade and the workshop." The blame, we are inclined to think, attaches in a great degree to those who, having the distribution of charitable funds at their disposal, are too indolent, too ignorant, or perhaps too reckless, to discriminate the wisest mode of discharging their duty.

Another argument in favour of these dispensaries is, that they tend to place the services of medical men in something like a proper point of view. The patients are taught to consider that, as well in their own case as in that of their attendants, "the labourer is worthy of his hire;" and they find themselves, we fancy, treated in a very different way from that which they experienced under the old system of being farmed out by the parish, at from *thirty to fifty shillings a hundred*, by the year. The medical men, it is true, are by no means adequately remunerated for their services; but the proper principle is kept up, which ought to be observed in every such case, for the mutual benefit of all parties.

The accounts from Coventry, where a self-supporting dispensary has been established within the last year, are very gratifying. During nine months, 1505 patents were admitted, 1189 were cured, 101 relieved, 186 remained under treatment, and but 19 died: there were 10 midwifery cases. The subscriptions amounted to 126*l.*; of which about 46*l.* were expended on medicine, and the remainder (80*l.*) fell to the surgeons! From Birmingham we learn that there has been "not only a great increase in the total number of patients, but a more than proportionate increase in the independent class."

We have read Mr. Smith's "Address" with considerable pleasure, and give him great credit for his kind and humane intentions.

SCIENCE IN EGYPT.

The Pacha of Egypt has made great exertions to introduce the most important improvements in science generally, and in medicine in particular, into his dominions. For this purpose he some years ago induced M. Clot, a French surgeon, to settle in Egypt, and patronized the establishment of a Medical School at Abouzabel, to the proceedings of which we have repeatedly alluded. The Pacha has recently created his French *protégé* a Bey, so that he now figures under the somewhat ludicrous appellation of *Clot-Bey;* and his patron is about to dispatch him with 12 young Egyptians to Paris, where they are to be educated in the most complete manner, and on their return to be appointed Professors of the different branches of knowledge they have acquired. Who knows but it may be destined that science shall again, as in times of old, find a resting place in Egypt.

CHOLERA MEDALS.

The French government has distributed a number of medals to those among the inhabitants of Paris who were most conspicuous in their philanthropic exertions during the late epidemic.

MORTALITY FROM CHOLERA IN PARIS.

From the 26th of March (when cholera broke out in Paris) to August 31st inclusive, the number of deaths *reported* were 17,978. April and July were the most fatal months: the former gives 12,723, and the latter 2577 deaths, the mortality of the other three months taken together being but 2678.

CONTROVERSY REGARDING THE STRUCTURE OF THE PLACENTA.

As the following letters have never before been published, we think they may be interesting to our readers at the present time, when the attention of anatomists has again been directed, by a paper of Dr. Robert Lee, in the "Philosophical Transactions," to the relation subsisting between the mother and fœtus during gestation. The subject is one of peculiar interest and difficulty, and we trust that it will now be investigated

in such a manner as to leave no ground for future controversy; in other words, that the doctrine of the Hunters will be either established or refuted. It appears to us that the most important points to be determined are the following, and we do not think they ever can be satisfactorily ascertained but by examining the parts in their natural situation, where they have not been disturbed by throwing forcibly foreign matters into the uterine vessels.

Do the uterine sinuses terminate in large, smooth, simicircular orifices in the inner membrane of the uterus, as represented by Dr. Lee? Are there openings in the decidua covering the placenta, corresponding with those in the inner membrane of the uterus? And, lastly, is it possible to demonstrate the existence of cells in the placenta without the employment of injections?

The best mode of investigating these subjects seems to be that first recommended by Mr Owen—viz. dissecting them under water; and we would recommend this mode, instead of forcing injections into the uterine vessels, to those who may enjoy the opportunity of examining the gravid uterus in the latter months. We would also recommend them to verify the statement of Dr. Lee, as to air not passing from the uterine sinuses into any part of the placenta; for it is hardly conceivable that air should not pass freely into the cells of the placenta, if the uterine sinuses were continued into these cells, as described by Professor Burns in his last paper.

Original Correspondence between John and William Hunter.

Windmill-Street, Feb. 3, 1780.

Dr. Hunter begs the favour, that the Secretary of the Royal Society will read to the Society what follows:—

Mr. Hunter's account of the structure of the human placenta, explaining the connexion and circulation between the mother and the *fœtus* in *utero*, which was read at the last meeting of the Royal Society, informs us that it was a discovery which he made with Dr. Mackenzie, and that it was not claimed by me.

The Society will be sensible that I am reduced to the necessity of taking notice of this mistake, when they are informed of the following facts:—

First. That the doctrine has been many years ago published in printed books as my discovery, and had been communicated as such by myself. See Baron Haller, for instance, in the second part of the eighth vol. (p. 220) of his great Physiology, in quarto, printed thirteen or fourteen years ago.

Secondly. Besides treating of it as my own discovery, in my lectures upon the subject, I have always done so for many years last past, in the very first lecture of my course, which is the most public of all, because the door is then open to every person whose curiosity prompts him to be present.

In the third place, occasionally both in what I have printed, and in my lectures, I hope I have not overlooked opportunities of doing justice to Mr. Hunter's great merits, and of acknowledging that he had been an excellent assistant to me in this and in many other pursuits. By doing so I always felt an inward gratification, shall I call it, or pride? I had given him all the little anatomical knowledge which I could communicate; and put him into the very best situation that I could for becoming what this Society has for some time known him to be.

May it be presumed, then, that I stand possessed of the discovery in question till proofs shall be brought to dispossess me?

I shall most willingly submit to the pleasure of the Society. If they signify an unwillingness that this emulation (I will call it) should go on, I shall acquiesce, and be silent. If curiosity, justice, or the laws and practice of the Society, should incline the Council to seek out and determine upon the merits, I shall be equally ready to obey their commands.

And if it should appear reasonable to them, I would first beg to know the grounds of Mr. Hunter's claim, as I am too well acquainted with his abilities not to think that he must be able to support his claim by something that I am ignorant of. And if I should receive that satisfaction, I shall immediately shew that I am more tenacious of truth than even of anatomical discoveries. But if that information should not alter my thoughts upon the question, I shall shew to the satisfaction of the Society, if I can at all judge of my own employments and pursuits, that my pretensions arise out of a long series of

observations and experiments made with a view to the discovery in question: that it was not a random conjecture, a lucky thought, or accidental occasion, but a persevering pursuit for twelve or thirteen years at least, the progress of which was always publicly known here, and admits of the most circumstantial proof.

WILLIAM HUNTER.

To the President of the Royal Society.

February 17, 1780.

SIR,

Though I know the constitution of the Society over which you preside too well to suppose that they will give their judgment on any subject, and respect it too much to think it a proper field for waging the war of controversy, I cannot avoid requesting you to lay before that learned body a short answer to the paper given in by my brother, Dr. Hunter, as silence on my part after his charge may be interpreted by my enemies into an acknowledgment that I have intentionally claimed to myself a discovery in reality his due. I am as tenacious as he is of anatomical discovery; and I flatter myself as tenacious also of truth.

The discovery was made in the manner in which I stated it in my paper. Dr. Mackenzie had injected the subject, and being unable, as I conceived, to explain an appearance which he had found in dissecting it, sent for me. I came to him; and having examined it further, explained the appearance in question, then, for the first time, to my own satisfaction and that of Dr. Mackenzie; and the evening of the same day, full of the discovery, I came to Dr. Hunter, and brought him with me to Dr. Mackenzie, to see and judge of the explanation which I had given, and Dr. Mackenzie had agreed to.

This is my state of the fact upon which I ground my belief of myself being the author of this anatomical discovery; but as my brother thinks differently, after a period of twenty-five years, I am content to abolish all remembrance of the successions of time in the course of that day, and to suppose that Dr. Mackenzie, Dr. Hunter, and myself, inspected the parts together, and together made the discovery: by which means the honour of it will be divided into three; one of which I may surely be allowed to take to myself, the other two may appertain to Dr. Mackenzie and Dr. Hunter, if they choose to claim and be content with them; though, in this division, we must make some reserve for the claim of several ingenious young men, at that time pupils, who were with us, and of course entitled to some proportional share in the discovery, though their present situations, settled at a distance from this town, have prevented them from getting early notice of the present claim, and of course from making application to the Society for their share.

However, I may here declare, that if Dr. Hunter will produce to me any claim which I can allow of his having discovered this anatomical fact at any period of time prior to this conference at Dr. Mackenzie's, I shall first declare, in excuse for having troubled the Society, that I was not before acquainted with it, and immediately after declare that he is entitled to the sole honour of it, at least in preference to myself.

I am, sir,
Your much obliged, and most
obedient humble servant,
JOHN HUNTER.

Jermyn-Street, Feb. 17, 1780.

THE SURGICAL PROFESSION IN IRELAND.

COMPARATIVE MERITS OF THE LONDON AND DUBLIN COLLEGES.

To the Editor of the London Medical Gazette.

SIR,

THE observations, on the Irish Grand-Jury Bill, contained in your number of the 18th August, have appeared to me to give rather an *ex-parte* view of the matter in dispute between the members of the London College of Surgeons practising in Ireland, and the licentiates, or members of the Dublin College; or rather, you have omitted to state fully the reason for the adoption of the *exclusive* clause.

Every one knows that the surgical profession in England and Ireland are upon a totally different footing. Till lately it was a fundamental article of the charter of the Irish College, that no member or licentiate of that body should dispense medicines: hence the almost total absence of a grade of the profession in Ireland corresponding to the

English general practitioner. In this country, on the other hand, the apothecary is all in all: possessed of the licence of the Hall, he is fully qualified to practise: he needs not the diploma of the College: nay, he has just as much right and title to practise medicine, surgery, and midwifery, without that diploma as with it. Hence the College, or more properly speaking, the Court of Examiners at the College of Surgeons, do not lean very heavily on the candidates that come before them. It is certain that they do not keep them in for any long time—fifteen minutes generally—sometimes perhaps twenty—nay, they may even go so far as the half hour—and I have lately been surprised to hear that some have been kept in a whole hour! Be this as it may, the fact is notorious that the students in general do not hold the examination at Lincoln's-Inn-Fields in much respect; and they smile if you compare that examination with the ordeal through which the worshipful Company of Apothecaries require them to pass. The truth is, that if the College required a considerable acquaintance with anatomy and physiology—did their examiners give a scrutinizing and practical examination in surgery, and require from the candidate an accurate knowledge of materia medica, and other collateral branches, the members' list would be reduced considerably—there would be a lamentable deficit in the examiners' fees, and the sphere of collegiate influence would be much contracted.

But I find, upon looking over my last paragraph, that I have made a statement not altogether correct: I have there stated that there is no grade of the profession in Ireland corresponding to the English general practitioner. There is, sir, as you are aware, another class, consisting of individuals who have obtained the license of the Irish Apothecaries' Company in the first instance. Now this license, or diploma, has hitherto in no way contributed to the respectability of its possessor. Obtained generally after a trivial and rather *pro formâ* examination, it afforded but little security to the public against ignorant practitioners. But this is not all: as soon as they have obtained the approbation of the apothecaries, they come over to London, and seldom fail to return dubbed M.R.C.S.L. a distinction which they take care to append to their names on all occasions, and which, doubtless, does not a little astonish the natives of some of the less civilized parts of the emerald isle. And these are the individuals who, under the altered (I cannot say improved) state of the law, would be admissible to the County Infirmaries.

And now let us inquire what system of education is laid down for those who, in order to be eligible under the existing law, must become licentiates of the Dublin College. To obtain this license, a student is required to go through an extended course of study, prescribed by the College, in evidence of which he produces certificates of hospital attendance for not less than three years, as well as attendance on lectures on the various branches of medical science. The minimum period of study is five years: those who have not served apprenticeships to members or licentiates of the College are limited to six, the College making a difference in favour of the apprentice, who is supposed to derive considerable advantage from having a master to instruct him. At the end of his probationary period, the student must submit to an examination on two several days, for an hour each day, in anatomy and physiology, in the theory and practice of surgery and medicine, in chemistry, and in the materia medica. This examination is generally, I may say always, held in the day-time; not, however, with closed doors, but in the presence of as many of the members and licentiates as choose to attend, nor by examiners paid for passing the candidate, but by men who work gratuitously, and often at personal inconvenience. The character of these examinations has been in general highly creditable to the examiners; indeed, some have considered them as too severe. Some years ago this might have been said of them with some justice; at present, however, they can scarcely be so considered; on the contrary, they rather appear to be less strict than they ought to be. The licentiates of the Dublin College of Surgeons are generally young men of very good education—many of them graduates in arts of Dublin University, and many graduates in medicine, either of that or Edinburgh University; a great number, too, avail themselves of the advantages of the splendid Lying-in Hospital of Dublin, and obtain, after examination, the diploma

which is conferred by the master and assistant-physician of that institution.

You will observe, sir, that I take it for granted it matters not whether a man belongs to the London or Dublin College when the question is one of abstract merit. But on this very principle, the Londoners manifest a littleness of mind when they boast of their connexion with a College which numbers amongst its members a Cline, an Abernethy, and a Cooper, even supposing we could not meet them, and boast of our Dease, and Colles, and Carmichael. Were the public competent judges of professional merit, it might with safety be left to them to decide between the parties; nor need the Irish licentiates fear the competition; but in the absence of this criterion, I conceive the Irish College not only justified, but imperatively called upon, in the discharge of their public duty, to resist any measure which would remove that local advantage to which the possessor of the Irish diploma is justly entitled by virtue of his superior qualifications.

I fully concur with you that this is not a mere Irish question; nay, I would go farther, and say it is a national one, because it in a great measure may tend to direct the eyes of a reforming legislature, where the most glaring abuses exist. The desire of the Irish College is, and justly, to prevent the inroad into places of trust and emolument of persons whose education they disapprove of, and over whom they can exercise no control. Let the London College, say they, raise their standard of professional knowledge—let there be a full assimilation of the modes of education in the two countries, and then we withdraw our opposition. The examiners of Lincoln's-Inn Fields admit our licentiates to examination without further testimonials, thereby testifying their approbation of our mode of education: this we cannot do with the London members, because we do not approve of their mode of education. Should the examiners of the Dublin College, then, continue to be worthy of public confidence did they sanction the appointment to County Infirmaries of men whom they deem unworthy to be admitted as candidates for their diploma? If, however, reform take place on this side the channel, and the English system of education be raised in its character, then may the Irish College with safety admit that reciprocity between the two bodies which it is the desire of every good man to see established.

But, sir, I have my fears that the standard of surgical merit in Ireland will be gradually lowered: that the College, finding their banner deserted for that of their more easy rival, will be weak enough to compromise; and thus the assimilation will take place so as to bring back the surgical profession in Ireland to a position from which the well-directed efforts of a few enlightened individuals had but a few years ago with difficulty rescued it. If, on the other hand, the opposition of the Irish College receive the support of the legislature, the College here may be induced to require a more minute professional knowledge from the candidates for their diploma, and thus a lasting and permanent good will be effected for both countries, and the malcontent petitioners will have their causes of complaint removed.

I have the honour to be, sir,
Your obedient servant,
An Irishman in London.

London, September 1, 1832.

BIOGRAPHICAL MEMOIR OF THE LATE M. SERULLAS*.

George Simon Serullas was born in 1774, at Poucin, a small village in the department de l'Ain; his father was a notary. Having been bound to an apothecary, the illustrious anatomist, Xavier Bichat, was his fellow apprentice. During the revolution his youth was spent in the military hospitals, where his great intelligence and desire of information showed that he was one destined for the improvement of his age. His talents were recognised by Parmentier and Payen, and at the age of twenty-two years he was appointed "Pharmacien Major;" but during the movement of the armies, Serullas had but little time or means for accomplishing his wishes for instruction; his genius was not as yet unfolded, and notwithstanding his having received medals of encouragement from the Society of Agriculture, and from the Society of Pharmacy, for some memoirs on grape sugar, he was obliged, in the capacity of "Pharmacien en Chef," to waste his ardour in the German campaigns.

* Abridged from the Dublin Medical Journal.

At the destruction of the empire, he obtained an occupation more conformable to his taste. Appointed, in 1814, as Professor to the Military Hospital of Instruction, at Metz, and at length free to devote himself to his favourite studies, he precipitated himself into that career to which his talents called him. He soon felt the necessity of re-arranging his intellectual existence, and at the age of forty-two he commenced the study of Greek and Mathematics.

With Serullas, study was a passion; days and nights were successively consumed without his regarding fatigue, expense, or health. Thus he soon elevated himself, not only to the level of existing knowledge, but soon excelled his masters. He commenced public courses of lectures at the hospital at Metz, and the interest of the novel experiments which he demonstrated, attracted to his lectures the pupils of the School of Application of Military Genius, who had finished their education at the Polytechnic School.

Serullas met with immense difficulties—he overcame them all. In 1820, he recognized the presence of potassium in antimony fused with cream of tartar; he detailed the curious facts connected with this subject, and demonstrated the existence of arsenic in all the antimonial preparations, except tartar-emetic. He obtained from the last, heated in a close vessel, with charcoal, a true pyrophorus, fulminating by the addition of a single drop of water. His beautiful memoirs on the per-iodide and the proto-iodide of carbon, those on the iodide of cyanogen (a most deleterious compound to experiment on), attracted the attention of the Institute. He prosecuted his researches on bromine, by forming hydro-bromic ether and bromide of cyanogen. He bore away from the illustrious Davy the priority of the fact relative to the gyratory movements of alloys of potassium on mercury and on water, by proving that they depended on electro-chemical action.

It is difficult even to enumerate, in our limited space, the names of the combinations formed by Serullas, of iodine, bromine, chlorine, the hydracids, &c. amongst each other, or with carbon or the metals. Thus his perchloride of cyanogen, obtained in beautiful crystals of a brilliant white colour, is one of the most deleterious substances known—its odour alone is immediately fatal. His novel researches on sulphuric ether are not less interesting; he obtained a neutral sulphate of carburetted hydrogen, and a very remarkable crystalline substance formed of bi-carburetted hydrogen. Besides these numerous new bodies, Serullas succeeded in isolating cyanic acid; he examined the chloride and phosphide of sulphur; he obtained, also, the iodates of potash and iodic acid, by very ingenious processes; he shewed that this last acid is capable of being used in a test for morphia and some other vegetable alkalies. The memoirs on the perchloric acid, and the perchlorates, the crystallization of perchloric acid, and the use of this re-agent to separate potassa from soda; finally, the bromide of silicium, and the hydrobromate of phosphuretted hydrogen, are some of the new substances which we owe to his more recent investigations.

At the death of the illustrious Vauquelin, in 1829, the eyes of all were turned on Serullas; he was overflowing with researches and discoveries. Although destitute of any property but his situation, he spared no expense to procure the most exact instruments—the most rare and new substances, which he submitted to examination. Such was his ardour, that every day he rashly hazarded his life in dangerous experiments; more than once he had nearly perished in the vapours of those compounds of chlorine, cyanogen, azote, &c. Nothing stopped him for an instant; the happiness of a discovery atoned for all difficulties and dangers.

He operated on hitherto untried substances, and penetrated into paths of research before unexplored. Thus did he enter, triumphantly, the portals of the Institute. Far from slumbering in the Academic chair, each week saw him bring into light some train of research; the mine was open, and incessantly he extracted from it the most profound riches. No petty pride or jealousy contaminated his love for science. He animated by his praises the zeal of younger chemists, and mixed, without manifesting superiority, in the labours of the Society of Pharmacy.

Harassed by such unintermitting labours, first in the army and afterwards in his laboratory, his robust and energetic constitution had been broken; his bilious and ardent temperament consumed itself in its own fire. How often, neglecting his health, has he not been all but poisoned, or asphyxiated,

in his hazardous experiments! From this negligence resulted a chronic gastritis, which forced him to renounce all stimulating food, and to confine himself to milk and vegetable diet.

From such a state of the digestive organs, we may conceive how predisposed he was to cholera; he was much afraid of it. When that destructive disease appeared in Paris, his temper, hitherto gay and joyful, became sad, and the future appeared to him covered with a sable veil. Thus affected, he went to the obsequies of Cuvier, to the Jardin des Plantes. At this time the chair of chemistry, vacant by the death of Laugier, was destined for him; a most brilliant career was opened for Serullas, and he had the hope of at once increasing his renown by new discoveries, and of being the cause of the comfort of his parents, as he had always been of their happiness.

Who, alas, would have thought, that that very day death should greet him on his return from that sad solemnity! He got wet and cold; the most energetic resources of art were in vain employed against an attack of cholera, which, on the 25th May, bore him away from science and from friendship, after an illness of nine hours.

He is buried near the great naturalist, whose death appeared to have broken the ties that connected him to life.

MIDDLESEX HOSPITAL.

Colica Pictonum produced by Cider.

W. D. a mason, aged 27, was admitted on the 3d of July, under the care of Dr. Francis Hawkins. The account which he gave of his complaints was, that six months previously he had been attacked by pain in the bowels, affecting principally the umbilical region, and attended with sickness and obstinate costiveness. At that time he had had no evacuation by stool for three days. Soon after this attack his hands became paralyzed. At the time of presenting himself at the hospital, he was able to raise the weight of the right hand by extending the wrist, but he could not extend the fingers of the same hand. On the left side he could neither extend the wrist nor the fingers. He was quite positive that he had not been exposed to the fumes of lead or of paint, and that he had made no use whatever of lead in any of his occupations. Upon further examination, however, it appeared that this attack had occurred to him in Gloucestershire, in the neighbourhood of Cheltenham, where he had been for about five months, drinking cider very freely. Once before he had suffered a similar attack of colic in the West Indies. The pain at that time lasted three days, but was not followed by paralysis. At present his bowels have become regular in their action, and his general health good.

He was ordered to take two grains of sulphate of quinine, dissolved in an infusion of mint and cloves, with the addition of a small quantity of sulphuric acid, three times a day. It was further directed that the right hand and arm should be electrified, and a splint applied to the left.

July 4th.—He states that the effect of the electricity has been very considerable, and that at first it almost restored the use of the right hand. He does not, indeed, retain so much power over the muscles, but a permanent sense of warmth remains in that arm which has been electrified.

7th.—A slight degree of purging and griping having been produced by the medicine, he was ordered to take pulv. cinchonæ, ℈j. three times a-day, and to submit both arms to the electric spark.

9th.—He is sure that electricity has been of service to the left arm, as well as to the right.

14th.—The wrist of the left arm can now be extended, as well as that of the right, and he was allowed therefore to lay aside the splint; but he has less power over the fingers of the left hand than over those of the right.

He was ordered to continue the use of the bark and of electricity, which he thinks by no means loses its effect, and to have the wrists and fingers rubbed with the compound camphor liniment.

Remarks.—The foregoing case has some interest attached to it; for this reason, that it illustrates closely, in more than one point, the able and laborious researches of Sir George Baker. To these researches it has been chiefly owing that the effects of lead upon the constitution have been exactly ascertained; that the mineral poison, and the symptoms which it produces, have been so clearly placed in the light and relation of cause and effect; that from the mere observation of the symptoms we can assign their origin with almost unerring certainty. It often happens that the patient suffering from saturnine colic, or palsy, will ignorantly or obstinately deny that he has in any way been exposed to the influence of lead. A case of this kind occurred to a physician of the highest eminence; on which occasion it was accidentally discovered that the patient, who was often engaged in shooting, had been accustomed to keep his charge of shot for some time in his mouth. In the treatment of this species of colic, it is a great advantage that we are at length acquainted with its true and sole origin. Before this had been ascertained, the cider-drinkers of Devonshire, like the subject of the present remarks, had long suffered from colica pic-

tonum, not only with a less certain prospect of relief than might now be offered to them, but without the means of avoiding its cause, of which they were ignorant. It is remarkable that the subject in this case should have suffered previously from the same disorder in the West Indies, his case thus bearing again upon the discoveries of Sir George Baker, who observed that colica pictonum was not only often produced by the use of cider, but also by that of new rum in the West Indies and in America, because during the distillation of this liquor worms, and vessels formed of lead, or of pewter containing lead, were formerly in too frequent use.

The real cause of so many cases of colica pictonum was for a long time pertinaciously denied by those who manufactured rum or cider carelessly, or who fraudulently made use of sugar of lead for the purpose of sweetening subacid liquors. And no small praise is due to Sir George Baker for establishing an important truth by patient industry and perseverance, in opposition to the arts of fraud and interest, and in spite of the incredulity of those who unwillingly admit the truth of a doctrine which is propounded to them for the first time.

The progress of the foregoing case shews that electricity is a stimulant which should not be laid aside in the treatment of saturnine palsy. Strychnine, the remedy which has lately been so highly extolled, was not at once resorted to, because it had not appeared to Dr. Hawkins to effect any marked benefit in the treatment of some other cases of the same disorder in the Middlesex Hospital. One of these cases was that of a servant who had suffered, together with several of her fellow-domestics, in a family in the country, from the use of water out of a cistern containing some lead in solution. The circumstances were similar to those which the late Dr. Warren has related in the Medical Transactions as having occurred in the family of the Duke of Newcastle.

Case of Cynanche Tonsillaris, attended with Hæmorrhage.

J. B. æt. 19, a servant, was admitted on the 3d of July, under the care of Dr. Francis Hawkins. He had been ill four days with sickness of stomach, sore throat, and fever, having, as he believed, caught a severe cold. The right tonsil was observed to be considerably enlarged, and there was a small white speck on the left. Pulse frequent; tongue furred; no redness nor heat of skin. He was ordered to take the Infus. Menthæ Sulphuricum, with ʒss. of Sulphate of Magnesia, three times a-day, and to use a gargle, composed of the Infus. Lini Comp. with Sulphuric Acid and Tincture of Myrrh.

July 4th.—The difficulty of swallowing and breathing is not at all diminished, the tonsils being fully as much swelled as before. Pulse 120; he is rather more feverish and hot.

Hirud. viij. gutteri. Hydrarg. Submur. gr. iv. Pulv. Antimon. gr. vj. Pulv. Ipecac. C. gr. v. f. pil. iij. horâ somni sumend. Contin. Haust. et Gargarima.

5th.—On the evening of this day he was found spitting blood in very considerable quantity. He says that about 9 P.M. he felt something give way in his throat, when "a nasty discharge" was brought up, and followed by blood, after which about three pints of florid blood were poured from his throat. The right tonsil was now seen to be partially covered with a coagulum of blood. Pulse 110, with some power; skin hot; bowels open.

He was immediately bled to the amount of six ounces, when he fainted. A saturated solution of alum to be used as a gargle occasionally: at all other times to dissolve ice in his mouth.

6th. The hæmorrhage has nearly ceased; a large quantity of half-digested blood has been passed by stool; the glands of the neck on each side are hard and painful; pulse 96, not much reduced; bowels not relaxed.

To continue the Infus. Menth. Sulph. with the addition of ♏v. of dilute Sulph. Acid, ʒj. of Sulphate of Magnesia, and ʒss. of Syrup of Poppies, three times a-day. The use of ice to be continued also.

On its being subsequently intermitted, there was on the 7th a slight return of hæmorrhage, the sputa being sanguineous, and stools containing blood as before. He was then ordered to continue the draught, and to take at bed-time, Hydr. Submur. gr. ij. Extr. Coloc. C. gr. vj.

9th.—The white specks which were observed on the right tonsil are fast diminishing in size.

An alum gargle to be used, with Tincture of Myrrh.

14th.—The right tonsil remains enlarged, although the specks upon its surface have entirely disappeared.

Quinæ Sulph. gr. ij. to be added to the draught.

On the 23d he was discharged cured.

Remarks.—The foregoing case attracted the greater attention on account of the similarity which it bore to another case of cynanche tonsillaris, attended with severe and even fatal hæmorrhage, which had occurred some time ago at the Middlesex Hospital. The appearance of the tonsils in that case (of which Dr. Watson gave a full account in the 3d volume of the Medical Gazette) was precisely the same as in the one which has now been related; the cause also was the same—viz. exposure to cold; and the effect the same—namely, the formation of an

abscess behind and below one of the tonsils, by which an artery must have been ulcerated and arterial hæmorrhage produced. But in Dr. Watson's case, the abscess (which was formed under the *left* tonsil) was larger and less tractable, and formed an external as well as internal opening. The hæmorrhage also was much longer continued; and, when the patient sunk, it was found that the trunk of the lingual artery had been divided by ulceration.

The history of these cases may serve to point out the situation in which an abscess is liable to be formed in consequence of inflammation of the tonsils and fauces—whether matter be formed in the tonsils themselves or not: viz. immediately behind and below one or the other of these glands;—and when such an occurrence has taken place, we see likewise the danger that may subsequently ensue.

In the latter of these cases we may observe the excellent effect which ice, suffered to dissolve in the mouth, has in allaying inflammation of the fauces and restraining hæmorrhage; for, upon the ice being accidentally withdrawn, in this case, for a time only, somewhat too early, the bleeding was immediately renewed.

TREATMENT OF CHOLERA AT LYNN.

We have received a letter, signed by seven highly-respectable practitioners at Lynn, in reference to a postscript appended to Mr. J. B. Whiting's paper, in our No. of the 8th instant, in which he says that the plan of treating the premonitory symptoms of cholera by means of opium and astringents was recommended to them by his brother, Dr. Whiting. The medical gentlemen whose signatures are appended, deny that they were influenced or guided in any way by Dr. Whiting; to whom, nevertheless, they mean no disrespect.

Mr. J. B. Whiting, hearing that such a letter was sent, has also addressed us. It appears that his brother, some time ago, wrote to him, advising the use of opium, and the letter in question was seen by almost every medical man in Lynn. We cannot make room for the letters, which are of no general interest; and we trust that, on consideration, the several parties will think we have adopted the wisest course.

CHOLERA AT ETON (?).

We have received a letter from Mr. Moss, in answer to the one from Mr. Okes, inserted in our last. Mr. Moss reiterates his former statements—says Dr. Fergusson coincided with him in opinion, regarding the case alluded to by Mr. O., and adds, that he furnished the addresses of upwards of thirty patients affected with cholera. We cannot admit farther correspondence on the subject.

LIBRARY OF THE COLLEGE OF SURGEONS.

To the Editor of the London Medical Gazette.

Sir,

Would you allow me, through the medium of your useful publication, to suggest to the Council of the College of Surgeons the great benefit which would accrue to the members residing in London if the hours for admission to their valuable library were altered or prolonged. At present many are precluded from availing themselves of consulting the works contained in it, as their time during the day is wholly taken up with their professional duties; whereas, if the library were permitted to be open a few hours every evening, they would have an equal opportunity of benefitting themselves by its contents as those whose engagements are less pressing. By doing this, sir, you will much oblige,

Your humble servant,
A Reading Member.

London, Sept. 6, 1832.

P.S.—It will be remembered that there are many young men whose time is taken up in a variety of other ways, during the day, besides that of the active practice of their profession, and who feel much the privation of access to good books during the leisure hours the evening generally affords.

WEEKLY ACCOUNT of BURIALS,

From the Bills of Mortality, *Sept.* 18, 1832.

Abscess	5	Hooping-Cough	4
Age and Debility	81	Inflammation	36
Apoplexy	4	Inflammation of the Bowels & Stomach	34
Asthma	10	Brain	6
Cancer	3	Lungs and Pleura	2
Childbirth	7	Insanity	3
Cholera	154	Jaundice	1
Consumption	90	Liver, Diseases of the	10
Convulsions	39	Locked Jaw	1
Croup	2	Measles	15
Dentition or Teething	9	Mortification	1
Diarrhœa	2	Paralysis	2
Dropsy	15	Small-Pox	33
Dropsy on the Brain	17	Spasms	2
Dropsy on the Chest	4	Sore Throat and Quinsey	2
Erysipelas	1	Thrush	3
Fever	24	Unknown causes	2
Fever, Scarlet	12		
Fever, Typhus	4	Stillborn	13
Gout	2		
Heart, Diseases of	1		

Decrease of Burials, as compared with the preceding Week . . . 286

METEOROLOGICAL JOURNAL.

September 1832.	Thermometer.		Barometer.	
Thursday . 13	from 50	to 66	30·03	to 29·92
Friday. . . 14	45	64	29·90	29·80
Saturday . 15	41	62	29·90	30·04
Sunday . . 16	41	64	30·09	30·15
Monday . . 17	45	63	30·22	30·07
Tuesday . 18	50	61	29·94	30·04
Wednesday 19	35	57	30·10	30·36

Wind variable, S.W. prevailing.

The 19th clear, otherwise alternately clear and cloudy; rain at times on the 13th, 15th, and 18th.

Rain fallen, ·025 of an inch.

Charles Henry Adams.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 29, 1832.

ST. GEORGE'S HOSPITAL.

CASES OF ABSCESS IN THE PELVIS,

With Clinical Remarks,

By Mr. Cæsar Hawkins.

1. *Diseased Sacrum—Fæcal Abscess.*
2. *Diseased Hip—Abscess bursting into the Bladder.*
3. *Diseased Sacrum—Paraplegic Symptoms.*

Case I.—*Diseased Sacrum—Fæcal Abscess.*

Elizabeth Bartholemew, æt. 28, admitted June 13, 1832, under the care of Mr. Hawkins. She was confined about twelve months ago, but has not nursed her child, having been obliged to wean it when about six weeks old, on account of sore nipples. About nine months ago she had inflammation of the bowels, which required the application of leeches, and she subsequently suffered from cholera, by which illnesses her health suffered materially. About eight months ago she first experienced pain and tenderness in the left side of the abdomen and groin, followed in a fortnight by swelling; and, about five or six weeks after this, a puncture was made, by which she says two quarts of very offensive pus were evacuated. The discharge continued to be very fœtid for a few days, and then became more healthy, and the orifice is frequently quite closed. She has not menstruated since her confinement, but she says that at each menstrual period she has acute pain in the back, with bearing down, and pain in micturition, with frequent desire to make water; and at those periods the abscess discharges, for a few days, a thin fluid unmixed with blood, and then heals up again. She says she has not become much worse in health since the abscess first opened, but is very thin and emaciated, and perspires much, and has a troublesome cough, with mucopurulent expectoration; she sleeps ill, has a bad appetite, and a quick weak pulse. There is no pain or tenderness in the back or loins, and she says she never feels any except at the supposed menstrual periods. Matter can be pressed down from the iliac fossa from a considerable sized cavity, through two openings on the front of the abdomen, near Poupart's ligament.

June 20th.—Some fæcal matter was observed to come through the openings.

July 17th.—This circumstance was again observed to-day.

July 26th.—She has much improved in health under a nourishing diet, with a small quantity of wine and porter, and the use of bark and quinine, with opiates to relieve her restlessness and cough, and once some chalk mixture, on account of diarrhœa. The openings into the abscess have been enlarged, so that the discharge has been free, and it has gradually diminished, as if the cavity was contracting, and she has had no pain since the bistoury was employed.

To-day the discharge has been more profuse, and mixed with fæces; and she has general pain and considerable tenderness over the whole abdomen, with rigors, succeeded by hot and dry skin, and a hard pulse, 120.

Hirud. xij. abdom.; Haust. Salin. Effervescence, 4tis horis.

27th.—Relieved by the leeches.

31st.—Some return of pain and tenderness, with more profuse discharge, and mixture of fæces; much perspiration and great debility. The openings in the groin, which had contracted, being again laid open, a considerable cavity was found to have formed in the hollow of the ilium and the outside of the hip.

Vini Rubii, ℥vj.; Jusculi Bovini, Oiss. &c. Fotus Papar. Abdomini.

August 23d.—She has occasionally had the pain in the abdomen, which is relieved

by fomentation, and her wine has not been intermitted. She has also again suffered once or twice from diarrhœa, requiring opiate enemata and chalk mixture occasionally. Her cough has been lately more troublesome, and the expectoration more copious.

September 3d.—She has gradually got weaker, and has suffered much from irritation in the bowels, producing frequent diarrhœa. The abscess has lately discharged less pus, but frequently fæcal matter. Died this morning.

On passing a director from the wound, which was much contracted, it was found to lead directly upwards towards the spine; and on laying open the cavity of the abscess, which was reduced to a mere sinus, it was found to lead to exposed bone at the upper part of the sacrum, and the under surface of the body of the last lumbar vertebra, the cartilage of which at the posterior surface was softened, and a probe passed behind to the opposite side of the sacrum, which was also exposed and covered with a small quantity of pus. From the general cavity, which occupied the substance of the psoas muscle, a sinus ran outwards in the substance of the iliacus internus, and a portion of carious bone, of the size of a shilling, was found at the centre of the crest of the ilium, but did not extend beyond. Inwardly the cavity extended over the linea ileopectinea for about an inch and a half, and here communicated with the sigmoid flexure of the colon (which was adherent to the cyst) by two small orifices, about large enough to admit the point of a common director. The intestines were collapsed and generally healthy, but a portion of ilium, six inches in length, was adherent to the side of the abscess at the margin of the pelvis; recent lymph was deposited on its outer surface, and the mucous membrane was highly inflamed and ulcerated in many places. The uterus was also adherent at this part.

The lungs were much diseased, having several vomicæ at the apex of each, and tubercles throughout their whole substance.

Case II.—*Diseased Hip—Abscess bursting into the Bladder.*

Geo. Farrow, æt. 15, admitted July 5, under the care of Mr. Hawkins. He is a weak scrofulous lad, who has been constantly in ill health. About ten weeks before his admission, he had a violent cold, with fever, and an abscess formed over the right tibia, which, when opened, was found to be connected with dead bone; and a few days before his admission a fresh abscess formed in the calf of the leg, with much inflammation, in consequence of the matter from the back of the tibia not being able to escape, two-thirds of the circumference of the bone being dead. He has also complained, for the last three or four weeks, of much pain about the hip of the same side, and has had an issue behind the trochanter, which has been allowed to heal, in consequence of the irritation it excited. On admission, the openings over the tibia were discharging freely, and there was some swelling apparently connected with the femur rather than the hip-joint, which extended some way down the bone.

℞ Haust. Cinchonæ, ℥iss.; Acid. Nitric. ♏viij. M. bis die sumend. Jusculi Bovini, Oj. quotidie. Dieta ordinaria Cerevisiæ fortioris, Oj. quotidie.

July 31st.—Health much improved. Very little thickening about the femur, and the pain is nearly gone. The ulcers on the leg are also healthy, and much contracted in size, and the exposed bone separating.

August 3d.—Bowels much disturbed, with sickness.

℞ Haust. Rhei Comp. h. s. s.; Cont. Haust. Cinch. c. Conf. Aromat. ℈j. vire Acid. Nitric.

11th.—Leg very painful; bowels now confined, and painful.

℞ Hydrag. Submur. gr. iv.; Pulv. Rhei, ℈j. M. h. s. s.

12th.—The wound in the leg is sloughing rapidly, with violent pain.

℞ Decoct. Cinch. ℥iss.; Ammon. Carbon. gr. iv.; Tinct. Opii, ♏xviij. M. 4tis horis.

14th.—Wound still sloughing. The bark heavy on the stomach.

Vini Rubri ℥iv. quotidie.

℞ Mist. Camphoræ, ℥iss; Træ. Opii, ♏xv.; Ammon. Carbon. gr. v. M. 4tis horis.

17th.—Sloughing stopped. Pain ceased.

Cont. Mist. c. Træ. Opii ♏v.

21st.—Wound quite clean, the sloughs having separated.

℞ Infus. Cuspariæ, ℥iss; Acid. Nitric. ♏v.; Acid. Muriat, ♏vj.; Syr. Zingib. ℥ss. M. bis die.

℞ Mist. Camphoræ, ℥iss.; Træ. Opii ♏x.; Sp. Æther. Nitros, ℥ss. M. o. n. s.

28th.—Swelling and pain in the right groin, where a gland is felt enlarged and tender.

Appl. Hirud. vj.

September 4th.—Since the last note, it was evident that the pain in the groin depended on deep abscess, which has been fomented. He has had a good deal of anxiety and fever, much pain and tenderness in the thigh, which is swelled to half its length,

though no matter is perceptible to the touch, and he complains of his water scalding him.

5th.—Yesterday afternoon the pain ceased, and he felt a sudden desire to make water, and discharged a considerable quantity of pus *from the bladder*; in the course of three or four hours, probably about two pints having been evacuated: the portion first discharged being dark-coloured, but the rest becoming subsequently white and healthy in appearance.

Haust. Opiat. h. s. s.; Vini Rubri, ℥vij. quotidie.

11th.—Pain and swelling in the thigh much lessened, and his health is somewhat improved, but there is still a good deal of swelling and much tenderness in the groin and lower part of the abdomen on pressure. The water continues mixed with a great deal of healthy pus, which is discharged rather frequently, but without pain or inconvenience. Once or twice the water has been clear, and he thought the pain was increased by this apparent want of free communication between the abscess and the bladder. No fluid can be felt in the thigh.

20th.—Going on well, the abscess continuing to discharge by the bladder.

Case III. — *Diseased Sacrum—Paraplegic Symptoms.*

Jane Elwood, æt. 26, admitted August 1, 1832, under the care of Mr. Hawkins.

Ten or twelve years ago she fell against a stool and struck the sacrum, to which a blister was applied. About two months afterwards an abscess formed at the side of the sacrum, and some dead bone has come away since. She has been twice pregnant, and each time the abscess burst open again after having been healed. It has now continued open a considerable time, and a small piece of dead bone is felt at the bottom of the sinus. About two months ago, a fresh abscess formed over the posterior part of the right ilium, which is now of large size. Since this has formed she has become almost completely paralytic in the lower extremities, especially in the left, which was always weak, and she has had paralysis of the bladder and rectum, so that both the urine and fæces are discharged involuntarily. Her health is much impaired, and she is much emaciated.

August 3d.—The abscess was opened, and above a pint of pus evacuated.

5th.—℞ Haust. Cinch. ℥iss.; Conf. Aromat. ℈j. M. ter die.

8th.—The bark not borne well; bowels constipated.

℞ Hydrag. Submur. gr. iij.; Pulv. Rhei, gr. xv.; Pulv. Zingib. gr. x. M. h. s. s. Omit. Cinch.

14th.—℞ Infus. Calumbæ, ℥iss,; Ammon. Carbon. gr. v.; Confect. Aromat ℈j. M. bis die. Cerevisiæ fort. ℈j. quotidie.

22d.—Improving; although another smaller abscess is burst in the loins.

September 1st.—Both abscesses healed up. She has regained much flesh and strength, and is able to sit up regularly. She can use her legs so as to walk with the assistance of another person, and the bladder and rectum are recovering their power, so that she can retain all but very fluid evacuations, and can hold the water for several hours, with perfect power of expulsion.

20th.—Nearly well.

Clinical Observations.

There are several cases of abscess about the pelvis which have been recently under your observation, which I will take as the foundation for a few remarks, as they are by no means unfrequent, and are sometimes obscure and difficult to manage. In one patient, who died a few days since, you have observed an opening in the front of the abdomen discharging fæces: would you look for the cause of such an occurrence in the sacrum? There is a second patient, under Mr. Keate's care, who has had an abscess opened in nearly the same situation, at the side of the abdomen, from which a piece of the os pubis has come away. In a third, a lad has had scalding in making water, with purulent discharge from the bladder; the cause of which is not in the bladder or kidney, but in disease of the thigh-bone or hip-joint. Here, again, are some preparations, in which abscesses connected with the hip have made their way into the pelvis in other directions. In short, the causes of these abscesses may be so numerous, and their course so varied and extraordinary, as sometimes to render them very puzzling and complicated. In Farrow's case the cause was clearly in the thigh-bone, in a very scrofulous subject.—[The notes of the case which we have narrated were here referred to.]—But let us take the case of Bartholemew more in detail, which has shewn you the necessity of careful examination to ascertain the cause of such an abscess.—[Mr. H. here read some notes of her history at the time of admission.]

Now a large abscess in the iliac fossa, or extensive sinuses in the groin or lower part of the abdomen, in which the probe may be buried, naturally lead one, in the first instance, to suspect psoas abscess, depending on diseased spine. The most careful examination, however, could not detect any tenderness in any part of the back or loins, nor any sensibility to the impression of a hot sponge, which sometimes discovers disease of the vertebral column when mere manual examination fails to do so. She had met with no accident, and expressly denied having any pain in the back, except periodically, which she herself attributed to menstruation. But if there was no disease of the

spine, might it be a simple abscess in the cellular membrane of the psoas and iliac muscles; or might it be an abscess connected with some disease of the ovarium? I saw a patient formerly, at the Asylum, with Dr. Seymour, when we were colleagues at that institution, who had a large abscess in the ovarium, which burst and discharged by the vagina; subsequent to which she was admitted, under our joint care, into this hospital since we have have been colleagues here also: the abscess having now ulcerated both into the colon and the bladder, so that part of her urine, and the fluid part of the fæces, with flatus, came through sinuses in the groin resembling those of Bartholemew, and she was always worse at the periods of menstruation, when the discharge used to be coloured, no doubt by communication with the vagina. I recollect another woman also, when I was house-surgeon here, who used to mentruate regularly through the abdomen; in whom the bones of an extra-uterine fœtus were evacuated by abscess several years after conception. Now Bartholemew also said that the abscess used to re-open, with much pain, at each period that she ought to have menstruated, though the discharge was not red, but, thin, neither was it per vaginam. In a few days, however, further light seemed to be thrown on her case, for some fæces were found to come away by the openings; which circumstance, combined with her history of cholera and inflammation of the bowels before the formation of the abscess, and the very fœtid quality of the matter evacuated at that time, seemed to make it most probable that the abscess depended on ulceration of the sigmoid flexure of the colon. It is true a fæcal abscess forms most frequently on the right side, from lodgment of fæces or foreign bodies in the cæcum, but they may occur in other situations also; and you may perhaps recollect a man who was under my care not long since, with an abscess in the umbilicus, which, from the smell and colour of the discharge, I have no doubt communicated with the arch of the colon, though fortunately I had no opportunity of verifying the fact, as the man got well.

On whatever cause, however, the disease depended, the treatment was clear; the indications being to prevent accumulation of matter by giving it a free exit, and to support her strength by medicine and diet, although, from the apprehended state of her chest and her great debility, the chances were much against her recovery. At first, indeed, she improved very much, but observe the remainder of her case. [Mr. Hawkins here referred to the case-book for the detail before given.]

Now you will observe, from this case, that the neighbourhood of an abscess to the intestines is not unattended with dangers which are not usually anticipated from a psoas abscess. The intestines (both the colon and the small intestine) adhered to the side of the abscess—both were inflamed—both were ulcerated in the interior, and the colon was even ulcerated through all its coats, making an artificial anus of a dangerous and nearly hopeless kind. You will observe also, that ulceration once excited, your remedial means are constantly counteracted, repeated attacks of diarrhœa destroy your patient's strength as fast or faster than you restore it, and they frequently sink under the disease from this cause only. But this is not all—there is danger also of general peritonitis; especially of that low and fatal kind which so often occurs in debilitated persons. You observe here some lymph on the small intestine, which was adherent to the abscess, but it was merely local, and was not severe enough even to require leeches more than once; so that, in fact, I considered her in greater danger from diarrhœa and irritation of the mucous membrane, than from peritonitis, and treated her accordingly, endeavouring to quiet this irritation while I supported her general strength. Take care, however, not to mistake the apparent debility arising from inflammation of the peritoneum for real weakness. A man was under my care with an abscess between the external and internal oblique muscles, which I opened. This man was carried off in about forty-eight hours by extensive peitonitis, and I found, on seeing him after, that on the first accession of the inflammation, wine had been ordered for the apparently sudden debility.

We found then that the cause of her abscess was disease of the front of the sacrum, and the junction of this bone with the last lumbar vertebra, the matter making its way along the psoas muscle; and this is a common course when the disease is on the inside of the bone. But it may proceed in other directions. Here is the os coccygis of a patient of mine, which I removed by operation, together with part of the sacrum. He was admitted into the hospital, with a fistula by the side of the anus, for operation, but of course, on finding that the probe touched the sacrum high up in the pelvis, the operation was not performed. I was enabled to make an opening behind the sacrum, (where the rectum had been opened by ulceration,) and afterwards removed this bone; the consequence of which was that the fistula was cured. A boy was under my care with a sinus behind the upper part of the sacrum, for which I could find no diseased bone or other cause; but after some little time, upon making an extensive incision of the sinus through the gluteus muscle, by the side of the sciatic nerve, I discovered that the opening led up again, at an acute angle, through the sciatic notch into the in-

terior of the pelvis; the whole of the inner surface of the sacrum being carious or dead. Sometimes the abscesses from diseased sacrum proceed in several directions, even on both sides of the body at the same time. If, again, the posterior part of the sacrum be the seat of the disease, the abscesses will be over the bone itself, or on the posterior surface of the ilium, or in the loins, as you see in Elwood's case. This patient has also another set of symptoms, which Bartholemew did not suffer from, and which are more rare. [Mr. Hawkins here mentioned the particulars of her case, showing the occurrence of paraplegic symptoms.]

Next, as to the treatment of such cases of abscesses from any cause about the pelvis.

1st. Is there an abscess not yet opened? If the skin is getting thin—if the patient is suffering much irritation from the formation of matter, and especially if he has the peculiar symptoms of a *foul abscess*—*i. e.* one containing sulphuretted hydrogen, either from diseased bone or fæcal abscess, or sloughs, let out the matter as soon as possible. You saw how immediately Elwood was relieved by this means, and how much less Farrow has suffered since the pus came away through the bladder than when it was confined among inflamed parts. You lessen also the danger of peritonitis by taking off the tension of the abscess. A poor little chimney-sweep was mounting a donkey, when he fell off, and trying to get up again he fell over on the other side, and hurt himself considerably on the hip. Three weeks afterwards, he was admitted into the hospital extremely emaciated, and with high irritative fever. During the first few days I could not ascertain the exact cause of his sufferings, and during that time he was repeatedly threatened with peritonitis, which required leeches and other measures. Then I discovered fluctuation over the front of the iliac region, and making an incision through the linea semilunaris, some way above Poupart's ligament, I let out a few ounces of pus from between the peritoneum and the transversalis muscle, the finger passing behind the rectus muscle on one side, and into the iliac fossa on the other side, within the abscess. From this time there was not the least sickness, or tenderness, or tension of the abdomen, and he got rapidly well.

Or 2dly, is the abscess already open, but the openings not sufficiently large to allow a free exit to the pus?—Then enlarge these openings, or make another, if possible, still nearer to the seat of the disease. You saw how much Bartholemew was relieved by this incision two or three times, and at the time of her death the abscess had contracted to a mere sinus, and, but for the diseased bone at the bottom, such a sinus may altogether fill up. It may do so even when some disease still remains, as in Elwood's case, whom I do not consider permanently cured, though all the openings are at present closed. I should wish, if I have an opportunity, to make a counter opening in Farrow's thigh, to prevent the pus entering the bladder, but I do not at present feel sufficiently positive of the situation of the abscess to do so, though I suppose it is in contact with a considerable part of the femur.

3. A third indication is to relieve irritation by opiates, and to support the patient's strength by proper food, by bark or quinine, and other means which I need not enumerate. Even where the abscess depends on diseased bone, and the bone is not accessible, constitutional means alone will sometimes effect a cure. A young woman called at my house a few days ago perfectly well, who was formerly under my care with disease either of the ilium or sacrum, who had two sinuses in the groin like Bartholemew's, one in the course of the psoas muscle, the other leading deep into the pelvis and communicating apparently with the vagina, as pus had escaped in that direction, and who had abscesses also in the lower part of the loins, where I felt diseased bone at some depth.

4. Is the bone carious or ulcerated, but not dead?—If accessible and superficial, stimulating applications, especially of nitric or muriatic acid, certainly assist in restoring a healthy action in the vessels of the part, while you attend to the more important object of altering the state of the constitution, and under their use the ulcer may heal, or some portion may exfoliate, and the surface below become healthy. If not accessible, as for instance, when on the inside of the sacrum, as in Bartholemew's case, something may no doubt be done at the proper period by blisters and counter-irritants, in the same way as you know that caries of the vertebræ is often checked by their employment; some benefit might perhaps have been obtained if a caustic issue had been made early in the case of Bartholemew. But unfortunately, in general, the insidious nature of the early symptoms prevents the early recognition of the disease, and counter-irritation has less power in diminishing suppuration than it has in preventing its occurrence; besides which, issues have less power over scrofulous disease of bones than over ulceration of the cartilages of joints. The issue did no good, for instance, for Farrow's disease of the thigh bone, and they do little for caries of the sacrum or innominatum.

5. Is there some dead bone, separated, or in progress of exfoliation?—Much more good can be done, than is usually imagined, in these cases, by the same treatment which you so often see employed in this hospital in necrosis of the long bones. In Mr. Keate's case, a considerable portion of the pubes has spontaneously separated, but this is generally a very tedious and slow process if left

entirely to nature, and you can hasten the cure by laying open the dead bone, and extracting it with the forceps or bone nippers. An old soldier, whom I have twice operated on for strangulated hernia, received a musket wound at the siege of Badajoz, the ball passing through the side of the abdomen, through the ilium, and out again at the back of the thigh; the wound had remained open ever since, discharging so copiously and exciting so much irritation, as frequently to incapacitate him for active exertion. I cut down to the bone, through the glutei muscles, and removed a portion of loose bone, and cut off some soft carious bone with a chisel, leaving an opening which allowed several fingers to pass through it into the iliac fossa. The wound, which had remained open for nineteen years, healed quickly, and has continued sound ever since. I recollect a patient of Mr. Brodie's, from whom a considerable portion of the tuberosity and ramus of the ischium was removed by an incision through the adductor muscles. Here, again, is the os coccygis of another patient, which I removed by operation, with relief of the same kind.

There are only two other circumstances connected with abscesses about the pelvis which our time will allow me to allude to. The first is the frequent occurrence of phthisis, in conjunction with large abscesses, in this situation, which you have witnessed in Bartholemow's case. The coincidence is very common, and renders it necessary to give a guarded prognosis, even in cases where the local condition seems tolerably favourable. It may be that tubercles in the lungs dispose a person to have abscesses formed in the pelvis, for the same reason that a phthisical state of the lungs so frequently occasions piles and fistulæ in ano; the alteration, namely of the circulation in the extremities of the vena portæ, owing to the mechanical obstruction in the lungs; or else it arises from the debilitated condition of the patient, which calls into activity any latent disposition to disease in the lungs.

The other circumstance is this—you see that a portion of the ilium, out of the course of the psoas abscess, was exposed and dead in this poor woman, and this is, in fact, frequently found to be a consequence of large abscesses; so that a person with disease in the spine frequently has caries established in another part of the spine, or in one of the bones of the pelvis, from the mere contact of matter. This serves to shew you the propriety of opening even chronic abscesses tolerably early, lest the addition of dead bone in another situation render the case more complicated, and the cure more uncertain.

TEST FOR COFFEE, &c.

To the Editor of the London Medical Gazette.

SIR,

As that superficial but useful method of examining substances, termed *testing*, is in vegetables attended with difficulty, any attempt to simplify it must be useful: accordingly, I have sent, if you deem it worthy of an insertion in your excellent journal, an analysis of Rye (*Secale cercale*) and Coffee (*Coffæa arabica*,) both raw and roasted; and the former being very frequently used to adulterate the latter, I have indicated a test for the detection of the fraud; and as your journal is, also, I believe, devoted partly to the diffusion of chemical science, I have copied in my analysis of the leaves of the *Arum maculatum*, none having been published since the discovery of its alkaloid element.

	COFFEE.		RYE.	
	Raw.	*Roasted.*	*Raw.*	*Roasted.*
Lignin . .	50·5	49·0	20·6	18·0
Caffein . .	3·0	—	—	—
Tannin . .	—	2·0	—	—
Starch . .	—	—	49·4	—
Resin . . .	1·5	6·0	5·0	7·0
Amidine*.	—	—	—	38·0
Gum . . .	12·0	?	?	—
Albumen .	1·0	—	?	—
Gluten . .	—	—	4·0	2·0
Extractive matter .	26·0	30·0	18·0	20·0
Carbonized do. . . .	—	8·0	—	11·0
Loss . . .	6·0	5·0	3·0	4·0
	100	100	100	100

Now, as it is well known that iodine forms a blue iodide with amidine or starch, and as rye contains 38 per cent. of the former substance (when roasted,) while it is absent from coffee, it follows that a mixture of roasted rye with roasted coffee is easy of detection, of which any one can convince himself by a simple experiment: for this purpose the alcoholic tincture of iodine is the most convenient form of the test.

* I have here limited this term to starch torrefied below 480°.

The analysis of the *Arum* indicated the presence of the following substances:

1. Aronia } combined.
2. Malic Acid }
3. Malate Lime.
4. Phosphate do.
5. An Ammoniacal Salt.
6. Chlorophylle.
7. Gum.
8. Lignin.
9. Wax.
10. Albumen.

The ashes gave traces of copper.

I am, sir,
Yours obediently,
GOLDING BIRD, JUN.

44, Seymour-Street, Euston-Square,
Sept. 17, 1832.

PAPERS ON CHOLERA.

FACTS AND OBSERVATIONS

Relative to the Nature and Treatment of the

FEVER OF THE PESTILENCE,

COMMONLY CALLED

THE CHOLERA MORBUS.

By WILLIAM JOHN THOMAS, M.R.C.S.

In my last communication I brought forward a series of facts and observations relative to the communicability of the fever of the pestilence. In my present paper I shall relate, as precisely as possible, the characteristic symptoms of this singular affection; and I shall more especially advert to those symptomatic modifications of the complicated variety of the disease, which I have had an opportunity of observing during my professional investigations. To enter into a detail of all the cases would be impracticable at the present moment, although every case that I have seen has presented shades of variation from the maximum of intensity, which to the investigator of morbid phenomena possess considerable scientific attractions. The fever of the pestilence, unlike any other disease with which I am acquainted, appears to comprise within its comprehensive range a vast variety of symptoms, from the first dawn of the irritability of impending congestion to that solemn and mortal appearance of constitutional agony, when the hand of death is about to interrupt pulsation, and the machinery of life to stand still for ever. Many of the local diseases are occasionally simulated in the symptoms of this pestilential malady. Thus we are presented in this epitome of diseases with the whole of the second section of the first order pyrexiæ, with a majority of the genera of the order phlegmasiæ, with an occasional appearance of the exanthemata, and with parts of the fourth and fifth orders of the first class. Nearly the whole of the comata adynamia and spasmi have occasionally appeared in the train of this disease. The sixty-fifth, sixty-sixth, and sixty-seventh genera of the order vesania, with a collateral supply from the cachexiæ, and also from the fourth class of Dr. Cullen's nosological arrangement, are simulated in the symptomatic modifications of the pestilence. When I reflect upon the long train of evil symptoms, so intense in their action, and so varied in their manifestations, that frequently succeed the slight watery purgations, I cannot but regret that the attention of the physician should be directed to an individual feature of an insidious disease. Having observed with minute attention the progress of the epidemic in some hundreds of cases, I have been led to conclude that the disease which has been called the cholera morbus is, in fact, a pestilential fever of a most formidable character. The purging, cramps, and vomiting, upon which so much stress has been laid in the most fatal forms of this complaint, are occasionally so trifling as to attract little attention. Fevers in general have their different stages: the premonitory stage; the cold stage, or collapse; and the hot stage. The more durable and intense the second stage may be, the more formidable will be the stage of reaction. In the fever of the pestilence we have the premonitory stage, in which the constitutional irritability deranges the sympathies and disorders the specific action of the various organs. This irritability of impending congestion is an effort of nature to throw off the exciting cause of the disease. It is, in fact, the commencement of the fever, and all fevers commence in the same manner; for the cold stage of fever is merely the first manifestation of the predominance of the morbid power over the salutary efforts of nature. This view of the sub-

ject may, in some degree, answer the question which has frequently been put to me, How can you consider this person, who died in the blue stage, to have expired in a fever? If the majority of deaths occur in the cold stage, surely you cannot consider such persons the victims of a disease which might have cut them off had they survived long enough to suffer from its effects? That the majority of deaths takes place in the cold stage I will readily grant; for upon referring to my private note-book, I find out of 45 deaths, 33 took place in the cold stage, or stage of collapse, and 12 in the consecutive fever, as it is generally called, but which I maintain to be merely a development of the operations of those latent pestilential powers which were acting with their sedative energies on the nervous system, to produce the vascular collapse. If then we can bring ourselves to consider the blue stage of cholera merely as a part or parcel of that constitutional influence from which it originates,—as one step only in the progress of a most formidable malady, we shall find our views of the nature of the disorder more enlarged, and our treatment more comprehensive and scientific. The disease which we are now considering is, in fact, a most formidable constitutional affection, and not a local malady, as the term cholera would appear to imply. I do not mean to underrate the serious character of the affection of the bowels, but we should dispassionately consider this affection in relation to the constitutional disorder, in order to ascertain precisely whether these watery purgations be sympathetic or idiopathic operations. It is most difficult to trace the almost invisible entanglement of causes and effects, in a disease so mysterious as the present; and it behoves us, in our progressive investigations, to be well aware that our deductions are drawn from incontrovertible facts. It may be expedient in this place to glance with a physiological eye upon the constitutional origin of the disease:—in doing this I shall take as data certain facts which I shall presently adduce. In my communication, page 644 of this journal, will be found a table of twelve distinct instances of the communicability of the fever of the pestilence. From these facts it appears that persons who have been in communication with each other have been affected by the same disease in rapid succession: from which evidence we necessarily infer that some morbid peculiarity must have emanated from the person first indisposed, and have been imparted to other individuals in the order of succession. Taking it for granted that some specific power has been in operation to engender in other animal bodies symptoms identical in character with those exhibited in the person of the original individual, we are led to inquire what that specific power may be, and what is the nature of its operation. In this part of our inquiry we should be compelled to pause, if a most important fact did not present itself as another step in our ascent upwards to the proximate cause of the phenomena of choleroid irritation. I have frequently perceived in persons labouring under the malignant collapse, and also in certain conditions of the fever stage, a peculiar effluvium, apparently produced by a pestilential decomposition of the fluid portions of the diseased bodies. This effluvium possesses an odour of so peculiar a character, as to lead us naturally to suspect that certain specific properties are contained within its essential principles. The effluvium mingles with the atmosphere, and the air which we breathe being impregnated with the volatile virus, is inhaled into the lungs. From the lungs it is, of course, returned with the arterialized blood into the vascular system, and through that medium introduced into the brain. The first impression upon the nervous system is that of a slight sedative; the vital functions, and more especially the nervous energy, being depressed below the standard of natural excitement, the functions of the great circulating organs are impeded. The ramifications of the sympathetic, which supply the capillary arteries of the intestines with nervous power, have their functions, in some degree, suspended, and of course the contractility of the muscular coats of such vasa vasorum must be diminished in proportion as their stimulus of irritability is withdrawn. Instead therefore of exhaling an eliminated fluid, prepared from the serum of the blood through delicate calibres, we shall have these capillary points irritated by spasmodic contractions of their circumvesting tunics—sometimes contracted and rendered impervious in their ducts by the salutary efforts of the vital powers to throw off the morbid sedative, at other times

losing the resiliency of their parietes under the negative operations of the rapidly depreciating stimulus of the nervous system. The exhalants, thus relaxed, pour forth the serum of the blood, and suffer the component parts of the vital fluid to escape into the intestines. The same sedative power operates upon all the capillary organs. The excretory ducts have their excrementa completely altered in their peculiar properties. The ramifications of arteries are paralysed, and the veins, dependent in some degree upon the vis a tergo, become congested, —the heart itself becomes, ultimately, deranged in its action. To the different degrees of intensity in the operation of the sedative powers, are to be attributed all the morbid phenomena of choleroid irritations. The sympathetic vomiting and purging, the cramps in the extremities, from the depreciated energy of the spinal nerves, and all that counterbalancement of the capillary circulation, originate from the rapidly subsiding powers of the nervous system.

These opinions might lead us necessarily to infer, that infection must always ensue when the infecting power is applied to the brain. This, however, we find not to be the case; and the reason of the escape of so many individuals may be assigned to some peculiar energy of their vital powers, by which the morbific germs are either immediately decomposed or instantaneously thrown off from the system. When, however, a pestilence of so formidable a character becomes universally epidemic over large tracts of land, there are, without doubt, certain geological causes conspiring to produce effects so universally diffused. It is highly probable that from some of those grand and magnificent movements which occasionally take place in the interior of the earth, the atmosphere impendent above the surface of particular tracts of land, becomes changed in its normal properties. This change, although in many instances inappreciable by the most delicate instrument, may render the atmospherical agents peculiarly predisposed to retain and nourish the effluvial germs.

In the investigation of the causes of those fearful epidemics which occasionally strike terror and dismay from one part of the world to the other, we perceive several natural and physical causes conspiring to produce one effect. The geological condition of the unstratified materials of the primitive rocks, operated upon by unextinguishable subterranean fires, may produce a terrestial effluvium from the volatile substances contained in the bosom of the earth. This effect may in its turn prove a powerful cause in producing an alteration in the normal electricity of the atmosphere. An alteration in the electrical condition of the air may, in like manner, predispose the aerial agent to become a receptacle for the miasmatic exhalations from the surface of the earth. Thus we may have the terrestial effluvium, the anormal electricity of the atmosphere, and the miasmatic exhalations from the surface, conspiring to produce a sedative effect upon the nervous system of individuals, which may only require the specific pestilential effluvium to bring the constitution into that precise condition in which the most trivial local irritant may cause an immediate development of the epidemic. It would be a most desirable point in medical statistics to ascertain the average period that elapses between the period of personal communication with infected persons, and the development of the disease in the individual communicant. By the accumulation of a multitude of facts an average might be obtained, which would throw much light upon the propagation of the disease. In the majority of cases I find that a few days only have intervened from the death of one individual in a family to the attack of a second. In some cases the disease has manifested itself in the course of the following day, as will be observed in No. 9. of the table of the communicability of the cholera. At page 645, a remarkable instance of escape from infection is related; a woman, in a state of intoxication, having removed the dead body of her husband (who died in the blue stage) from the bed upon which he expired, and, without removing the bed-clothes, entered within the contaminated coverings, and slept therein during the night. The man died on the 19th July, and at the date of my last communication the woman remained uninfected. From the period of the death of her husband she was daily intoxicated. To form some idea of the amount of spirits she consumed, I have been informed that she spent 18*s.* in one day in this pernicious practice. I shall now relate the sequel of this case. On the 24th August I was sent for early in the

morning to visit this woman, a district patient. I found her extended on the floor of the garret which she inhabited; the place presented the most perfect picture of desolation—not a single article of furniture was in the place, the bed-posts having been sent to the pawn-shop, and 4*s*. the amount lent upon them, having been spent in spirits. The woman was extended supine upon the floor, which was saturated with fluid purgations and the matter vomited. Her hands and arms presented to the touch a sensation of deadly iciness; the radial pulse was extinct; and she exhibited all the features of malignant collapse in the most intense degree. This wretched creature died at half-past one P.M. of the 24th August—precisely 36 days after her husband's death. Here then is a problem which the controversialists may resolve if they can. Did this woman receive the infection from the bed-clothes of her husband or not? If she did, were the sedative effects of the morbid poison prevented from developing themselves by the stimulus of the spirits? I have made most precise inquiries into this case. My opinion is that the stimulus of the spirit might have been withdrawn previous to her attack, on account of the woman's pecuniary inability to procure her favourite viands. The neighbours, however, state that she was tipsy daily, including the night of the attack, since the 19th July. The next question would be, Did she receive the full supply of spirits or not? this however cannot be ascertained; it may be presumed she did not, for nothing of this description was found in the house, and the neighbours state that she had not sufficient money left to pay for a glass of brandy, which they purchased for her before my arrival. The woman assured me, in the most positive terms, that she had not been near a cholera patient since her husband died. I have no reason to doubt this statement, for she would not visit the person who assisted her to remove her husband's body, although she was her intimate friend, and lived in the adjoining house. It may indeed be maintained that this individual was equally under the influence of the choleroid atmosphere with others who may have been attacked the same moment, and that the fact of her having slept in the contaminated bed may have had no relation whatever to the subsequent attack. It must, however, be remembered, that the woman who assisted her to remove her husband's body, took the disease six days after that event, and that this person passed through the cold blue stage of the pestilential fever, from which she ultimately recovered. If, therefore, there is one instance of the apparent communicability of the disease, we must allow that the two individuals *might* have received the contaminating principles from the same original source.

A few days since I was called to a district patient, who acted as nurse to persons attacked with cholera. She had that morning prepared the body of a patient, who died in the cold blue stage, for interment. This old woman had attended to the whole of the family, marked No. 8 in the table on the communicability of the fever of the pestilence. She informed me that she had been attacked twice with the disease. On the morning of the 22d of this month I saw her, in perfect health, at the bed-side of the patient: at 7, P.M. of the same day, my attendance was required at her house. When I arrived, she exhibited the following symptoms:—Cramps in the flexors of the fingers and of the legs; acute spasm, referred to the region of the colon. The eyes were injected and flushed; the tongue baked, brown, and adhesive to the touch. The pulse quick and rather full; there had been no diarrhœa, nor had the bowels been naturally relaxed. The woman was screaming loudly from the spasm of the bowels, and was much incommoded by the cramps in the extremities. I took about a pint and a half of blood from her, and prescribed a dose of calomel and opium (gr. xv. of the former to ½ gr. of the latter): she was much relieved by the treatment; the fever stage became developed, and considerable local determinations to the brain appeared in the course of the following day. The symptoms were put down in succession as they presented themselves by appropriate remedies, and in four days from the attack she was convalescent. The blood drawn from the arm was darker than the natural colour, and, upon being allowed to coagulate, presented the serum of the colour and consistence of milk, with a yellow tinge. Now, although the diarrhœa was absent in this case, yet the choleroid fever was present; and I have no doubt, that had the symptoms not been promptly ex-

tinguished, the case would have terminated differently. This case is remarkable from the almost immediate contamination of the patient. She had washed the dead body at 12 o'clock, and at 7 of the evening of the same day she was attacked herself. About the same period I was called to another district patient, who had visited a cholera case, and was seized with giddiness upon entering the room. I saw this person a few hours after the latter event, and found her labouring under that class of symptoms which I have taken the liberty of calling the irritability of impending congestion: she had vomited repeatedly, and the cramps were commencing. I bled her to a considerable extent, and prescribed gr. xv. of calomel with gr. ½ of opium. Determination took place to the brain, which was obviated by constitutional treatment and the local application of leeches. These two cases are examples of the rapidity with which certain persons become affected by the choleroid irritation, and of the great importance of an immediate suppression of the earliest symptoms.

The three principal stages of the complaint are so well known, that I shall not dwell upon the symptoms which they present. I shall, however, occupy the remaining portion of my paper, which I perceive is already nearly filled, by making a few observations on the phenomena presented in the complex variety of the fever. I have already stated, that I consider the disease, *ab initio*, to be a masked fever of a pestilential type; and that when the cold stage is over, the true character of the malady appears. There are, however, certain points which I have not met with in the course of inquiries from books written upon the subject of cholera, which I shall briefly touch upon. In the complicated varieties of cholera, I have frequently observed anomalous sensations of a most distressing character. When the system has been exhausted by repeated purgings of the watery excrementa, a group of symptoms presents itself, which I shall class under the emphatic title of "the constitutional agony." This classification of symptoms presents a curious epitome of physiological contradictions. I have now a case in point under my care:—A young man, aged 24, was attacked on the 27th of this month with malignant cholera. His mother died on the 24th of the same month in the state of collapse. When I was called to him, the countenance had assumed a leaden hue, the dark areolæ were present, and he had purged the watery motions. Symptoms of local congestion induced me to perform venesection: I could only obtain about two ounces of dark thick blood from the arm, which afforded no relief. Powerful stimuli were then prescribed. The purgations were arrested by cretaceous mixtures and aromatics. Calomel and small doses of opium were administered. In the evening the pulse became extinct. Although the radial pulse had vanished, yet the hands were hot and dry; the tongue, in opposition to this, was cold to the touch and moist. The skin of the fingers and hands was slightly corrugated; but the most remarkable circumstance was, the development of a group of symptoms, to which I have given the name of the constitutional agony. The patient was continually tossing about from one side to the other, and from his back upon his face;—not an instant elapsed without a change of posture. He groaned deeply. The face presented marks of extreme agony. He declared that he had not the slightest pain, and that he could not tell why he was so restless. For several hours not a moment passed without the patient yawning, and the jaw was opened so wide by the convulsive gape, that I seriously apprehended the condyloid processes would have been dislocated from the glenoid cavity. On Monday evening the pulse became extinct, and the hands remained hot and dry until Tuesday, when the icy dampness took place. The radial pulsations have never appeared up to the present moment; and from Monday evening to Friday morning, this individual has existed without a pulse at the wrist. The patient has been under the influence of powerful stimuli. Counter-irritation has been applied down the spinal column, and large doses of the submurias hydrargyri have been administered. The warmth of the hands has partially returned, and inflammation has become developed in the brain: he has had five or six profuse dark-coloured motions during the night, produced by the combined operations of the calomel and castor oil. Counter-irritation is again applied to the nape of the neck, and he is taking a grain of calomel every

half hour, without opium. The internal stimuli are partially suspended for the present. Nothing is more paradoxical in the medical art than the appearances of these counter-indicated indications. It indeed requires an extraordinary exertion of scientific skill to navigate such shattered barks between the whirlpool of Charybdis and the yawning abyss of Scylla.

I fear that I have extended this paper to an inconvenient length, but the great importance of the subject may, perhaps, be my best apology.

Liverpool, August 31.

SPONTANEOUS TERMINATION OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

Among the numerous communications on the subject of cholera, from correspondents proposing every variety of treatment, I have looked in vain for a suggestion of the possibility of the disease terminating spontaneously. To ascertain the truth of this being the mode of its termination generally, I think an object of so much importance, that I am induced again to request admission into your columns, to urge the necessity of calling attention to this point, which, if proved to be true, cannot be too emphatically made known. My proposition is, "That, in some instances, the disease continues its progress till the death of the patient; but, in the very large majority of cases, when left to nature, the disease ceases when it has produced a state of collapse, varying in intensity."

The arguments I adduce in support of this proposition are these:—

1. That under every conceivable mode and variety of treatment, there have been instances of recovery.

2. The more simple the form of treatment, the greater has been the average success.

We have records of recovery under circumstances of total deprivation of drink, when the system had probably lost half its fluids;—under stimulants, in every degree of excess;—under opium;—astringents and purgatives;—acids and alkalies;—heat and cold applied with every degree of ingenuity;—emetics, and the most anxious solicitude to allay vomiting.

Now, it is a fair inference, that under such contrary treatment, some of the modes must be injurious; yet, as there are some instances of recovery under every mode of practice, it proves, I think, the strong tendency to a natural termination of the disease, which even improper treatment cannot altogether subvert.

The second argument derives support from the host of evidence in favour of the homœopathic system—the saline treatment—the cold-water treatment—and all the more simple forms of treatment.—I remain, sir,

Your obedient servant,
John Geo. French.

St. James's Infirmary,
Sept. 19th.

[The papers which follow have been transmitted by the Central Board of Health.]

ADMINISTRATION of COLD WATER in the *Collapse Stage of Malignant Cholera.*

By Hardwicke Shute, M.D.

To the Medical Gentlemen of the Central Board of Health.

Gentlemen,

The following memorial was nearly completed, when the circular, requesting that the particulars of any plan of treatment which appeared to have been unusually successful in the Asiatic cholera, might be transmitted to the Central Board of Health, was put into my hands. It is intended to record the circumstances, favourable and unfavourable, which have attended the cold-water treatment, to state more fully the facts which appear most deserving of attention in the prosecution of that practice, and to suggest certain modifications, by which its success in some particular forms of the disease may probably be enhanced. As such it is, I conceive, strictly consistent with the object of the circular mentioned, and is now transmitted to the Board.

I am, Gentlemen,
Your obedient humble servant,
Hardwicke Shute.

Gloucester, September 15, 1832.

Memorial, No. 3.

From the manner in which my communications to the Board of Health, relative to the cold-water treatment of the second and third stages of the Asiatic cholera, have been received by the profession, I can have no doubt that the result of that practice in other hands, will at no distant period be made known. This great object of my former memorials being fully effected, I shall, on the present occasion, confine myself to some remarks which appear calculated to ensure a more strict adherence to the minutiæ of the plan recommended, and consequently a more satisfactory trial of its effects, and also to throw some light ultimately upon the nature of that formidable disease, with which, as professional men, we are now called upon to contend.

To those who have not had an opportunity of witnessing, and perhaps to those most conversant with the disease, it may appear rather extraordinary to speak of patients continuing in a state of collapse for 24, 36, and 48 hours, as a matter of frequent occurrence, and doubts may be entertained as to the identity of the collapse described by others, and alluded to in my former memorials; it therefore becomes necessary to state, that by the term collapse, I mean to express, the cadaverous expression and remarkable sinking of the features, which so strongly characterize the disease, accompanied by a leaden hue of the face and extremities, shrivelling of the skin, a pulse scarcely, and in most instances not at all, perceptible, and a corresponding state of the animal heat. I have seen patients continue in this state for 24, 36, 48, and even 72 hours. From a collapse of 24 and 36 hours many have recovered, some after a collapse of 48 hours, and even after a longer period, but that period has not in my own experience extended to 72 hours. Relying upon these facts, and comparing them with what I had previously witnessed, I hazarded the assertion, that in all cases of collapse the progress towards death is in a remarkable manner retarded by the cold-water treatment; and I shall take this opportunity of repeating, with the view of inculcating the importance of attending to the advice previously given, that the practitioner should be satisfied, and make not the least alteration in the plan of treatment recommended, so long as his patient is merely not getting worse. This fact, as pointing out the compatibility of ultimate recovery, with a prolonged state of collapse, may suggest to those who are pursuing a different plan of treatment, many difficulties which have arisen from an over anxiety to restore the circulation as soon as possible, and a more temperate view of the disease, by which those difficulties may be obviated. Speaking from my own experience, I should say that the most favourable and satisfactory cases of recovery have been those in which the pulse was not restored in less than 12, or 24 hours, and then as gradually as possible. I may also, upon this occasion, urge the necessity of a strict adherence, not only to the plan but the principle of the treatment—the principle of never administering any kind of stimulant which, in degree or extent, exceeds the impaired vital energy of the system. If my opinion on this subject be correct (and unsupported by the accumulated evidence of others, it does not profess to be more than opinion)—if, I say, the principle of treatment be correct, no practitioner can, in future, witness the death of his patient after the free administration of brandy, or other stimulant, in an advanced stage of collapse, and feel quite satisfied in his own mind that he has not infringed (we mean unintentionally of course) the sixth commandment.

In my second memorial I stated, that when the irritability of the stomach is excessive or impaired, the water taken with eagerness almost immediately rejected, and then again and frequently desired by the patient, reaction is established in a very great proportion of the cases; and I have now to add, that my subsequent experience, which has extended from 48 to double that number of cases, enables me to repeat the statement with increased confidence; and also the importance of not checking, by opiates or similar medicines, the vomiting, which, according to my views of the disease and its treatment, has a conservative tendency. In many instances reaction has been established, when it was necessary to rouse the patient from a state of apparent insensibility, and offer the water frequently and repeatedly, when the water was taken, if not reluctantly, without any evident satisfaction, and therefore the

treatment is not at all confined to the former cases; but I felt myself called upon, in my second memorial, to direct the attention of the profession to those cases more particularly, in which I felt assured that the cold-water treatment would lead to the most striking effects, and be attended with the same beneficial results as I had witnessed in my own practice, leaving it to others to ascertain how far the treatment might be applicable to a different form of the disease.

I also stated, that when the natural powers of the constitution are not unusually weak from extremes of age, or other causes (and of these causes previous habits of intoxication and inadequate support are the most prominent), such reaction will be followed by an early convalescence without fever, or the assistance of any kind of medicine. This statement, which has also been confirmed by my subsequent experience, is in itself an acknowledgment that recovery has not always followed reaction; and I shall now state some of the difficulties with which, in the less favourable cases, the practitioner will have to contend, and the reflections to which these cases have given rise, it being distinctly understood that I rely upon the cold water alone in all cases as the agent by which reaction is to be established, and in the more favourable cases as the sole agent by which convalescence is ultimately established.

In the most favourable cases, reaction is accompanied by an improved state of the alvine excretions, a cessation of vomiting and purging, a disposition to sleep, a restoration of the urinary secretions, and an uninterrupted progress towards a healthy state of the constitution. In others the return to health, after the pulse has been restored, is not progressive, and the alvine excretions will, upon examination, be found of an unhealthy character, and sometimes partaking, more or less, of the morbid state so peculiar to the disease. If the bowels are costive (by no means an unusual circumstance), the practitioner will seldom be wrong in suspecting a diseased state of the excretions. I have usually given ten grains of the pil. hydrarg., or of the hydrarg. c. creta, with or without castor oil, according to circumstances, and the advance towards convalescence has been subsequently uninterrupted. The patient, however, is most commonly in a state to which the ordinary principles of medical practice are applicable, and therefore the selection of the medicines employed may be left to the judgment of the practitioner, but with the precaution of avoiding such doses as may be too powerful in their operation. The necessity of this precaution arises from the singular fact, and the fact must never be lost sight of, that the constitutional powers seem to be more under the influence of debilitating causes after the pulse has been restored, than it was during the collapse. The same, or a minor degree of muscular exertion, which was apparently innocuous during the collapse, has, in three instances, brought on a relapse, and a relapse is, I am sorry to add, almost always fatal. If purging continue to an immoderate extent, the administration of one grain of the argentum nitratum has proved highly beneficial. In some instances the vomiting has been obstinate after reaction has been established, but this has generally happened when, at the commencement of the collapse, there was no vomiting at all. In more than one instance the urinary secretion was not restored, but my experience hitherto does not enable me to offer any particular observation upon these cases.

The next circumstance which I deem worthy of particular notice, is a tendency to local congestion, and chiefly of the brain. Vascular congestion of the conjunctiva is a frequent occurrence, but has not in itself appeared to require any particular treatment. The case, however, is sometimes otherwise, and in infants and young children more particularly, such congestion being attended with symptoms of pressure upon the brain. As connected with this subject I may also mention the occurrence of opisthotonos in two infants and one female.

This tendency to congestion, after the restoration of the pulse, naturally suggests the question, whether bleeding would in any way assist the cold-water treatment, and at what period of the disease, if any, it should be adopted. From the present state of the disease in Gloucester (there not being a single case in the hospital), there is little probability of my being able to determine the point, and therefore I must throw myself on the experience of others, merely suggesting that the period of the complaint I should myself select

for the experiment, would be that when the cold-water treatment had been persevered in long enough to justify the presumption that the fluidity of the blood had in some degree been restored. In one case a small quantity of blood, which was at first dark and venous, and in half an hour afterwards more florid than usual, was taken at this period of the disease. The pulse was restored at an earlier period than I had observed in other cases, but in the course of the night the patient became restless and irritable, was allowed to get out of bed frequently, had a relapse and died, confirming, as it appeared to me, my former impression, that a very gradual restoration of the pulse was an essential part of the cold-water treatment. I may here mention that in two cases this restlessness and irritability amounted, if not to maniacal aberration, to maniacal impatience and obstinacy, not very different perhaps to the state of mind which is said to occur in extreme exhaustion from exposure to excessive cold or starvation.

The occurrence of opisthotohos in the cases mentioned, naturally suggests the idea that the cold-water treatment might under certain circumstances be rendered more efficacious by the application of stimulants to the spinal canal. The cases recorded by Mr. Whiting are, in this point of view, highly interesting, but when the question is asked how far the application of blisters to the whole course of the spine may be regarded as essential to the cold-water treatment, I can have no hesitation in saying that it is not, inasmuch as the blistering plaisters without the water are wholly inefficacious, whilst the cold water is, in a great majority of the cases, a powerful and efficient agent without their assistance. From the state of the circulation on the surface during the collapse, the effect of the blisters must at that time, I conceive, be trifling and unimportant, but I think it highly probable that in some cases, in infants and children perhaps more particularly, their operation, when the circulation begins to return to the surface, may prove extremely beneficial, but were they had recourse to in all cases, and considered as constituting an important part of the cold-water treatment, consecutive fever would, I fear, be a much more frequent occurrence.

I shall close this part of my subject by stating briefly two general principles which ought to regulate our practice after the pulse and animal heat have been restored; the patient must be considered as having passed from a state of depression to that of exhaustion, and the fundamental principle of not allowing any kind of stimulant which is disproportioned to that state, must be strictly adhered to and enforced.

This candid statement of the difficulties with which, in the less favourable cases, I have had to contend, may possibly convey the idea that my confidence in the practice is not what it was, and that farther experience has not confirmed my former statement. It is quite sufficient to say that such is not my own impression, or the impression which this statement, if read with attention, and due allowance be made for the number of patients in which a practice directly opposed to my own principles had been pursued previous to their admission into the hospital, ought to convey to the minds of others. It is wholly unnecessary to say more upon this subject, because the value of the treatment recommended, must obviously be determined by the experience of others, and not by the representations of an individual. That experience, as far as it has hitherto come to my knowledge, is greatly in favour of the practice.

There is one circumstance connected with the pathology of the disease, and which, from the protracted duration of the collapsed state under the cold-water treatment, I may have had more opportunities of witnessing than other practitioners, highly deserving of mature consideration. The circumstance to which I allude, and which constitutes a distinguishing feature, if not pathognomonic symptoms of the Asiatic cholera, is a marked want of correspondence between the state of the circulation, and the functions of the brain. The fact of the mental powers being comparatively unimpaired was noticed soon after the first appearance of the disease, but the remark has not, in my opinion, been sufficiently extended to the powers of voluntary motion as connected with the brain, and to the powers of the muscles themselves. The continued and apparently undiminished sensibility of the nerves of taste has been strikingly exemplified in a great majority of the cases which have come under my notice. At a period of the disease when the circu-

lation is to all appearance arrested—when not more than a tea-spoonful of blood escapes from a vein which has been compressed and opened, and that blood is in a state little capable, according to our previous notions, (and these notions are probably correct,) of supporting any function — when the body feels as cold, I may say colder, than any inanimate object in its vicinity, the patient will take with eagerness repeated draughts of cold water, and reject with the utmost pertinacity every other kind of fluid. In one remarkable instance, when the collapse had been confirmed, and the pulse imperceptible for at least 48 hours, the patient complained of her bed, which contained no material more yielding and accommodating than straw, being very hard and uncomfortable. Under precisely similar circumstances the patients will turn themselves in bed, respire deeply and regularly, swallow without the least difficulty, and, if occasion require, get out of and return to bed without the least assistance. In one remarkable case of this kind a female succeeded in getting out of bed in opposition to all the resistance which three nurses could offer. The Asiatic cholera differs in this marked peculiarity from asphyxia, and indeed from any disease which has hitherto been described. The "entasia acrotismus" of Dr. Good approaches nearer to it than any other. This want of correspondence between the state of the circulation and the functions of the brain, as far as sensation, volition, mental action in a higher grade of perfection, and the powers of the voluntary muscles are concerned, demands, on the part of the profession, the most mature and unprejudiced, may I add unbigoted, consideration. It is a fact, which strikes at the very root, and saps the foundation, of the physiological principles most commonly adopted, and if of the physiological principles on which the practice of medicine is based, of medicine as a science, and will, in my opinion, lead ultimately to a greater revolution in that science than is at present anticipated.

The opinion of the profession as to the originality of my views of the disease termed Asiatic cholera and its treatment, is a matter of very secondary importance if put in competition with their correctness and consequent utility; and yet, in justice to myself, I cannot omit throwing out a few suggestions on this subject. In a comparative view of the different modes of treatment it will now be necessary to distinguish between the effects (I allude more particularly to the saline and antimonial treatment) of the medicines themselves, and of the water in which they are conveyed, or which is at the same time allowed. In having directed the attention of the profession to the agency of the water itself, I have materially deviated, as far as I know, from the course of other writers. There is a material difference, I may observe, between not refusing cold water, and urging its use as the agent by which reaction is to be established; between not opposing the desires of nature, and reconciling those desires with a conservative object, and with a long-acknowledged principle of therapeutics. That the vomiting is a conservative action, and as such should not be officiously interfered with or interrupted, is also an important, and, I believe, a peculiar item in the detail of the cold-water practice.

P.S. This memorial being explanatory of those already communicated to the Central Board, and published in the Lancet and Medical Gazette, I shall feel obliged by its being transmitted to the editors of those publications.

CALOMEL IN THE STAGE OF BILIOUS DIARRHŒA.—MERCURIAL INUNCTION IN THE STAGE OF COLLAPSE.

To the Secretary of the Central Board of Health.

Sir,

I have the honour to forward a short account of my method of treatment of the epidemic, in obedience to the circulars (No. 1. and 2,) addressed to the profession by the Central Board of Health, Sept. 3, 1832. It may not be improper to observe that, having served in India between six and seven years, where I treated the epidemic on a more extensive scale, it may be possible that I have approached the bed-sides of the sufferers, if not with more confidence, at least with more calmness, and with a degree more of what may be termed rational medicine, than those altogether unacquainted with the disease. I fear that what I have called "rational medicine" has been altogether lost sight of

during the panic, which has attacked even medical men at the onset of this most formidable disease. All our hardly-earned pathological, physiological, anatomical and therapeutic experience, has apparently been laid altogether aside in order to seek for new theories, new *modi medendi*, which have been for a time blazoned forth to the public, only to suffer defeat, nay more, by their failure they have increased the dread (already too great) and aggravated unnecessarily the feelings of the public respecting this vampire in the shape of disease. It would be exceedingly ill timed, at this period, to allude to measures which have rendered altogether abortive the wisdom, foresight, and competency of the sanitary regulations of the Central or Government Board of Health; with their permission and indulgence, however, at a fit opportunity, I have it in my power to prove that their measures have been in some instances altogether disregarded. To return to my immediate object—the Treatment of Cholera.

1st. When in the form of bilious diarrhœa.—In this stage, avoiding as much as possible all known ladentia, and employing in an equal degree all known juvantia, I have trusted entirely to the conf. opii, in doses gr. xxxvj. aquæ pimentæ ℥j. to adults, repeated at well timed intervals, watching the patient attentively. The success attendant on this treatment has been most signal.

2d. In that of rice-water evacuations.—In conjunction with the above I have employed doses of calomel and camphor, gr. iij. of each.

3d. In the stage of collapse—Here I have relied, *cæteris paribus*, entirely on full doses (℈j.) of calomel with moderate quantities (gr. j. or gr. ½) of opium, MERCURIAL INUNCTION, and calomel and camphor gr. iij. of each every half hour, or every second or third hour, combining with each dose as reaction became established one grain of pulv. calumbæ. The last case of malignant cholera (this day fortnight) is truly singular and worthy of a place here:—

Jane Fayrer, aged 52, residing at No. 3, Three King's Court, Whitecross-Street, of spare habit, had been ill several days with disordered bowels; on visiting her I found as bad a case of malignant cholera as ever I witnessed. I did not think her capable of bearing calomel at all. I therefore gave her gr. xxxvj. conf. opii, in ʒ vj. of aqua pimentæ. This was in part returned; she however experienced some relief. I then sent her the following—

> Camphoræ ℈j. Pulv. Opii gr. j. Calumbæ gr. vj. M. et divide in pulveres sex; one to be taken every half hour.

This was all the medicine she had. In the evening when I saw her reaction was perfectly established. It continued. She was left exceedingly weak: however, with the aid of the tinct. calumbæ, which I consider highly valuable, she has at length begun to resume her usual domestic duties. I did not expect this woman would survive six hours.

It has been my object to simplify, and at the same time to render effectual, as far as possible, my treatment of the disease, and I can safely say this method has been attended with, *at least*, as much success as any other with which I am acquainted. Viewing the disease, and I do, as one in which the nervous system is primarily and principally affected, I am inclined to place much reliance on moderate (not excessive) and well timed doses of opium, combined with calomel in the stage of collapse. Perhaps somewhat of the success of this plan may be attributed to climate and to the habits of life of the majority of those who have fallen under my care. I consider that the case of Jane Fayrer establishes the well-known caprice of the epidemic in a most extraordinary manner.

I have the honour to be, sir,
Your most obedient servant,
EDWARD HICKMAN,
Late Assistant-Surgeon
Honourable Company's Bengal Establishment.

No. 47, Chiswell-Street,
Sept. 21, 1832.

CALOMEL, GINGER, AND OPIUM, FOLLOWED BY A PURGATIVE—COPIOUS ENEMATA—FRESH AIR, &c.

To the Secretary of the Central Board of Health.

Newcastle, Sept. 19, 1832.

SIR,

I SHALL endeavour to answer the several queries contained in the circular which has reached me from the Central Board of Health with clearness and brevity.

In conducting the treatment of cholera, in all its grades and stages, I have always endeavoured to form a clear conception of the objects to be held in view, that every remedy might be prescribed with a distinct and definitive purpose. In the treatise which I published a few months ago, and which I directed to be forwarded to the Central Board, I endeavoured to explain the rationale of the disease, and the indications of cure which were then, and, with a few modifications, have since been my guides. It is now far from my intention to boast of success—hitherto, I fear, few practitioners have had cause to do so, whatever plan of treatment they may have pursued—but it does not appear that any plan has had a very decided advantage over that which I have pointed out, since the relative mortality has not been any where greatly lessened. It is true that the saline treatment, as recommended by Dr. Stevens, seems, in his own hands, to have been very successful, but, without meaning to throw doubt upon the accuracy of his statements, its efficiency requires to be confirmed by the experience of others. But to revert to the queries which I am required to reply to:—

1. The bilious diarrhœa, which so frequently constitutes the early stage of cholera (*premonitory* is an erroneous term, it is not a premonition of the disease, but a catenation of symptoms constituting the early period of the disease itself) is to be considered as an irritation of the mucous membrane of the bowels, occasioned by the poisonous agency by which cholera is produced. The process of digestion is interrupted, and irritating undigested matter remains in the primæ viæ. Our indications must be to relieve the irritation and expel the irritating matter. Four or five grains of calomel, two of ginger, and one of opium, will very generally answer the first indication, and a dose of castor oil, or other purgative, given an hour or two afterwards, will frequently complete the cure. It frequently happens that this form of the disease is accompanied with headache, and pain or tenderness at the scrob. cord. the pulse being full and quickened; in cases of this description venesection will be found of service. Occasionally the undue action of the bowels continues, the stools being bilious and feculent, and no other symptom of disease remaining. One or two doses of mist. cretæ. with five or ten drops of t. opii, will frequently check a purging of this description altogether.

2. When the discharges have assumed the appearance of rice-water, we must aim at relieving the irritation, checking the discharges, and restoring the biliary and urinary secretions. Rest, calomel and opium, as above, a smaller dose being given at intervals, the opium either being omitted or reduced to a small quantity, *e. g.* calomel gr. ii. p. zingib. gr. ii. opii gr. $\frac{1}{8}$ every hour or two hours, according to the urgency of the symptoms; when the discharges have been entirely restrained the opium should be omitted, and the calomel continued till bile and urine are secreted, often till the patient's gums become affected. Purgatives must also be given to obtain natural excretions. The best article of diet I have found, in these cases, to be gruel salted, of which some patients have drank enormous quantities with apparent benefit. Saline draughts, with an excess of alkali, have also been grateful and useful to the patient. Stimulants of every description injurious. The head and abdomen require close watching, and any symptoms indicating disease in either region, must be promptly attended to. Leeches or general bleeding must be employed on the first appearance of uneasiness. The latter agent must, however, be used with great caution. Some patients who have never fallen into the stage of collapse, have nevertheless ultimately sunk in the febrile stage, with head disease of an obscure character, beginning with slight giddiness and scarcely appreciable uneasiness, but steadily proceeding to coma, delirium, and death; the tongue becoming dry and black, and the patient exhibiting altogether the worst form of typhus fever. By close and early attention, the fatal tendency of the head affection has been successfully grappled with.

3. The stage of collapse I have found always difficult to conduct upon any well-established principles. When well-marked, I fear we can scarcely hope for any large proportion of recoveries, whatever plan is adopted. Still it ought not to be treated at random. Brandy and other stimulants appear to me uniformly to increase the gastric irritation. The calomel and opium, as before recommended, appear to allay that irrita-

tion, as well as to be useful in answering the indications already noticed. The salted gruel is often a very grateful drink: the temperature of this, or any other beverage, may be safely accommodated to the feelings of the patient. Copious injections into the rectum I have so frequently found useful, that I feel no hesitation in recommending their general employment in collapse. Emetics are of more doubtful utility than I was formerly led to believe, unless when crudities are known to remain in the stomach. On the whole, my main reliance for simply restoring vascular action is placed on copious drinks and copious injections, quietude, fresh air, and comfortable cloathing. When patients rally from collapse, it is often most difficult to ascertain in what causes their emergence from it has depended. I fear various remedies have often obtained the credit which has been due to the spontaneous efforts of nature.

From the preceding remarks it will be seen that the remedial agents which I have employed in cholera (and I believe my practice to have been attended with a full average share of success), have been such as the profession is familiar with, and that their employment has been conducted on established therapeutic principles. It is in this way only, I believe, that any considerable improvement can be effected in the treatment of this destructive malady, and not by experiments which are entered into without any distinct anticipation of their possible effects. I apprehend it is hopeless to expect a specific effect to be produced in cholera by any remedy whatever: in the stage of collapse, the condition of the stomach and bowels renders nearly inert the most powerful agents we can employ, and I am disposed to think they are more sensible to the stimulus of distention than any other, while it is least likely to occasion ulterior injurious results. Hence it is, probably, that large quantities of fluid introduced into the stomach and intestines are often found unequivocally beneficial. I am not certain whether it is really material to impregnate the fluids employed with saline and alkaline matters; but while agreeable to the patient, I am in the habit of doing so—when they cease to be grateful, they are omitted.

The late irruption of cholera in Newcastle has differed, in some of its features, from the former one; more especially in the frequency with which fever, of a bad form, has succeeded, even where the collapse has been incomplete—in the great difficulty of restoring the secretion of bile in cases of rice-water diarrhœa, and in the disease having shewn itself amongst the middling and higher classes of the community. Derangement of the stomach and bowels, diarrhœas, dysenteries, and cholera in its various forms and degrees, have been almost universal in the neighbourhood, shewing how general is the cause by which they have been produced.—I am, sir,

Your obedient servant,

T. M. Greenhow.

CHLORIDE OF POTASS—COMPOUND TINCTURE OF CAMPHOR.

Douglas, Isle of Man, Sept. 19, 1832.

Sir,

In compliance with the Central Board's inquiry (No. 2.) we beg leave to submit the following, as the most effectual consistent with our practice.

1st. When in the form of bilious diarrhœa, small and frequent doses of calomel, opium, and ipecacuan, in combination with chalk mixture, &c. &c.; which seems in general to have arrested the progress of the disease.

2d. In that of rice-water evacuations, blood-letting, pediluvia, and the frequent administration of calomel, antimony, and opium. We have also tried the use of croton-oil with apparent advantage, which seems to produce its effect by causing a transfer of action, and consequently changing the state of the serous evacuations.

3d. In the stage of collapse. When this stage of the disease has occurred, we have in general had recourse to the chloride of potass, in combination with soda and compound tincture of camphor, accompanied with stimulants of different kinds. Latterly we have tried cold water, according to Dr. Shute's system, and although a few recoveries have taken place under this treatment, yet we have reason to believe, that when persevered in too far, it induces very high consecutive fever, and consequent congestion of the brain.

Garrett and Harrison,

Surgeons.

FURTHER OBSERVATIONS ON THE BLOOD.

By Dr. Stevens.

To the Editor of the London Medical Gazette.

Sir,

A knowledge of the cause of the red colour of the blood is not a matter of mere curiosity; it is of great value to the physiologist, and of vital importance to the practical physician. It is now well known, for example, that the blood is both black and morbid in the last stage, not only of cholera, but in all other malignant epidemics: and therefore to restore its red colour and concomitant soundness, in such diseases, is, or ought to be, the chief aim of every rational practitioner. Most will admit this, and few will deny that physicians acquainted with the true cause of the redness of the blood in health, and its blackness in disease, will be more likely to accomplish a cure than such as are governed by hypotheses, which are not only in direct opposition to facts now well ascertained, but which have led to a practice of the most injurious tendency.

From the result of certain experiments, I could not help believing that it was not iron, but the natural saline matter of the blood which was the chief cause of its red colour. As one proof of this, I stated that when the blood was allowed to coagulate out of the body, the crassamentum became black in the centre in proportion as the *salt* serum left the coagulum and rose to the surface. This has been denied by some Irish experimentors, but the most perverse hypocriticism cannot disprove a fact which can easily be verified in any hospital.

In about twenty-four hours after coagulation, the greater part of the serum has separated from the clot; but the crassamentum cannot be entirely freed from its saline serum, either by this natural separation or by subsequent immersion in distilled water. But even the pretenders to more minute experiments admit that four-fifths of the saline matter are thus spontaneously abstracted from contact with the red globules; for they assign about three parts in every thousand as the residuary saline matter of the black clot, instead of fifteen, which are known to exist in the serum. Now because three parts of salt will not redden one thousand parts of blood, is it rational to infer that fifteen will not? Every chemist knows that turmeric-paper, a common test for alkaline matter, is not affected by it, unless in a certain concentration; so that the liquid which lessens the red of litmus does not change the bright yellow of turmeric-paper; and as the effect of the salt on the colouring matter depends on the quantity, perhaps it may strike some of your readers that as fifteen parts to the thousand are necessary to give the red colour to the blood of a healthy person, we have then no right to expect any thing like redness in a clot, where there is only three parts of saline matter in place of fifteen, which is about the quantity, I believe, invariably found in the serum of healthy blood.

While the colouring matter is in contact with the salt serum, as in the circulating current, it remains red; but after the blood is drawn from the veins, the clot becomes perfectly black only when the serum has almost entirely left the coagulum. When we cut out a piece of the red clot, before the separation, and suspend it in the air, at first it becomes more red, chiefly, as I believe, from the sudden removal of the carbonic acid; but in proportion as the salt serum drains out of it, the colouring matter becomes perfectly black, even though it remain *in immediate and constant contact with the oxygen of the atmosphere*; but neither air nor pure oxygen can redden the colour of this black clot. The redness can be permanently restored only by the addition of a certain quantity of saline matter. And if any one who has a doubt upon this subject will add the difference betwixt three and fifteen parts of salt, or immerse the black clot in water that contains twelve parts of saline matter to one thousand, he will then find that the black clot almost immediately changes its colour, and becomes as red as it had been previously to the drainage of its saline serum. It has also been proved that an extra addition of saline matter will not only strike a full red, but a hue that is highly arterial: and as this change can be effected *even in an atmosphere of carbonic acid*, can any one hesitate to admit that it is the salts of the serum, and neither iron nor aërial oxygen, which is the chief agent in striking the red colour with the colouring matter? Hence, though we admit that a clot which has been immersed in distilled water may still contain a small quantity of serum, yet this only proves that the

portion of salt in the black clot is too minute in quantity to produce any evident redness.

When we immerse a black clot in a saline fluid, it instantly changes from black to scarlet; but when we immerse a red clot in distilled water, it soon changes from red to black. The reviewer of my work, in one of your contemporaries, admits this; but denies positively that the water takes up even one particle of the salt: and this he proves by a careful experiment, about which, he affirms, there can be no deception. The red crassamentum, however, contains a saline fluid, which oozes out of it while immersed in the water; and if the two be well mixed together, as they ought to be, it is impossible that a salt fluid can be agitated in the same vessel with distilled water without the water abstracting a part of the saline ingredients. Is it not known, for example, to every cook, that oversalted meat can be rendered almost fresh by immersion in fresh water, which becomes proportionally saline? And will the critic pretend to prove by an accurate experiment that there is as much salt in the meat after immersion as there had been before? Yet he asserts as much, in reference to the crassamentum of the blood, in which the salt is more loosely combined. Few can be misled, however, by such sophistry; and, for my own part, I can place no confidence whatever in experiments which are in direct opposition to common observation: for the critic might, with the same propriety, attempt to prove by experiment that the river water which runs into the sea does not become impregnated with saline matter, as to say that distilled water does not take up a part of the salts when it becomes mixed with the salt serum of the blood: and if it does, the clot from which the serum has oozed out must naturally contain less saline matter, and of course become blacker than it had been before. In fact, any one who takes the trouble to make this simple experiment, will find that distilled water, in which the red clot had been immersed, acquires a notable proportion of the alkaline salts; and as this must be derived from the serum which oozes out from the crassamentum, it is very evident that the black clot also must contain less salt after immersion than it had done before.

It has been asserted that serum cannot redden the blood; but this is in direct opposition to facts. When we divide a clot that has been coagulated for twenty-four hours, we find that it is all perfectly black, except the thin film on the upper surface, which has been covered with serum. When we remove the one half of this clot, and put it aside, the black surface does not become red, even when exposed directly to the air; whilst the other black surface which remains on the serum, becomes not only red, but as florid or arterial as the upper surface.

The objections to which I have referred are so preposterous that they would scarcely deceive even a tyro; but a more plausible cavil remains to be considered. I have stated that carbonic acid is the chief cause of the dark colour of the venous blood, and that oxygen cannot directly redden the blood except through the medium of its saline ingredients; but that it indirectly reddens the colour, chiefly by its peculiar property of abstracting the carbonic acid, which is the true darkening agent. It has been attempted to disprove this proposition by an experiment, which is at the same time very simple, *and about which there can be no mistake.* A *black clot* is taken and immersed in distilled water; a part of the carbonic acid is then removed by means of an air pump; and finding that the black clot does not become red by removing the acid, it is at once concluded, *per saltem*, that carbonic acid cannot be the cause of the dark colour. It ought to be recollected, however, that, in this instance, the clot was black previous to the experiment—and why? merely because it did not contain a sufficient quantity of salt to strike a red colour with the hæmatosine; and for this obvious reason the mere removal of the carbonic acid cannot produce redness in the clot, particularly when immersed in distilled water. This experiment, therefore, is only a proof of how little its contriver is acquainted with this subject, and so far from "destroying the new theory," it serves to confirm and sustain it. In fact, it was altogether gratuitous; for had the individual who has performed it condescended to read my work, with common attention, he would have found it proved, in more places than one, that the mere removal of carbonic acid cannot produce any thing like redness in a black clot, or in black blood, unless there is a sufficient quantity of saline matter in immediate contact with the

hematosine to strike the red colour with this substance, when the acid is removed: and this surely is a sufficient proof that oxygen alone is not the sole cause of the red or arterial colour.

It has long been ascertained, that blood which is drawn from the veins into an exhausted flask, does not become red. This is probably because the carbonic acid remains in contact with the colouring matter; but the moment that this dark blood is exposed to the air, the acid is rapidly removed from its upper surface by the atmospheric oxygen, and the colour changes in the same way that it does in the lungs. But how is the venous blood to become red in an exhausted flask, if the darkening acid be not removed, or if the gas which rises be still in immediate contact with the colouring matter? yet this experiment is considered as affording proof positive that the salt is not the cause of the redness, nor the carbonic acid of the purple hue.

When we agitate a small quantity of venous blood in a phial of atmospheric air, it becomes beautifully florid; but, when we make the same experiment with a small quantity of arterial blood and a large volume of *pure* carbonic acid, the colour darkens into a venous tinge. This latter experiment was first made by Dr. Priestley, who ascertained that this was the fact. Care, however, must be taken to exclude all communication with the external air. When this condition is observed, and when the blood and the acid are sufficiently agitated together, the colour changes from a florid to a purple red. In fact, as every one of the acids blackens the blood, exactly in proportion to its neutralising power, it would be rather strange if the carbonic, which ranks so high in this respect, were to form an exception to this rule.

It is asserted in the last number of the Dublin Journal, that even a black clot from which the serum had completely drained out, became red again when it was cut open and exposed to the air. This may be the case with Irish blood, and in the atmosphere of the sister kingdom, but when the experiment is fairly made on British blood, in the air of England, it does not exhibit this phenomenon. I have already remarked, that the whole serum is never extruded out of a clot by the force of spontaneous coagulation; but that much still adheres, particularly where the mass is large and the drainage imperfect. To make this experiment fairly, it is necessary that the clot should be *very small*. When such a clot is immersed in distilled water, the saline serum flows out of it very readily into the lighter fluid, and after a short period the clot becomes perfectly black. The Irish experiment, however, was made with a coagulum of bullock's blood, which weighed no less than *four or five pounds*. This huge mass was exposed to the air. The external part of it would necessarily become somewhat indurated in a dry atmosphere, and thus impede the free egress of the serum. This experiment, therefore, so far from giving a death-blow to the new theory, is only another proof in its favour; for in such a large coagulum there is evidently enough of the serum left to redden the blood when the acid is removed by the oxygen of the air. Yet this inaccurate and clumsy experiment is brought forward for the purpose of removing the very key-stone of the arch which has lately been erected, and the engineer is only waiting at a respectful distance to witness the entire destruction of the whole edifice. Certainly he will be disappointed, unless he brings forward a different sort of artillery from that which has hitherto been tried with so little effect.

It has not yet been proved that the addition of oxygen reddens the blood, but from the facts that have been stated, it is very evident that though oxygen may have a direct as well as an indirect share in the change of colour, yet it is equally certain that the salts are the medium through which this change is effected; for oxygen without salt cannot redden the colouring matter, still salts without oxygen immediately give an arterial colour to the blackest blood; and though we cannot redden the black blood in cholera by any other means, yet the healthy colour can easily be restored, by the judicious use of the neutral salts; and the knowledge of this practical fact is certainly of considerable importance to every medical practitioner.

Practical results are the best test of the real value of a medical doctrine: for, "such is the close connexion betwixt theory and practice, that good or bad they invariably keep pace with each other." And pray let me ask, what have we gained in practice by the doctrine that oxygen is the sole cause of

the red colour of the blood? has it lessened the mortality of malignant epidemics?—not in a solitary instance. Can we remove or destroy the poison of cholera, or give redness and healthiness to the black and morbid blood in the last stage of that disease, by the mere inhalation of oxygen gas, or can we redden this black blood by the internal use of those acids which contain oxygen?—most assuredly not: for these agents blacken even the arterial blood out of the body, and there is no want of evidence to prove that this same prepossession about oxygen and acids has led to a more destructive practice than any other ignis-fatuus creed of the medical profession.

It would be idle to follow the writers on the other side, through the labyrinth of error, wherewith they have attempted to mystify the ignorant. Most of their errors are sufficiently obvious to common sense; there is, however, one mistake which may have a fatal influence over the inexperienced mind, and on which, therefore, it may be proper to animadvert: for the inference which a recent reviewer draws from the experiments of Dr. O'Shaughnessy, is fraught with dangers no less imminent, than certain printed instructions for the treatment of cholera which are now doing so much mischief all over the country.

We are informed that Dr. O'Shaughnessy has analysed the blood previous to the attack of cholera, and finds it to contain its usual quantity of water, salts, colouring matter, albumen, and fibrin; and therefore he infers that the blood cannot be diseased either before the attack or in the first stage. For faith in such a conclusion, a much larger body of evidence than has yet been adduced, would be desirable; nor will any one admit, that after the discharges of the premonitory stage, the blood remains unchanged, as in health, or that the quantity of salts which is found in the dejections is not derived from the blood, and must consequently lessen the quantity of its saline matter. Yet, granting for a moment that the experiments to which he refers are correct, is he entitled to affirm that the blood is not morbid; or is it a proof that nothing has been added merely because nothing has been taken away? But it is very clear that the blood may be injured by addition as well as by subtraction; and the mere fact that it contains its usual proportion of chemical compounds is no proof whatever that a poison has not been added, or that this noxious agent has not lessened the vitality of the blood as well as of the solids. When we analyse the air that broods over a pestiferous swamp, we find that it contains its usual proportion of oxygen, nitrogen, and carbonic acid; but are we to conclude from this that the air must be wholesome because it has not lost one particle of its usual chemical constituents; or when we see its effects in producing fever, are we to deny that this air does not contain a poison, merely because the chemist has not yet discovered any test which can enable him to prove its existence? Have we any test for the poison of small-pox?—but those who deny its existence either in the air or the blood, on such reasoning as this, might tell us, with equal propriety, that small-pox is a delusion, or that the aërial poisons are totally incapable of producing disease.

It is admitted by those best acquainted with the subject, that animal chemistry is still very imperfect; and are we to deny the evidence of our own senses in favour of facts, merely because an experimentalist says he cannot confirm them by tangible proofs? In the month of June last, I selected three of the nurses in the House of Correction at Cold-Bath-Fields prison, who had been longest in immediate contact with the sick: they were all of them apparently perfectly well at the time, and attending to their duties. About six ounces of blood were drawn from each, by venesection, and in every one of them the blood was not merely darker in colour, and of an aspect unlike that of health, but it was especially remarkable, that when their blood was exposed to the air it assumed the blue appearance peculiar to the skin in the last stage of the Asiatic cholera, instead of becoming florid. After the blood had been for some time exposed to the atmosphere, the serum became perfectly clear; but a part of the matter which occasioned the blueness was precipitated and adhered to the inner side of the cup. Now, admitting that this blood contained its common proportion of the usual ingredients, no one will contend that it was not diseased, or doubt the evidence of his own senses, merely because there are no chemical tests to prove the existence of the poison, or, as yet, any analytical account of the

agent which forms the blue precipitate.

An extra portion of blood was drawn from one of the above nurses, and a small quantity of carbonate of soda was mixed with this dark blood while it was still warm and fluid; but in place of becoming blue, when exposed to the air it became beautifully florid: and we now know well, that when the alkaline salts are used internally, they act upon the blood and redden its colour in the same way that they effect this purpose out of the body.

It has been supposed by some, that the saline treatment was adopted in malignant diseases on account of the deficiency of the natural saline matter of the blood. This, however, is an error; for the treatment was commenced on practical grounds, before I was aware of this fact; and it was then just as successful in the West Indies as it has been since in other places, wherever this treatment has been fairly tried.

When the blood is healthy, its natural quantity of saline matter is quite sufficient to give it the red colour, to enable it to stimulate the heart, and to perform its healthy functions; but when the vital fluid is under the influence of a narcotic poison, it requires an extra portion of saline ingredients to enable it to resist the effects of any deleterious agents—such as the poison which is the cause of cholera. The salts, however, not only brighten the colour of the black or diseased blood, but they are powerful antiseptics, and some of them, at least, appear to possess the power of neutralising the aerial poisons, or counteracting their agency on the living body; consequently, when given in excess, they increase the secretions and prevent or remove the morbid predisposition, not only in the circulating current, but in the whole system. It is for this reason that their administration is so essentially useful in the first stage of the Asiatic cholera; and when we see that out of fifty patients who are thus treated in the first stage, not more than one sinks into a state of dangerous prostration, and know that similar cases in the same districts, which are treated with opium, stimulants, &c. fall rapidly into a state of mortal collapse; are we then, in the face of such facts, to use opium in place of the saline remedies, even in the commencement, and sacrifice our patients to a vague hypothesis founded on delusive experiments, unhappily pointing to a practice which is well known to be deplorably unsuccessful?

No one who advances doctrines directly opposed to common belief, can reasonably expect that they will fail to excite opposition, if not hostility. Such was the lot of the immortal Harvey, and how can others hope to escape it?—My chief object in coming to England was to lay an outline of my views before the public. This I have partly, but I admit very imperfectly, done in my work "On the Blood." I am aware that it contains more errors than one; but being now on the eve of embarking for another country, I must leave it to its fate. I trust, however, that the intelligent and impartial members of our profession will not look merely to its faults, but consider it as a whole, and then, when the influence of their natural repugnance to innovation shall have passed away, they may form what estimate they will of its merits.

I am, sir,
Your obedient servant,
Wm. Stevens, M.D.

Since the above was written, a letter has appeared in the Times, in which it is asserted that the new theory of respiration is "altogether erroneous." This assertion is made on the authority of an experiment something similar, but evidently as ill conducted, as the one with the black clot. There are circumstances, however, connected with the said experiment which render the conclusion which has been drawn from it of no value. When the atmospheric pressure is removed, the serum of the blood in vacuo does not rise to the surface nearly so fast as it does when a similar quantity of the same fluid is exposed to the air; but when the serum, which contains the reddening agent, does rise to the surface, the blood in vacuo becomes quite as florid as if it had been exposed directly to the air. Sir Charles Scudamore is brought forward as an evidence against the new theory; but he might with equal propriety have been brought forward to prove that the muriate of soda blackens the blood. Mr. Prater is also referred to, as a new and impartial evidence in favour of the old hypothesis; but the writer forgets to state that Mr. Prater is, in the outline, not only favourable to the new theory, but a warm advocate for the practical conclusions which have been drawn from it. He

expresses also his belief that the facts which have lately been ascertained with respect to the changes of the blood in cholera, would never have been *thought of* had it not been for the paper which was read at the College of Physicians in May 1830.

ANALYSES & NOTICES OF BOOKS.

Dublin Journal of Medical and Chemical Science. No. IV. Vol. II.

CONTINUING our notice of the last number of our Dublin contemporary, we have next to allude to

Contributions to Thoracic Pathology, by DR. STOKES.

The paper is an interesting one, intended and calculated to illustrate some obscure pathological points, and shewing, in a very remarkable manner, the advantage in diagnosis derived from the stethoscope. The first case is a striking proof of both those positions: an extensive abscess forms in the right lung; after a time it heals, and the cicatrix happens to insolate a portion of the lung. The patient is afterwards attacked with pleuro-pneumonia, and while all the adjacent pulmonary texture participates in the inflammation, that portion which is separated by the cicatrix escapes, and is ascertained by auscultation to have done so. The details are very interesting:—

"In the spring of 1829, a young man, by trade a blacksmith, was admitted under my care to the fever wards of the Meath Hospital, for an affection of the chest, which had been of a few weeks standing, and ushered in with the usual symptoms of an acute irritation of the lung. On examination he presented the symptoms of bronchitis, but to our surprise we found that the right lung, from the clavicle to the mamma, sounded dull. *Here and over the shoulder, all the signs of an extensive cavity were discoverable by the stethoscope, cavernous respiration, gurgling, and distinct pectoriloquism.* Over the rest of the thorax the phenomena of bronchitis existed.

"The patient suffered much from cough and dyspnœa, but under the usual treatment of bronchitis, these symptoms almost completely subsided. He regained in a short time his looks, flesh, and strength; the pulse became natural, and his appetite excellent, although all the signs of the abscess continued unchanged. In this state he left the hospital, declaring himself perfectly well, and, indeed, to an ordinary observer, there was no symptom whatever that would cause a suspicion of organic disease of the lung.

"After a few weeks, having caught cold, he returned with a slight bronchitis, the signs of the abscess continuing unchanged; and, after some days, again left the hospital, and resumed his occupation of a smith. We then lost sight of him for a twelvemonth, when he was again admitted labouring under severe symptoms of pleuro-pneumony, which had been neglected, and were of five days' standing. It appeared that after his last dismissal, he had enjoyed the most perfect health, although toiling at his laborious occupation, until five days before his admission, when he was seized with pain of the side, cough, dyspnœa, and fever: he continued to work until his sufferings obliged him to desist, and on the following day he entered our wards.

"He then presented all the symptoms of the third stage of acute pleuro-pneumony of the right lung. On percussion, the whole of this side, both anteriorly and posteriorly, sounded completely dull, *except in the sub-clavicular region, where it was comparatively clear.* This, it will be recollected, was the former seat of the abscess. Over the whole dull portion, bronchial respiration, mixed with an intense muco-crepitating râle, was audible; but, on examining the sub-clavicular region, we found to our great surprise, *that all the phenomena of a cavity had disappeared, and were replaced by a puerile respiration.*

"Here was a case full of difficulty. It was plain that the greater portion of the lung was solidified, and had passed into the third stage; but why a small portion of it should have escaped the disease *in toto,* when the rest was so far advanced, and that this portion should be that formerly occupied by an abscess, was indeed difficult of explanation.

"All treatment proved inefficacious, and the patient sunk on the third day after admission, the stethoscopic phenomena having continued unaltered.

"On dissection, we found the right lung solid over the whole extent indi-

cated by the stethoscope. From the fourth rib downwards, the pleura was covered with coagulable lymph, which being removed, allowed us to see the lung through the serous membrane of a yellow colour. In the superior portion of the lung, the adhesions were evidently old, as considerable strength was required for their separation. On the summit and antero-superior surface, the peculiar puckered appearance described by Laennec, as resulting from the cicatrization of an abscess, was evident.

The lung was then divided in a line running along the situation of the angle of the ribs, so as to separate it into two portions, connected only at the root of the lung. This gave us at once an explanation of all the phenomena. The supero-anterior portion, for a space of three square inches, was perfectly crepitating, and not all engorged. This was separated from the rest of the organ by the cicatrix of the abscess, which had been obliterated by the adhesion of its walls, so as to form a cartilaginous septum, superiorly of half an inch in thickness, and easily separable through its whole extent into two layers. This septum ran from behind forwards, commencing at the summit of the lung, and terminating where the large bronchus separates to be distributed to the upper lobe of the lung; its whole length being more than three inches. Thus the subclavicular portion was isolated, and we found that its bronchial communication was preserved by a trunk passing from the larger tube immediately below the termination of the cicatrix. The interlobular septa in this portion were remarkably hypertrophied, but in no other respect did it differ from healthy lung. The remainder of the upper lobe, the middle and inferior lobes, were in the state of grey hepatization. No tubercle was found in any part of the system."

Cure of Phthisis Pulmonalis is the flattering title to the portion of the Essay which follows. Dr. Stokes has been for some time convinced that pulmonary consumption was cured more frequently than he had been taught to believe; and within the last year has seen several cures, "in all of which the most unequivocal indications of tubercle existed," and in which, nevertheless, a cure, more or less complete, was effected. The plan usually adopted was that of establishing a drain, or counter-irritation, in the neighbourhood of the cavity. Exercise, travelling, and attention to the general health; especial caution being observed in the administration of purgatives—a class of medicines which he thinks increases much the risk of ulceration of the digestive tube: "Purgatives must be inhibited, and enemata and the mildest laxatives used when necessary." The diet was "bland and nutritious;" but wine was not allowed in any of the cases which recovered. Another point worthy of attention is, that Dr. Stokes thinks harm is often done by the too free use of astringents, especially sulphuric acid, with a view of checking the colliquative perspirations.

On comparing the cases in which recovery took place, Dr. Stokes says:—"There appear certain points of resemblance between them, the consideration of which throws light on the question of the curability of consumption. None of these patients, with the exception of the first, presented that general appearance called the strumous diathesis. Their hair, eyes, and complexion, were dark; their muscular fibre was originally strong; they presented no marks of external glandular disease; their family was not consumptive; the disease had supervened on an inflammatory attack of the lung; *and in none was there diarrhœa, or other signs of gastro-enteric complication.* I shall again allude to this last circumstance, as I look on it as one of essential importance.

"Although, perhaps, the division may not be found to stand a critical investigation, yet I think we may separate cases of phthisis into two classes, the constitutional, and accidental phthisis. The first, where the tubercular development supervenes, either with, or without some precursory irritation in persons strongly predisposed to it from original confirmation. In these the disease generally runs a rapid course, invades both lungs, and is commonly complicated with gastro-enteric disease. In the second variety, we meet the disease in persons not of the strumous diathesis, and who have no hereditary disposition to the affection. In them the exciting cause is generally an inflammatory attack of the lung. The disease proceeds slowly, is long confined to one lung, the hectic is slight, and

often wanting, and the liability to enteric disease is much less. These are the cases in which it appears to me rational to hope for a cure, and justifiable to adopt decided measures, even after extensive lesions have been formed in the lung. With a single exception, these were the cases in which I have witnessed a cure, either by cicatrization of the abscess, the cretaceous transformation of the tubercle, or by both of these modes together."

There is also much justice in the following remark, which the experience of almost every man must have verified.

"There is another case nearly equally common, where the life of the patient is shortened, and the chance of cure removed, by the same practice. A young female with symptoms of incipient phthisis, ceases to menstruate. To this, which is the effect and not the cause of the disease, all the symptoms are attributed, and the efforts of the practitioner are directed not to remove the irritation of the lung, *but to force the uterine action.* Emmenagogues, which in British practice means cathartics, are lavished, and the consequences are such as might be expected from this unphysiological and barbarous practice. The pulmonary disease advances rapidly; first, because it is neglected; secondly, because the lung is stimulated by the enteritis induced by the remedies; diarrhœa sets in, and the delicate and amiable patient dies of a '*Galloping Consumption.*'"

The absence of strumous diathesis, the slowness of progress, the freedom from constitutional symptoms, and the integrity of the digestive system, are the great sources of a favourable prognosis.—But, alas! how seldom are these combined in a case of genuine phthisis!

Empyema next comes under consideration, on which the only circumstance we have to remark is, that Dr. Stokes regards it as more frequently curable without operation than most practitioners seem to think.—Repeated blistering is what he mainly trusts to.

The paper on *Thoracic* Pathology concludes with two cases of *abdominal* aneurism.—Is our intelligent author desirous of reminding us that he writes for the other side of the Irish Channel?

Some valuable observations are contained in a communication from the pen of Dr. Graves, and entitled, *Observations on the Treatment of various Diseases.* The account of hooping-cough treated by tincture of cantharides is the only portion for which we can make room.

"The treatment of hooping-cough is of vast importance, as the disease, particularly in young and delicate children, frequently proves either directly fatal, or else lays the foundation of other equally unmanageable forms of pulmonary complaints. The observations of Doctor Mackintosh upon the symptoms, pathology, and treatment of this disease, are excellent, and ought to be attentively perused by every practitioner. It appears to me, however, that he has omitted to mention one of the most valuable remedies, tincture of cantharides.

I quite agree with Dr. Mackintosh in the treatment he proposes for the first or catarrhal stage of the disease; the antiphlogistic regimen and treatment, leeches to the larynx, in some cases even general bloodletting, antimonials, and ipecacuanha in nauseating doses, are the remedies which the first stages of hooping-cough almost invariably require; after the second or third week, and in some cases even after the first, the well-marked symptoms of constitutional fever, and local inflammation, generally subside, while the violence of the cough, and the peculiar sound of the hoop, are far from being alleviated. It is at this period that the true nature of the disease is most evident; it is now that it too frequently becomes an *opprobium* to the medical attendant. The patient's friends complain that no remedy can be found to allay the cough, and abate the peculiar sense of suffering which accompanies the paroxysms of hooping, and they think it very singular, that while the health in all other respects seems unimpaired, the original disease goes on unmitigated. Hence it is that they seek for, and not unfrequently apply, empirical remedies. Many authors have mentioned the tincture of cantharides as a medicine which has occasionally proved useful in hooping-cough. Most practitioners, however, are prejudiced against its employment, and prefer remedies which are not so likely to excite anxiety in the mind of the physician, during the period of their exhibition. Such, I confess, were my former feelings upon this subject, and my antipathy to the tincture of cantharides would have probably continued had I not been persuaded by my friend, Doctor Thomas Beattie, to give the medicine a fair trial. He alleged in its favour, his own experience, and that of his father, who had for many years employed tincture of cantharides in the following formula:—

℞ Tincturæ Cinchonæ Compositæ, ℥v.
——— Cantharidis.
——— Opii Camphorat, aa. ℥ss.
ꝗ. Ft. Mistura.

One drachm of this may be taken in linseed tea, or barley-water, three times a-day, and in persons above five or six years of age, the dose may be daily increased one-third, until half an ounce is taken three times in the day. Much smaller doses, however, than that last mentioned, will frequently be sufficient to check the violence of the disorder, and, of course, when this happens, the dose need not be augmented. Thus, in the case of a grown-up woman, named Mahon, admitted into the Meath Hospital on the 17th of May last, the maximum dose was a drachm given five times a-day. This case was treated under my direction, by Mr. James Costello, and was of three weeks' standing when admitted. I treated, along with Mr. Nicholl, a young lady aged nine, who was seized with the first symptoms of the disease on the 13th of May last. On the 6th of June she commenced taking the medicine at the rate of a drachm three times a-day; on the 8th the diminution in the hooping was evident, and on the 13th she did not hoop more than twice a-day. There was no occasion to augment the dose of the medicine in this case.

If necessary, I could relate many other cases in which this medicine produced a most decidedly good effect; but at present I shall content myself with observing that the tincture of cantharides, exhibited in the formula recommended by Doctor Beatty, has the peculiar method of producing its good effects without giving rise to urinary irritation, at least in a very great number of cases no such irritation takes place. Doctor Beatty informs me that he has been equally successful in curing the disease in infants by means of this remedy exhibited in appropriate doses; such cases are seldom placed under my care, and therefore I cannot speak from my own experience on the subject. What influence is exerted by the cochineal and other ingredients contained in the compound tincture of bark, and camphorated tincture of opium, I am unable to determine. A very important observation is made by Dr. Mackintosh: "Change of air is extolled by some individuals, but is often productive of great mischief, by occasioning a return of the disease It is an important fact, that during the late epidemic, which was the most severe I have ever witnessed, all the children that were moved for change of air, had the disease the longest." Now it is very remarkable that the late Dr. Beatty made it an invariable rule to keep his patients confined to their bed-room until the cure was completed, and he used to lay great stress upon an accurate observance of his directions with regard to this point. I have reason to believe that in the great majority of cases the recovery of the patient is much accelerated by this means. This fact alone is sufficient to demonstrate that hooping-cough is owing to something very different from mere inflammation, for after the first violence of catarrh, laryngeal inflammation, or bronchitis, has subsided, change of air often acts like a charm in removing the disease.

Since I have adopted the remedy above-mentioned, I have laid aside the tartar emetic ointment altogether; indeed I never derived any evident or permanent advantage from its application in hooping-cough, either over the stomach or the spine."

MEDICAL GAZETTE.

Saturday, September 22, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

CLOSE OF THE VOLUME—A RETROSPECTION.

THE present number brings to a close another volume of the Gazette — *the Tenth,* — and thus forms a resting-place whence we may be allowed to look back, for a moment, on our past career, and to glance at our future prospects. The past speaks for itself: our journal, with whatever good or evil it may contain, is before the public, and though we cannot suppose that they will take our estimate of its merits instead of judging for themselves, yet to some points connected with its history it may not be impertinent, nor altogether uninteresting, to advert. In judging of the manner in which any undertaking has been executed, it is necessary to keep in mind the circumstances under which it was begun, and the ends which it had in view; nor, perhaps, ought even the motives of its conductors to be entirely lost sight of. To most of our readers the early history of the Gazette is sufficiently well known; but the recent accessions to our subscribers have been so considerable, that we may fairly presume many who now do us the honour of perusing our pages, are unaware of the

occasion which first called forth our humble efforts as medical journalists.

At the time to which we allude, a tyranny of a new and intolerable kind had been set up over practitioners generally, but particularly over those in the metropolis—such as we believe had no parallel in the records of any other profession. This state of things, which was rapidly bearing down the respectability of our calling, and degrading medical science by holding up its cultivators to the public as men destitute alike of talent, honour, or humanity, cannot perhaps be better described than in the words of a late eloquent writer, to whose pen we were frequently indebted: after alluding to the once peaceful tenor of the medical career, he continues,—and the remarks on the subject of lectures especially have recently found a most conspicuous illustration,—

"But a few years ago a set of literary plunderers broke in on the peace and quiet of our profession. Lecturers who had spent their lives in collecting knowledge—arranging it for communication—and acquiring the difficult art of oral instruction, saw the produce of their lives suddenly snatched from them, and published for the profit of others, with the additional mortification of finding what they had taken so much pains with, disfigured by bad English and ridiculous or mischievous blunders. Whoever attempted to arrest these piracies became the object of furious and unrelenting abuse. Hospital physicians and surgeons, who have to prescribe and operate in public, and at stated times, in whatever condition of bodily health or mental feeling they may happen to be, and exercising in the face of critics (not always competent to decide on their merits)—a science so avowedly imperfect as to afford abundant scope for uncandid and ill-natured remarks, however judiciously practised,—were held up to public scorn for errors to which, even if actually committed, the ablest men are occasionally liable, while those who leagued in secret with their calumniators, and who, with one or two exceptions, were as insignificant in station and talents as they were equivocal in character, were represented as at the summit of science and professional eminence.

"We do not deny that public exposure may have, in some few instances, done good; it may have abolished some foolish custom, or led to the reformation of some trifling abuse; but weigh the evil against the good—it has deprived eminent men of their intellectual property, and destroyed the mutual confidence between pupils and their teachers; it has lowered the respectability of the profession, and has spread general distrust; it has broken up private friendships; it has placed man in hostility to man; and has set so many bad passions into ferment, that well-disposed men become disgusted with the state of their profession, and vow that they never will inflict it on their sons."

Such was the state of our profession at the time this journal was projected, and such the nature of the evils it was intended to remove—or at least to mitigate. So serious, however, were the difficulties which presented themselves—so many had been the previous failures in the same attempt—such was the dread of encountering that remorseless persecution with which *they* were sure to be visited, who dared the wrath of a paper which had always waged war to the knife, against whoever presumed to oppose it—such, in short, from various considerations, was the reluctance to the undertaking, that both the publisher and the editor, with whom the plan originated, abandoned it as hopeless. A different view, however, was taken by the spirited House which afterwards ushered this journal into the world, and the result

has shewn the superiority of their discernment. For ourselves we always confidently anticipated the success which has followed, because we looked upon it as a calumny on our profession to suppose that there existed not in it a sufficient number of men willing and able to secure the triumph of a journal conducted on principles of integrity and good faith, if any degree of talent were displayed by its contributors, and any tolerable diligence exhibited in its management. The praise of talent we willingly bestow where it is justly due; namely, on those numerous and eminent members of the profession who originally became, and have subsequently continued, our steady and valued contributors; the merit of industry, —and it is all we claim—will not we trust be denied to ourselves. Fearful of becoming egotistical, we shall allude to ourselves no farther than this:—the only difficulty which we have experienced in regard to our editorial conduct has been how far we should restrict our pages to mere details on points of science, and how far grapple with our opponents, and practise retaliation, by speaking of them, personally, so as to make them individually experience the smart of those weapons they were wont to make use of against others. We could only do this, it is true, on unequal terms; for while the whole profession was open to one side for malicious caricature or malignant detraction—to us, only those concerned in conducting this atrocious system were presented as objects of attack; and even here, unlike our adversaries, we had of course to confine ourselves to what was true. The subjects to be depicted, as we have hinted, were few, but fortunately they were of a very striking description—they only required to be faithfully displayed—neither caricature nor detraction were required in the exposure, nor could they have added one iota to the effect of exhibiting the parties *in puris naturalibus*, unmasked, and stripped of all those disguises with which the press enables artful men to cloak the loathsome nakedness of their secret motives. To no part of our labours do we look back without pleasure except to those devoted to such delineations, for although they were defensive and in exposure of the unworthy, we did and do feel, that the necessity which occasioned them was one derogatory to science—but the shame rest with those in whose violation of all those decencies which had previously been held sacred, that necessity originated.

No one who has paid attention to the events in our profession during the last few years, can fail to have perceived, that a great change has taken place in the state of general feeling—there exists not now that prurient appetency for slander to which the novelty of the system which we deprecate had gradually led. How far the change to which we refer has been effected by any labours of ours, is a matter of little moment; though it is not unreasonable to suppose that the contradiction of falsehoods, almost as soon as penned, had some influence in rendering the manufacture less agreeable and less profitable; at all events, the article was soon found less marketable, and a smaller supply was consequently provided. In proportion as this turn in the public taste continued to advance, in the same ratio did the competition between the journals become more equal, because more confined to the same kind of supplies;—and it is in the highest degree gratifying to us to be able to refer to the past year as having produced a remarkable increase in the number of our correspondents—as it also has a great augmentation of our subscribers. This statement, with regard to the state of the concern, considered in a mercantile point of view, is one which those acquainted with our editorial character

(to which at the end of ten volumes we may, perhaps, be permitted to appeal) will of course believe on our simple asseveration; albeit, we perceive that just the reverse is gravely declared by a contemporary, who either pertly prates of what he knows nothing about, or else asserts that which he knows to be untrue. Again, then, we refer with gratification to the original communications which have appeared in this journal during the past year; comprising, in the number and eminence of the contributors, such a list as no medical periodical published in this country has ever before had to boast of; and we would add, that by "original" papers *we* mean those really furnished by the reputed authors—an explanation which we feel called upon to make in contradistinction to a method recently adopted by some others, of advertising as contributors to their work distinguished men, who have not written one line of what is given in their names, and some of whom have actually adopted legal proceedings in consequence of this disgraceful piece of knavery.

We have said that we should take a retrospective view of the past, and a glance at the future. The former we have in some measure done—but as to the latter, it is an unopened book, and we may not speak of its contents so positively. No considerable change is contemplated in our general plan, for we are chary of altering that under which we have flourished. The departments in which so much important matter has already been accumulated, will be continued; and we hope with undiminished interest. The valuable Lectures which will constitute a complete *System of Medicine*, by Dr. Elliotson, will be resumed in our next number; and we shall give, in the ensuing volumes, some short *Courses*—particularly one on the *Diseases of the Eye*, by Mr. Middlemore, of Birmingham; which, from the interest of the subject, the extensive opportunities of the writer, and the manner in which the essays are executed, we doubt not will be acceptable to our readers. We shall also, from time to time, adopt such suggestions of our correspondents as we find practicable, and calculated to be of interest;—to such course, for instance, we owe the introduction of the *Weekly Meteorological Reports* and *Bills of Mortality;* which, though they occupy but a niche, present an interesting record connected with the science of public health.

As to our *principles*, we have no professions to make—but take leave to refer to the past: if the articles which have recently appeared on the three medical corporations be not admitted as evidences of our entertaining liberal opinions, and being ready to express them with freedom, no declarations of ours could be of any value;—besides, we have always observed that those who are loudest in their protestations of independence, are, in reality, the most subservient to what they think is the most popular side of the question: with freedom on their lips, they are the very slaves of party.

We have only further to add, that, desirous of making our external appearance keep pace with our internal improvements, a new fount of types has been procured expressly for this journal, and that the present is the last appearance we shall make in our old suit.

Here, then, we close our volume, and, with respect to that which is to come—if we may borrow a phrase, which the allusion to the arrangements of our printer has suggested, and thus *typify* the subject—we would express our earnest hope, that the *impression*, which each reader receives, may be a good one.

SUPPLY OF THE SCHOOLS OF ANATOMY.

THE principal anatomical teachers of the metropolis have had a meeting this week, at the Freemasons' Tavern, to arrange matters of common concernment for the ensuing campaign: Mr. Stanley in the chair. Resolutions were passed expressive of abhorrence of the old system, and determination to discountenance altogether the disgusting race of exhumators. It was then agreed that a deputation should wait on the Home Secretary, to obtain from him, if possible, assurances that no favour or partiality should be observed towards any person or persons in particular; but that an equitable distribution of subjects should be secured, in proportion to the wants of each school: and for this purpose a subscription was entered into. We think there cannot be a doubt that it is the bounden duty of every teacher of anatomy, at the present moment, to come forward and make common cause with his brethren, and to see that no private or underhand trafficking be pursued: it is the only way in which all parties can be permanently benefitted, and placed in a reputable and proper light before the public. Why, then, may we ask, were not the gentlemen of Guy's and St. Thomas' present?

EFFECTS OF DARKNESS IN PRODUCING DEFORMITIES.

A CORRESPONDENT writes us the following curious fact. There is at present in Paris an artist of the Louvre, an eminent historical painter, of the name of Ducornet, who paints with his feet. He was born without arms, of poor parents, at Lille. There are also about the French metropolis a number of beggars, twelve or thirteen of them at least, all deformed in various ways, and all born at Lille, in certain dark caverns under the fortifications. The effect of these places, from their want of light producing malformed births, is so notorious, that the magistrates of Lille have issued strict orders to prohibit the poor from taking up their abode in them. It is added by our correspondent, that he had a conversation with Mr. Edwards on the subject, and that gentleman was greatly struck with the confirmation which the above circumstances afford to his views, stated in his work, *Sur l'influence des agens physiques sur la vie.* Mr. Edwards's experiments of detaining tadpoles in darkness, and thus causing them to grow into gigantic and monstrous *tadpoles,* instead of being transformed into frogs, are well known.

WEEKLY ACCOUNT OF BURIALS,

From the BILLS OF MORTALITY, *Sept.* 25, 1832.

Abscess	1	Heart, Diseases of	1
Age and Debility	45	Hooping-Cough	6
Apoplexy	4	Inflammation	31
Asthma	8	Inflammation of the Bowels & Stomach	10
Childbirth	7	Lungs and Pleura	3
Cholera	39	Insanity	7
Consumption	75	Liver, Diseases of the	4
Constipation of the Bowels	3	Measles	5
Convulsions	30	Miscarriage	1
Croup	2	Mortification	7
Dentition or Teething	8	Paralysis	2
Dropsy	22	Small-Pox	17
Dropsy on the Brain	9	Sore Throat and Quinsey	2
Dysentery	2	Thrush	1
Erysipelas	1	Tumour	1
Fever	13	Unknown causes	79
Fever, Scarlet	5		
Fever, Typhus	4		
Gout	3	Stillborn	11
Hæmorrhage	2		

Decrease of Burials, as compared with the preceding Week . . . 187

METEOROLOGICAL JOURNAL.

September 1832.	THERMOMETER.		BAROMETER.	
Thursday . 20	from 30	to 61	30·42	to 30·44
Friday. . . 21	38	65	30·46	30·44
Saturday . 22	39	64	30·42	30·31
Sunday . . 23	40	67	30·29	30·24
Monday . . 24	38	73	30·30	30·33
Tuesday . 25	40	71	30·35	30·30
Wednesday 26	39	71	30·24	30·16

Wind variable, S.W. and N.W. prevailing.
Except the 22d, clear.
The unusually fine, and very warm weather of the past week, is worthy of particular remark.

CHARLES HENRY ADAMS.

LIST OF LECTURERS.

WE cannot comply with the request of a Pupil, by devoting any part of the body of the journal to the announcement of lectures: we refer him to the advertisements on the wrapper.

NOTICE.

The case of Schirrus of the Intestine did not come to hand: we shall be happy to insert it if our correspondent can furnish us with another copy.

BOOKS RECEIVED FOR REVIEW.

Mr. Corbyn on the Epidemic Cholera, as it has prevailed in India.

Mr. Sheldrake's Animal-Mechanics applied to the Cure of Spinal Curvature and other Deformities. Part I.

Dr. Keir on Cholera, as it prevailed in Moscow.

Mr. Percivall's Anatomy of the Horse, embracing the structure of the Foot.

ERRATUM.

In Mr. Morrah's paper, page 801, line 15, *for* "a few doses," *read* "a full dose."

W. WILSON, Printer, 57, Skinner-Street, London.

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(APRIL 7, 1832, TO SEPTEMBER 29, 1832.)

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END OF VOL. X.

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